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AN INTERFERENCE THEORY OF COLOR VISION

By W. T. M. FORBES, Cornell University

At present two principal theories of color vision hold th field. The so-called three-color theory, with which the name of Young and Helmholtz[1] are associated, appeals specially t the physicists, but the psychological difficulties seem hopeless especially in the interpretation of violet and brown. It is interesting that some of the physicists who have worked on i most professionally confuse blue and violet so persistently i their writings as to raise the suspicion that they may be viole blind.[2] The alternative theory is usually associated with th name of Hering,[3] but his formulation appears needlessly complex to the physicists and seems to assume physiologica processes for which we have no precedent. Ladd-Franklin has developed a scheme which avoids most of the difficulties o the other two, but which requires that each retinal cell shal send to the brain at least three separate types of stimulation As there is only a single fibre to each cone or rod, these thre (or more) excitations would have to be carried in a single fibre which would be unprecedented, as in all the cases yet examine a single fibre appears able to produce only a single kind o reaction, with variation in frequency alone.[5]

As is well known, it is possible to make an array of all conceivable color in a figure of three dimensions (double cone or prism), and not less—so i the interest of simplicity it seems best to assume that there are also thre processes involved in the transmission of each colored stimulus to the brai each one variable only quantitatively. Both the familiar *three-color* an *four-color* theories in fact have this property, the three independen variables in the four-color scheme being represented by red-green, blu

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*Accepted for publication January 16, 1927.

[1] H. von Helmholtz, *Treatise on Physiological Optics*, American ed., II, 141 ff.

[2] In the sense merely of inability to distinguish violet from blue as hues.

[3] Summarized with full references in J. H. Parsons, *An Introduction to the Study* *Colour Vision*, 251-263.

[4] Helmholtz, *op. cit.*, II, 455-468; Parsons, *op. cit.*, 271-274.

[5] E. D. Adrian, *J. Physiol.*, 61, 1926, 65, 465; 62, 1926, 33.

yellow and white-gray-black; in the three-color by white-green-black, white-red-black and white-blue-black, or a similar trio. It has likewise been noted that the three variables: dominant wave-length, intensity, and saturation, would also serve, but no color-theory is built on this trio, and it is difficult to imagine an end-organ for the detection of saturation independent of hue or intensity. This method of analysis, however, has proved very useful in the practical specification of color.

To prevent the difficulty of having a single nerve carry more than one process, it will be necessary to assume three fibres involved in each colored sensation, and therefore three cones (or rods).

In the hypothesis here presented I endeavor, using three kinds of cones, to avoid the difficulties of each of these theories, without assuming the existence of substances, structures, or processes which would be unprecedented in living tissues. I do not consider anything that happens behind the retina, and so will not fully treat the phenomena of fatigue and contrast, or many others which seems to involve the function of ganglia as well as the visual cells. I also will give little attention to the so-called rod-vision. In its typical form it is wholly uncolored, and except in the matter of certain after-images it can be viewed as merely a form of white-vision associated with an extreme degree of light-sensitiveness. It is very questionable if the current accounts are correct; in fact there is no clear evidence that the so-called rod-vision and cone-vision are in fact limited to the rods and cones. The whole matter should be worked over from the point of view of the gradually varying structures of different zones of the retina. At present the clear accounts of retinal structure apply mainly or wholly to the peripheral parts, while the physiological data which have been applied to them are mainly or wholly derived from central and paracentral regions of the retina with a far different structure. Incidentally the end-organs in the fovea are not slightly modified cones, as most text-books state, but approximate much more closely in appearance and, according to Cajal, in their nerve-connections also, to the rods. In the remainder of this paper I shall use the term 'cones' indifferently to indicate visual end-organs, with the proviso that they may in fact turn out to be in part the rods in the peripheral retina, and certainly in the central retina include the various modified and more or less rod-like structures that exist there. I have a private suspicion that in the peripheral retina, where true cones exist, they are the color-organs only, and that the rods provide the white element at photopic as well as scotopic levels, there being two pigments rather than two structures involved.

The hypothesis presented in this paper requires:

(1) *A structure of dimensions of the order of the half wave-length of light,* and of moderate uniformity. A familiar example of such a structure is the thin reflecting film which produces the iridescence colors of birds and

butterflies. Certain structures in the muscle-striations are examples
soft tissues of comparable fineness and sufficient uniformity.

(2) *A light-sensitive substance,* with its color-sensitivity proportio
to the known sensitivity of the retina. The familiar visual purple i
substance of the proper type, but both from its absorption curve, and
easy bleaching with moderate amounts of light, it has been without questi
associated with the sensitiveness to light of low intensity. For photo
vision one would have to postulate a similar pigment with its maxim
absorption slightly nearer the red end of the spectrum. As such a substa
would be bluer than the visual purple, I shall refer to it as a visual viol
The ordinary method of preparing the visual purple under red light wo
not reveal it, as it would be bleached by red light,[6] and in any case it ne
be present only in such minute amount as possibly to be invisible, like t
dyes in an ortho-chromatic plate; but there has actually been revea
some slight evidence of a visual violet, momentarily laid bare by ra
bleaching of a dark-adapted eye.

(3) *A fine membrane* which shall either be an insulator of moder
efficiency or semipermeable. The ordinary cell and nuclear membra
are structures of this character. It is possible that this structure could
dispensed with, the nerve-excitation being caused by a gradient wit
the cell which would produce an electric or chemical polarity, but t
statement of the theory is simplified by assuming a membrane, and it
by no means an improbable structure.

(4) *Three kinds of 'cones'* in the color-sensitive parts of the reti
There should be differences of structure which might *possibly* be visi
but which would be at the limit of visibility.

(5) *Post-retinal structures* capable of explaining the effects of contr
and of the reflex part of adaptation, and perhaps of the black sensation
far as it seems even darker than the idioretinal level. The 'granul
layers of the retina and more central ganglia provide such structures
abundance.

(6) *A restoring mechanism* tending to bring the cell back to a resti
state. The imperfect insulation provided by any organic structure wo
discharge any electrical accumulation. A restoration of the light-sensit
substance is needed for any theory, and is well represented by the w
known recovery of the visual purple (in the case of photopic vision
presented by the postulated visual violet) when unstimulated.

(7) *Explanation of after-images.* The two restoring processes j
mentioned if somewhat sluggish (as they undoubtedly are) would expl
positive and negative after-images, respectively. Some slight analysi
made of this phase in the appendix. There is also perhaps some slight c
tribution by central processes of persistence and of fatigue.

THE INTERFERENCE THEORY

(A) *The receptive substance.* For this I assume that the
exists a pair of visual pigments, one the familiar 'visual purpl
of scotopic vision, the other a closely similar substance with

---

[6] E. Köttgen and G. Abelsdorff, *Zsch. f. Psych. Phys. d. Sinn.* 12, 1896, 161; A. Kö
(with Köttgen), *Sits. k. Pr. Akad. d. Wiss. zu Berlin,* 1894, 577 (collected papers, 3
König and Kühne worked with orange sodium light, Köttgen and Abelsdorff with deep
light. Their respective curves tend to show irregularities at about 580 and 660 mμ
spectively, suggesting both that the light may not have been without selective influe
and that there may be more than one photopic pigment involved.

[7] This term is in fact a misnomer, the visual pigment of scotopic vision being in fa
rose and not a purple, but it is too late to make the change now, the term 'visual pur
being so fully established in the literature. Similarly the other pigment which I assu
should really be called a purple, as it would transmit both ends of the spectrum more
less equally; but the *relation* in absorption will be sufficiently indicated by the term 'vis

curve of sensitiveness corresponding to the apparent brightness of the spectral colors to the light-adapted eye. This second substance would have a maximum of absorption in the yellow rather than the green, and I will call it therefore the 'visual violet.' It should exist in the eye only in a minute and probably invisible amount, in fact in about the same proportion to the visual purple as the ratio of maximum photopic and scotopic sensitivities—some such matter as 1:1000.

It is usually assumed that these two pigments are associated with distinct end-organs. There seems to be no necessity of this,[8] and the tendency of colored stimuli to change their hue near the threshold of color-sensivity seems to indicate that the color-cones also possess the scotopic as well as the photopic pigment, though in far less quantity than the more peripheral of the white-organs (rods). Only in the fovea is there no definite evidence of two pigments.

As a tentative suggestion I will assume that the visual pigment when acted on by light is oxidized, producing acid[9] accumulation at the point stimulated. If this is the portion of the cell to which the nerve is attached, the activity of the nerve is increased and a sensation produced. This acid will gradually dissipate, but it is a physical substance independent of the light-stimulus itself, and until it is discharged it will continue to stimulate the nerve, thereby causing a positive after-image of gradually decreasing intensity.

I assume that the visual pigment exists in a thin layer (at least in the color-cones) near the tips of the cones.

I assume further that the oxidized visual pigment is not itself sensitive to light, with the result that until it is again reduced by some external substance (not inconceivably the sugar of the lymph) there is a decrease of light-sensitivity of the cone or layer of the cone affected, and if this does not take place till after the 'acid' is dissipated and the nerve no longer stimulated, there will result a period of negative after-image.

The existence of the idioretinal light would indicate that there is some spontaneous decomposition even without the presence of light. To explain the existence of negative after-images in complete darkness it would have to be assumed that the oxidized pigment is incapable of this decomposition as well as that due to light. This is to be expected, as chemically the two processes are no doubt alike.

It will be seen that the chemical mechanism postulated is that usually assumed for this as well as many other physiological processes, the oxidized pigment being a 'precursor' and the reducing substance being the activator. The visual pigment should not be viewed as being destroyed, but as alternating between a light-sensitive and a chemically active state.

(B) *Receptive structures.* It will be necessary to assume three types of cones (see Fig. 1).

(1) *White cones* (perhaps rods in the outer part of the retina). The white cones do not need to have any particular structure nor location of the visual pigment. It will be assumed

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[8] This point is discussed more fully on p. 21. Cf. Fig. 5.

[9] It is known that the retina becomes acid on stimulation. For reference see Parsons, *op. cit.*, 12.

that the whole pigmented portion of the cone (in the rod the whole rod) is in touch with the nerve-ending and serves to stimulate it, and that there is either no inhibitory effect as suggested below for the color cones, or else that it acts only between neighboring cones (contrast). The automatic activity of these cones is assumed to be slight and to cause a dark gray idioretinal light—the most inconspicuous color imaginable. The more positive sensation of *black* is assumed to be the result of a central process—inhibition by the activity of neighboring neurones[10] or less probably to a reflex cutting off of the restoring substance. The waves of relative blackness and brightness which pass over the retina in the dark at certain levels of dark-adaptation are more probably due to (vasomotor) waves alternately cutting off and supplying extra amounts of restoring substance.

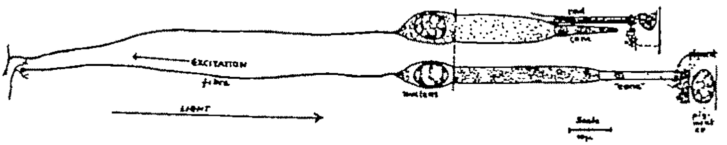

FIG. 1. Above: typical cone cell from peripheral part of retina, and apical portion of a rod. Below: typical foveal organ, intermediate between rod and cone. The figure is semi-diagrammatic, mainly based on the retina of a monkey in the collection of the Cornell University Medical College.

Activity of these cones will by itself produce the series of grays and white, and the range of sensitivity must be assumed to be greater than in the case of the color cones, explaining the persistence of the white sensation when the light is too faint to stimulate the sense of color; and also continuing to increase with increase of intensity long after the color cones have reached their maximum, and in the end wholly covering and obliterating the colored sensations.

The first of these effects would naturally result from the existence of a greater quantity of visual pigment just as well as from a greater sensitivity; the second, I assume, would be a result of loss of the color-sense by effectively complete bleaching of the pigment in the color organs while enough of the larger amount still survives in the white-organs to function.

(2) *Yellow cones* (yellow-blue). In the case of the color cones I assume the existence of a characteristic structure. The apex of a cone is turned away from the source of light (see Figs. 1 and 2) and ends bluntly, being immersed in a probably less refractive lymph or undifferentiated substance (the cone being highly refractive). This will produce a certain amount of reflection of light, which will interfere with the arriving wave and

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[10] There is evidence from after-images that this inhibition is exercised on color as well as white sensations.

produce a series of interference maxima and minima in the substance of the cone. I assume that the visual pigment exists in a thin layer at the apex of the cone, so thin that only the first (or in the case of violet light the second also) of the maxima falls within it.

At each of these maxima there will be a maximum of photochemical activity, exactly as in the case of the Lippmann process of photography, where there is a similar reflection, and silver plates are laid down at the interference maxima. In the case of homogeneous colored light these lines of maximum activity will be sharp, while in the case of ordinary mixed lights, there will be a band of activity corresponding to the whole assortment of wave-lengths present; but in any colored light there will be a maximum corresponding to the dominant wave-length. Now we can imagine the existence of a semipermeable film, similar to a nuclear membrane in character, parallel to the reflecting end of the cone and at a distance from it equal to half the wave-length of the primary green light (say 500 mμ).[11] In this case yellow light would produce a maximum which would lie above the film, that is on the side of it which was connected with the cell-body and its efferent nerve, while blue light would similarly cause activity on the further side of it (toward the choroid), and would be insulated from the cell-

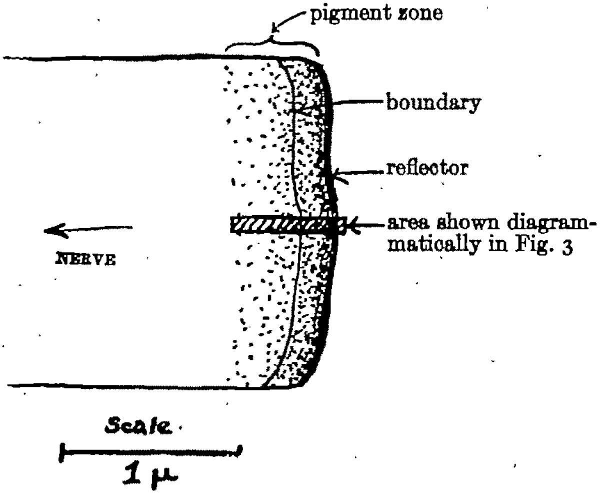

FIG. 2. Tip of color-cone, showing the structures postulated.

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[11] There is a tendency, perhaps more prevalent among psychologists than physicists, to use the symbol $\mu\mu$ to represent the micromillimeter (.000,000,001 M.). As m everywhere in the metric system stands for one thousandth, and μ for one millionth, mμ is the normal combination for a thousand-millionth. Incidentally, with the recent work on radioactivity, $\mu\mu$ is coming into use in its proper meaning of one millionth of a millionth (.000,000,000,001 M)

body. There should also, in equilibrium, be equal quantities of the visual pigment above and below this film or boundary.

(3) *Red cones* (red-green). The red cones may be considered as of exactly the same structure as the yellow ones, save that the distance from the back surface of the cone to the film is a little greater, corresponding to half the wave-length of yellow light (say 575 mμ), and the visual pigment is correspondingly deeper, so that it is still halved by the film (in quantity) and some of it reaches so far from the reflecting surface that in the case of the shorter waves (blue and violet) the second interference-maximum as well as the first will fall within the band of visual pigment, the two bands falling on opposite sides of the boundary film.

The result, then, in the case of the short waves will be to produce the same effect as the longest (that is the red sensation). In the blue these effects will be assumed to be equal in amount, and neutralize each other, while in the violet the second maximum will fall in a region of greater sensitiveness than the first, so that the net effect will be to excite the red rather than the green sensation.

It is to be noted that on the short-wave side of the boundary in each cone there is a narrower sensitive band than on the long-wave side. We assume, therefore, considering that white light produces no net effect in the color cones, that there is a higher *concentration* of pigment on the short-wave side (toward the choroid) gradually fading out toward the cell-body. This is entirely probable, as the phenomena when the retina is separated from the choroid indicate that the restoring substance for the visual pigment is supplied from that side. Not only does a retina separated from its choroid fail to recover its visual purple,[12] but it is interesting that the yellow-blue organs whose sensitive layers would, according to this theory, be both thinner and nearer the back of the retina, are in fact the first to be detroyed in cases of separation of the retina.[13]

This also explains a curious, and I believe hitherto unexplained fact, viz., the effectiveness of light of different colors in adding to the intensity of white light, for instance as measured in a flicker photometer, does not correspond at all to its intensity as measured by its power to neutralize a complementary light. As we go progressively toward the violet end of the spectrum the light rapidly becomes more effective in its neutralizing (color) power. This would naturally follow from such a gradient of visual pigment as the other evidence suggests, for the white value would correspond to the *chemical* effectiveness of the light in acting on the visual pigment in the white cones, while in the color cones its effectiveness would depend on the product of its chemical effectiveness with the concentration of pigment at the level where the band of activity lies. As the pigment becomes more concentrated toward the reflecting surface (where the shorter waves fall), the effectiveness of the light (for color only) would increase in proportion. The second maximum at the red end of the red-green cones would also be doubled in color-effectiveness, because of the spreading out of the second order spectrum, which will be twice as long as the first.

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[12] Parsons, *op. cit.*, 14.

[13] Parsons, *op. cit.*, 180; A. König, *Sitz. k. Pr. Akad. d. Wiss. zu Berlin*, 1897, 720. Note especially König's report on observers 7, 8, and 9.

If for any reason the band of visual pigment in the red cones were less deep than usual, the result would be that the observer would not see a red component in violet light at all, but would tend to confuse violet and blue, which would differ in saturation only, if at all. I believe I have come across one such person (DBJ), who, so far as we know, is otherwise normal. He chooses as his best blue a point in the spectrum about 450 mμ, where the normal person chooses 475 or 480 mμ, and sees no suggestion of redness at the violet end of the spectrum. He is also of somewhat subnormal sensitiveness at the red end of the spectrum, apparently seeing the extreme reds a little darker than the normal person. This indicates that not merely the portion of the visual pigment corresponding to the second maximum of the violet rays (about $2 \times 450 = 900$ mμ) is absent or so weak as to be wholly dominated by the green-activity of the first maximum, but that there is also an abnormally small amount as far down as 700 mμ or a little farther. This would be practically the distribution which would be normal for the yellow cones, and it is a plausible suggestion that in this case one of the differentiating factors between the two types of cones has been lost. There is evidence that in the case of the deuteranope the other differential only is lost, the light-sensitiveness corresponding to the normal range for the red cones, though there is no distinctive red-sense. How common is this type of defect? A good many cases in the literature could be cited where 450 mμ is chosen as a representive of blue, and even where the same writer uses the terms blue and violet as equivalent. Perhaps some of these observers are also violet-blind, though in some it may be merely a careless use of color words. Still I think the normal observer would be far more likely to confuse violet with purple than with blue.

The substantial equality of the blue-yellow and the red-green sensations suggests that the two kinds of organs are probably present in the retina in equal numbers;[14] the white cones are no doubt present in larger (perhaps much larger) numbers. It would be interesting to determine if any such pair-wise arrangement of the cones could be detected in the actual structure of the retina. The retina is made up, as usually described, of little circular groups of cones and rods (a ring of rods about a central cone in the peripheral retina, much like the so-called retinulae of the insect eye). It would be worth while to hunt for a zone in which the cones were arranged in pairs.

(C) *Process.* Light falling on any point in a cone causes activity (and exhaustion) of visual pigment in proportion to its intensity at that point, and to its sensitivity to the wave-length concerned.

In the white cone all activity causes a like effect and a sensation in proportion merely to the amount of light of each wave-length × the sensitivity (integrated over the various wave-lengths present) × the amount of visual pigment in an active state at that moment.

In a colored cone we may divide the light into two portions (see Fig. 3). First, there will be the light that passes through the cone and out, without reflection, and therefore without producing maxima and minima. This will cause an equal effect on all parts of the cone exposed to it, and on the assumption that the amount of pigment is equal on the two sides of the boundary film, will leave the cone as a whole in a balanced and inactive condition. White light also will not produce any bands, and will therefore produce an equal action on both sides and no net result, except an exhaustion of the visual pigment and thereby a weakening of the color process.

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[14] The evidence for equality of the two processes is not too tangible, and should perhaps not be emphasized. It is mainly that colors which may be considered equally saturated make about an equally strong impression on the consciousness, allowing for the interference of the white sensation in the middle of the spectrum, and for the processes of inhibition (blue and green) being weaker than those of direct stimulation (red and yellow).

If the light is colored, however, there will be a more or less definite maximum produced by the part of the light which is reflected, at a level corresponding to its dominant wavelength. This will cause more active material to accumulate at this level, and produce a chemically polarized condition of the cone. I assume (for the purpose of a formulation) that the active layer on one side of the membrane and the inactive layer on the opposite side act as a sort of battery and cause an electrical accumulation on the side which is active.[15] This, if it is on the side of the boundary to which the nerve is attached stimulates the nerve and causes a discharge more rapid than the spontaneous rate. If it is on the opposite side, the current will be away from the part attached to the nerve, and will cause a corresponding degree of inhibition.[16]

If the active band lies astride the boundary, the two sides will work against each other and the effect will be weakened, till in the case where it is evenly astride there will be no net activity and no sensation.

I assume that the cone when in an unstimulated state has a characteristic rate of discharge, which like many other nerve-conditions of tonus does not stimulate the central nervous system, allowing the latter to produce the sensation of mid-gray with which it seems to fill all areas (like the blind spot, for instance) which are wholly unstimulated.

I assume, furthermore, that of the four primary colors, red and yellow are produced by overactivity of their respective organs, and green and blue by inhibition. Yellow and red, then, might perhaps be thought of as successive differentiations of white, and blue and green similarly of black.

(D) *Revival.* I have assumed that within the range of ordinary color sensitiveness the visual pigment is restored at a constant rate. Undoubtedly this is not strictly true, as the rate of restoration is no doubt dependent in part on the supply of the restoring substance, which, in case it is derived from the circulating blood or lymph, may be under reflex control; and there may also be other disturbing factors. Nevertheless in a broad way the statement is true. The result is that in a steady state of illumination the pigment must be bleached at the rate it is restored; for if it were bleached more rapidly, there would be a decrease in its total amount, and a corresponding decrease in the activity, for the number of molecules decomposed in a given time in a given intensity of light is no doubt proportional to the number present. Similarly less rapid bleaching would promptly allow an accumulation of pigment, and an accelerated rate. As a result of this, in the white cones there would tend to be a constant activity of the nerve under a steady illumination, regardless of the intensity. In fact there is a little variation, but it appears that a change of a good many thousandfold in the brightness of illumination only causes a very moderate change in the sensation, which remains well within the range of a mid-gray. There is evidently, however, a slowing up of the accumulation of pigment as the cell approaches saturation, and no doubt eventually a complete cessation, and similarly the accumulation seems sluggish in a completely exhausted cone,[17] which suggests that the process may follow something like an autocatalytic curve.

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[15] An equally plausible assumption is that the colloidal condition of the protoplasm is changed, and causes a drift of water in one direction or the other, the resulting dilution or concentration being the immediate stimulus.

[16] There is some evidence from a consideration of minimum distinguishable steps, that fewer distinguishable degrees of inhibition are possible than of activity; and Steindler's data, when plotted in a curve, seem to point in the same direction (Fig. 4).

In the color cones a different condition arises. The visual pigment is evidently very sluggish in diffusion, perhaps even non-diffusible, while the restoring substance, being supplied from outside, must be easily diffused. Therefore in this case the inequality in distribution of the purple will remain so long as the unbalanced light is applied, and the equilibrium of rate of decomposition to rate of restoration will only apply to the cone as a whole. Color then will tend not to be completely fatiguable, but merely will be decreased; for the white light, and the light that fails of reflection, will exhaust the purple in the whole cone, and lessen the possible degree of polarization. There should exist with brilliant illumination, then, a paling of all colors proportionately, substantially what the artist represents, when painting them in a high key.

(E) *Explanation of particular colors.* Here follows an analysis of the effect that light of certain particular colors might be expected to have on the three kinds of cones (see Table I and Fig. 3).

Red (Fig. 3, lines 3 and 4). I have assumed that the stimulation of the white cone, which must exist, is negligible with moderate illumination. The red cone will have its entire band of stimulation in the near (red) side and of full intensity. The yellow cone is also stimulated, and of considerable intensity, thereby explaining why adding some blue light to a spectral red produces a more primary hue.

I think most people choose a carmine red as the pure color, unless their judgment has been changed by familiarity with the spectrum, rather than the spectral red. The carmine red is also the color which remains unchanged as one goes toward the periphery of the retina, while the spectral red changes to yellow when one passes outside the zone of red-green sensitiveness. I judge that the amount of blue which is mixed with spectral red to produce this result is the amount which equals the yellow activity caused by the red light striking on the yellow cone.

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[17] By completely exhausted, I mean exhaustion to the point of no longer being able to initiate an impulse in the nerve-fibre. Whether all trace of pigment is exhausted, or whether the amount indefinitely approaches zero as the light is increased is a matter of secondary importance.

Pink. Cited as an example of a color lighter than the spectral color. The white admixture increases the activity of the white cones, but in color cones merely exhausts the visual pigment, and thereby reduces the intensity.

Many people see the pink produced as a dilution of spectral red without any conscious element of yellowness (I happen to be one). I believe that in such cases the yellow activity—which is undoubtedly less than the red activity under red light, the band falling farther from the boundary film and nearer the level where the visual pigment fades out—falls below threshold level, and the red and white sensations only are stimulated.

TABLE I

Color	White Cone	Yellow Cone	Red Cone
Red	(+)	+	+ +
Orange	+	+ +	+
Yellow	+ +	+ +	o
Green	+	o	—
Blue	+	– –	o*
Violet	(+)	– –	+†
Purple (red + blue)	(+)	–	+ +‡
White	+ + +	any	any
Gray	+	o	o
Black	o	o	o
Brown (yellow – low intensity)	(+)	+	(+)
Pink (red + white)	+ +	(+)	+ +

o = No effect, or no net effect.
+ = Moderate intensity.
++ = Strong intensity.
+++ = Maximum intensity, reached by white cone only.
(+) = Theoretically functioning, negligible in proportion to other cones.
* First order in the negative and second order in the positive of red cone equally strong.
† Second order.
‡ Both orders, the first order of the red and second order of the blue light.

Yellow (Fig. 3, lines 7, 8). Yellow light is the color of maximum effect on the visual pigment in the white organs and stimulates them to the point of overshadowing the color process. The yellow cone is of course at maximum activity, while the band in the red cone falls astride the boundary film and is neutral in effect. We then have a maximum yellow process combined with a very high white-process.

Decreasing somewhat the intensity of the light, or what is much the same thing, lowering the white-sensitivity by adapting the eye to a brighter surrounding field, will to a certain point seem to increase the saturation of the color. Perhaps the true primary yellow should be chosen a deeper shade than is usually done (the spectral yellow as seen against a dark background), but it is questionable whether brown should be substituted for yellow.

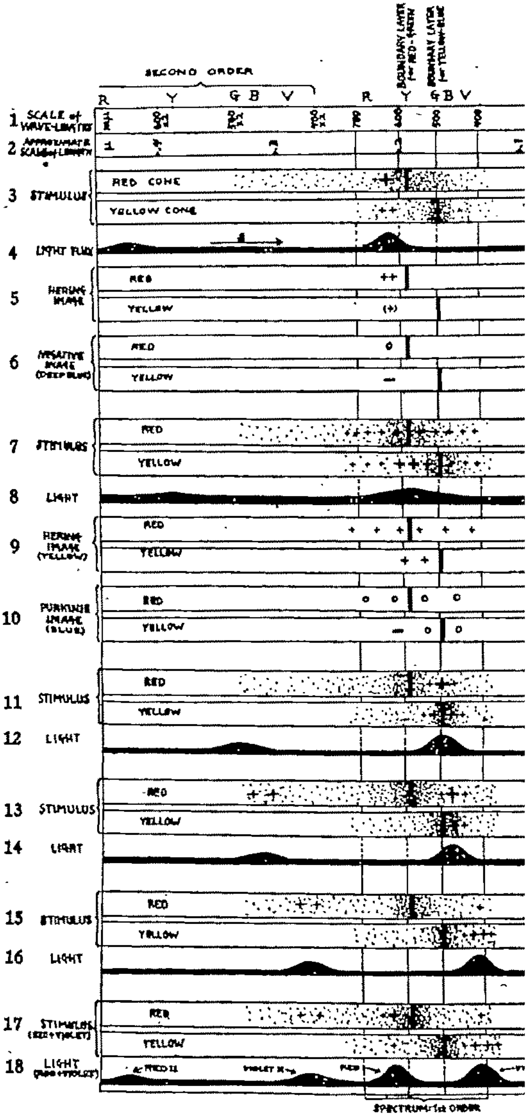

FIG. 3. Illustrating the formation of standing ligh kinds of color-cones, and the relations of reflecting apex dary film, and layer of sensitive substance.

The upper line (line 1) indicates the positions at v waves would fall for the various wave-lengths. Thes back of the reflecting surface equal to half the wave-le by the index of refraction of the cone-substance, whic

Brown. A typical brown will have the same maxima in the color scheme as yellow,[18] but the intensity has fallen to a point where the white sensation no longer dominates the picture. On the other hand the yellow process itself is certainly not functioning at its maximum intensity, and brown hardly represents the true primary color, which should have the saturation of yellow, but more nearly the luminosity of brown.[19] The primary might be described as a yellow *minus* white.

Blue (Fig. 3, lines 13, 14). In the red-green cone it is assumed that both the first and second interference maxima are functioning, and that they are of near enough equal strength and symmetry to neutralize each other, the first lying on the green side of the insulating film and the second on the red, further out than the red maximum would fall. In the blue-

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[18] Or orange, but I take yellow as a type to analyse.

[19] The same is true to a less degree of all the colors, as all can be stimulated only along with the white sensation to a greater or less degree, but the effect reaches a maximum in the middle of the spectrum. One would expect it to be equally strong in the yellow-green, but by the time we reach the invariable (presumably primary) green of around 500μ it has fallen off considerably, and we may assume we experience something like the true primary green sensation.

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of a diagram I have taken as 1.5. The second line (line 2) shows the actual scale of measurement in microns.

In the other lines (lines 3-18) the two oblongs are diagrammatic strips from the apices of the two kinds of color-cones, corresponding to the shaded area in Fig. 2. The reflecting surface and boundary film are indicated by heavy bars, the sensitive band by stippling of a density to correspond with its *effective* sensitiveness (the combined effect of the density of substance and its sensitivity to the wave-length that falls at a given point).

The black band labelled 'light flux' diagrammatically shows the light passing in from the base of the cone, reflected in part from its apex, and producing standing waves represented by the widenings of the band, which form the effective stimulus. These bands should be imagined as superposed on the cones to which they are bracketed. Finally the characters in the cone-diagrams (+, 0) indicate the strength and position of actual stimulus—the product of sensitiveness of the cone (stippling) by amount of light (black stripe).

Hering image (lines 5, 9). The small plus signs indicate the residual positive activity a moment after the actual stimulus and its resultant activity (lines 3, 7) ceases. It is like the reaction to the continuing stimulus, but weaker.

Purkinje image (line 10). The stimulus (line 7) has fallen on a saturated cone (therefore not actively regenerating the visual pigment) and is of intensity too low to accumulate active substance and leave a surviving Hering after-image. There is therefore no activity surviving, but a defect due to fatigue at the point of maximum previous activity in the yellow cone (indicated by a *minus* sign) which throws that cone out of balance and leaves a blue (*minus* yellow) sensation. With red light (line 6) there is no Purkinje image, but after a strong stimulus and its resulting Hering image the less strongly stimulated yellow cone goes into its negative condition more rapidly than the strongly stimulated red one, resulting in a blue negative condition.

The diagram for purple (lines 17, 18) is merely a superposition of those for red (3, 4) and for blue or violet (15, 16).

yellow cone the first maximum falls on the blue side, while the second falls beyond the end of the pigment area and is inactive.

Violet (Fig. 3, lines 15, 16). Here the second maximum is assumed actually to overpower the first, so that the red-green cone is functioning actually as red, the stimulation being greater than the inhibition. Note that the *wave-length* of the light serves merely to determine its effect on the visual pigment while the *position* (in this case dependent not on the wave-length itself but the *double* wave-length) determines the color reported.

Purple (Fig. 3, lines 17, 18). To obtain a real purple we have to combine red with blue or violet light, omitting the intervening colors. In my diagram of this color I have assumed a mixture in which the violet band is enough stronger than the red to have a dominant effect on the yellow cone, while on the red-green cone the red band is fully functional and the violet (or blue) produces a second-order band to strengthen it further.

It is plain that a larger proportion of blue than that necessary to produce purple will produce a violet (only of less than spectral saturation), the red partially neutralizing the effect of the blue on the yellow cone, while in the red cone the red is fully present, but the blue serves only to exhaust the cone. Similarly with a greater excess of red, the small blue band will merely cancel the effect of the red light on the yellow cone while falling outside the region of strong activity in the red cone, so that a pure red activity will be left (but of less than spectral saturation).

Spectral red plus green. According to Ladd-Franklin spectral red plus green produces yellow. In the proposed theory the white cone will function moderately (average of red and green effects), the yellow cone moderately (the green falling on the neutral part and the moderate yellow stimulus caused by the red surviving), and the red-green cone not at all, the red and green neutralizing each other. In sum there will be the same combination as in the case of yellow light, but with each process of lower intensity. Of course as noted by Hering,[20] a red and a green can be chosen that will neutralize to produce white, and Hess[21] finds that as closely as can be measured these are the exact colors which drop out together without exposing any yellow or blue under-tint in the peripheral retina. No doubt they are the true primary colors.

Blue plus yellow. According to all authorities blue plus yellow produces white. The yellow will stimulate the white cone strongly, the yellow cone will have strong but evenly balanced stimuli, resulting in no excitation, and in the red cone the yellow will fall at the zero position astride the boundary film, while for the blue the first and second interference maxima will be evenly balanced. Therefore there will be no color sensation, but a strong white.

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[20] Parsons, *op. cit.*, 68.

[21] C. Hess, *Arch. f. Ophth.*, 35, 1859, 1. See Parsons (*op. cit.*, 70) for further references.

DISCUSSION

In going over published work to find data which tend either to confirm or contradict this theory, a curious fact stands out very strongly: practically all the experimental work has been done at a level of adaptation where the eye is most unstable and sensitive to changes of illumination, and where the phenomena are most complex. Furthermore a very large proportion of the work has been done with the parafoveal part of the retina (1°–6° circles), a region of whose distinctive physical structure practically nothing is known. As a result published data are very difficult to interpret on the basis of any theory. A few examples may be cited.

Abney,[22] attempting to determine the threshold sensitiveness of the retina allowed 12 min. of dark adaptation. It is now known from Piper's work[23] that adaptation goes on for an hour or more (and to a certain extent for many hours); and also that 12 min. after entering the dark-room is a period when adaptation is going on at a very high rate—for many observers at the maximum rate. This fact has been verified very strikingly by Judd.[24] The only probable explanation of Abney's result appears to be that there was a very substantial amount of stray light, which held the observers' retinas to approximately the level which a normal eye reaches at the end of 12 min. This naturally throws doubt on all the quantitative results he publishes, as there is no guarantee that this light was constant in the course of his experiments, and as at that level the eye responds very rapidly to changes of illumination. In other cases (as with Priest, in his work on the determination of white)[25] the eye apparently came into equilibrium with the average effect of an intermittent stimulus. In this case the eye level was fixed in the sense that the experiment could undoubtedly be duplicated, but it is equally certain that the eye was being tested at the point where photopic and scotopic phenomena were mixed, and it is very questionable if the results would apply to normal day vision, and not certain that even a moderate change in the brightness of the stimulus might not cause a large difference in the value.

Steindler[26] has shown the very striking differences that exist among normal individuals in relative color-sensitiveness, and it is to be presumed that her variations in the regions of maximum power of discrimination may correspond with differences in the wave-length which excites the four primary (invariant) retinal sensations, but so far as I know no one has made these two kinds of measurements on the same individuals. She also has not distinguished change of saturation from change of hue, nor reported either the brilliancy of the light nor the retinal state, further than to indicate that the light was not constant for different parts of the spectrum The retina appears to have been in equilibrium with the stimulus light, but it seems not to be clearly stated, and as the stimulus light was small and apparently surrounded by a black field it is perhaps questionable.

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[22] W. de W. Abney, *Phil. Trans. R. S.*, 190 A, 155, etc.

[23] H. Piper, *Zsch. f. Psych u. Physiol. d. Sinnesorgane*, 31, 1903, 161.

[24] D. B. Judd, this JOURNAL, 38, 1927, 507–533. Cf. Fig. 9.

[25] I. G. Priest, *U. S. Bureau of Standards, Tech. Paper* 417. I have not seen the original report; in the summary in the *J. Opt. Soc. Amer.*, 5, 1921, 205 ff; there appears to be a contradiction in the statement of the condition of adaptation. his condition 2c being impossible.

[26] O. Steindler, *Sits. d. Weiner Akad.*, 115, 1906, 115.

Rods and cones.[27] There is extraordinary disagreement as to the structure of the retina. The fact appears to be that typical rods and cones, as described in the text-books, only exist in the peripheral retina, and may be ignored in interpreting most experimental work. The fovea is said to contain only cones, but the more careful writers always make this statement with some proviso. The fact appears to be that the foveal organs are almost typical rods, and a monkey retina in the Cornell University collection agrees with this.

The distinctive characters of a rod appear to be: (1) it extends clear to the pigment membrane; (2) it is slender and nearly cylindrical; (3) its nucleus lies close against the limiting membrane; (4) its efferent fibre ends not in an arborization but in a knob, which is received in a basket-like dendrite, apparently connected with a single ganglionic cell; and (5) it contains visual purple.

In the first four of these characters the foveal organs would be *rods.* If we assume that the form and the pigmentation of the organs are not necessarily bound together, it would appear that the low sensitiveness of the foveal organs is correlated merely with the absence of visual purple. The second and fourth structural features and perhaps the first seem really adapted for accurate vision, and would actually imply that we may have to reverse the rôle of rods and cones. It seems more plausible that the cones of the outer retina are the color organs, and that the rods produce the white sense at all levels, and also the finer details, for which their one-to-one connection with the ganglion cells would seem to be adapted. In that case the diffuseness of nocturnal vision would have to be explained as due to the withdrawal of pigment, and probably also to the coming into function of tangential connections in the deeper layers of the retina. The third after-image (diffuse) would be also explained in the same way, and its tardiness and diffuseness is quite what would be expected of cells of that type.

A good many experiments on foveal vision are vitiated by the use of too large a stimulus. The fovea is only about ¾° in diam.[28] and various experiments reported as foveal use stimuli of a diameter as large as 2°. This complicates the analysis of reports on supposed foveal after-images. In fact, with my own eye the fovea seems to have a bias toward a flash-image of red-yellow[29]—it is apparently thrown into greater activity by all colored stimuli. This is difficult of interpretation, and equally difficult with all theories thus far proposed.

Visual violet. Visual violet is an unsatisfactory term; the pigment should in fact be a purple, but the term visual purple is already preëmpted for what is in fact a rose. The fishes[30] appear to have a pigment much more like what I should like to postulate, the maximum absorption being at 540 mμ, fairly in the center of the yellow, about where the maximum brightness is in man in the lower stages of color-adaptation (mesopic).[31] It is possible that this is due either to a more primitive substance which is differentiated into two substances in man, or to an equal mixture of the two substances—the postulated visual violet being in larger quantity in the fish.

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[27] J. H. Parsons, *An Introduction to the Study of Colour Vision*, 203 f. In what follows I shall refer frequently to this book, which gives summaries of the work on the various fields, and full references to original reports of experimental work. I have gone back to the sources in many cases to find if conditions were not more strictly defined than would appear from Parson's account, but usually have been disappointed.

[28] Parsons, *op. cit.*, 9, gives it about .3 x .2 mm.

[29] Flash-image yellow with yellowish or blue-green stimulus alike.

[30] Köttgen and Abelsdorff, *op. cit.*, 171 ff.; Parsons, *op. cit.*, 13.

Purple and violet. There is considerable confusion in the literature in the use of the terms blue, purple, and violet. Parsons[32] speaks of violet as a color "which occurs rarely in nature." It is certainly true that a color composed only of the shortest wave-lengths is very rare in nature (in the rainbow and on a few butterflies), but to my own eye there are 'purples' which exactly match it but are spectrally mixtures of blue or violet and red. Evidently to Parsons there is a far smaller proportion of red in the sensation produced by the shortest waves, for he writes as if there were little likelihood of confusing violet (as a color) with purple, and as though the confusion were only of nomenclature.[33] One the other hand he accepts Abney's 'blue' of 460 mμ, and even 435 mμ,[34] as blues without comment. To my own eye neither has any real right to the name of blue, the first is decidedly an 'indigo' half way between blue and violet, the second has almost as much red as blue, and could be well matched by the 'purple' of such flowers as the clematis or the violet. My belief is that such variation of judgment points to a real variation in the amount of red stimulated by the short waves, and by the theory here presented is easily explained on the ground that persons who compare spectral violet with blue rather than with purple have a substantially thinner layer of pigment than those (like myself) to whom 'violet' and 'purple' are merely different words. On the classical theories this variation is difficult to explain, as an abnormal amount of yellow pigment in the macula should produce more disturbance of the blue (480 mμ) as well.

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[31] I propose to use the term *mesopic* to represent the states of eye adaptation intermediate between the ordinary daylight state (photopic) and the fully achromatic state of dark-adaptation (scotopic). Besides qualities intermediate between those of day and dark adaptation it has some special features of its own, notably the dominance of the Purkinje type of flash after-image, and a peculiarly high degree of saturation of some colors, combined with a relative insensitiveness for yellow. In full photopic vision the Hering after-image is dominant or exists alone, while in scotopic vision no colored after-images are present. A further and more important reason for distinguishing this level, is that a very large part of the actual experimental work is done within it, the stimuli being kept for convenience near the lowest level at which colors are perceived with good saturation, and this work has been reported either as done on photopic or on scotopic vision according as the observer had lately been working in daylight or in a dark-room. This is also a region where the eye both fatigues and adapts with maximum rapidity, so that the exact state of the eye is far more important than at either scotopic or photopic levels. It may be roughly defined as the period after 12 min. in the dark-room and until color sense is lost. Further on (Fig. 5) I have tried to indicate some possibilities of further subdivisions of this series of states.

[32] *Op. cit.*, 27.

[33] *Op. cit.*, 75.

[34] *Op. cit.*, 163, in discussion of König and Dieterici's work.

Brightness and saturation. If my theory is true, brilliance or luminosity,[35] as measured for instance by the flicker photometer, is a function of the white organs *only*, as with Hering's theory.

By the usual method of use of the flicker photometer the rapidity of alternation of the two fields compared is set so that there is a strong sense of flicker when white lights of unequal brightness are compared. If then we compare two contrastingly *colored* lights of slightly unequal brightness there is a curious phenomenon. There is first a well-marked flicker of the same frequency as before, but without any distinct color quality, and superposed on this a very marked but slow alternation of the two contrasting *colors* in practically their full saturation or even with exaggerated saturation. This shows very strikingly with green and orange. At the proper degree of unbalance I am able to see both the rapid flicker and the slower alternation. If my theory (or any other which involves a separate white process) is true, the true flicker is due to the white-organs alone, and is entirely independent of the slower alternation which would be due to the more strongly stimulated pair of color-organs. The slow alternation would no doubt be some form of fatigue-phenomenon. At a slower speed the apparent alternation of color is synchronized with the actual alternation of stimuli, but at that speed the noticeable flicker of *intensity* is no longer prominent. The simplest explanation would be that the white- and color-organs have different rates of flicker, and the alternation which causes the phenomenon in either is ineffective or produces a non-synchronous fatigue effect in the other. If so the flicker photometer as practically used would be a really accurate measure of the effect of the various lights on the white-organs *alone*.

There is a good deal of confusion in the use of the terms brightness, brilliancy, luminosity, and the like. We may accept Troland's definitions: brilliancy is the psychological effect, which according to the four-color theories measures the activity of the white-process, and is what Abney and others call luminosity. Luminosity will be the quasi-physical property which would correspond to the brilliancy as seen by an eye in a standard state of adaptation, and is a practically measurable quantity, by comparison under standard conditions with a standard light. Brilliancy, according to this theory, is luminosity multiplied by a factor representing the amount of functional visual pigment at a given moment.

Similarly there are two different conceptions of saturation. First, it may be viewed as the measurable ratio between the amount of spectral light and the total (spectral *plus* white) light in an artificial mixture of spectral and white light which matches the given light (this is a quasi-physical quantity corresponding to luminosity, and applies to a standard eye). Secondly it may be viewed as the ratio of the activity of the color process *in the eye* to the white process (brilliance). There is no necessary relation of one of these things to the other, but I have not seen them clearly distinguished. Under the present theory the activity of the color process, being due to a state of balance of the two sides of the color cones, will tend to become weaker with exhaustion of the visual pigment by the total amount of light, while in equilibrium the white process tends to become unity (as explained in the first part of this section). Therefore the saturation (sensation) will tend to become less with increasing luminosity, without any change in the physical saturation. If the eye is not in equilibrium the brilliance may be more or less than unity, according as the stimulus has lately increased or decreased, and the color process will also increase or decrease *pari passu*, if there has not been time for any adaptation, but if adaptation has begun the effect will depend on the relative adaptation curves of the white and color organs, and cannot be predicted a priori. But considering the apparent sluggishness of the color organs, as shown by

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[35] The words luminosity and brilliance are distinguished and defined by Troland (*J. Opt. Soc. Amer.*, 6, 1922, 537), restricting brilliance to the meaning given here, which is called luminosity by Parsons (*op. cit.*, 29); Abney, and others. Troland would restrict the term luminosity to the psychophysical quality mentioned below.

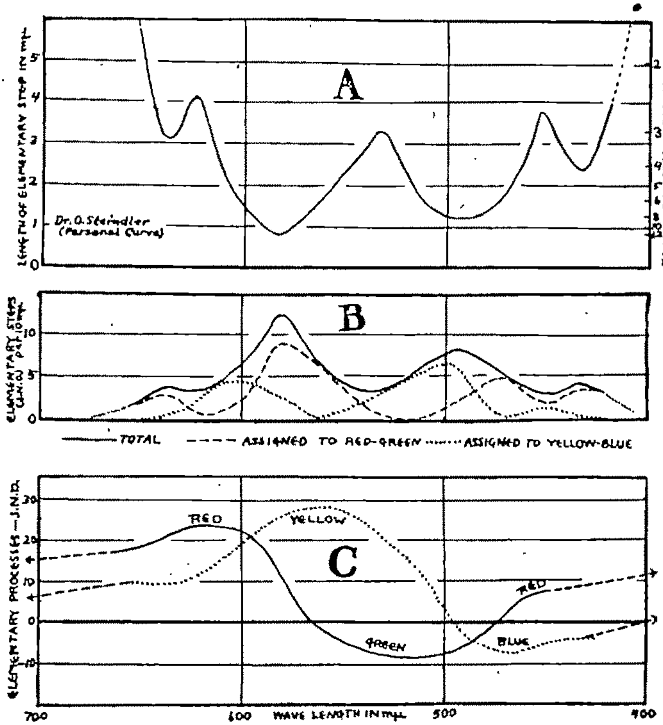

FIG. 4. Elementary processes, plotted in J.N.D. units.

A shows Steindler's data on the minimum discriminable difference of wave-length for the various hues of the spectrum. The scale at the left shows the length of the elementary step (J.N.D.) in millimicrons, the scale at the right the corresponding number of elementary steps per 10 mμ.

B shows the same data replotted with the number of J.N.D. per 10 mμ as a unit, and these numbers are arbitrarily divided between the red-green and yellow-blue processes, on the assumption that they share equally in accounting for hue-discrimination as a whole, and that the sum of the steps of discrimination for which each is responsible in a given interval is the number of J.N.D.s observed in that interval. It is assumed that at the two points where protanopes show minima of hue (saturation) discrimination, the whole normal discriminating power is to be assigned to the red-green process, and that there exist two similar points where the whole discrimination is due to the blue-yellow process, these being the points of invariant blue and yellow.

C is constructed by measuring off the numbers of steps thus found in the second curve.

their relative slowness of flicker, we may expect a decrease of sensation-saturation with a partially adapted increase in intensity, and an increase with a decrease in intensity. Of course with very low intensities the color organs drop out and we get decreasing sensation-saturation under a constant stimulus-saturation, even in equilibrium.

Discriminability of hues. Steindler[36] and others have found the power of discrimination of hues very different in different parts of the spectrum and substantially different even for different individuals. Steindler corrects for difference of brightness, but seems to have made no attempt to distinguish between difference in hue and in saturation. It is to be assumed that with color-blind individuals saturation alone is involved, but that with normals both are present.

I present, in Fig. 4, Steindler's data for herself, as a curve, and I attempt to analyse it into the two color-processes, on the assumption that the number of discriminable steps between any two colors is the sum of the unit differences for the two color processes between the same limits. For some of the observers a smoother curve could be made by assuming that except where the two color processes were of substantially equal strength, only one of them was active in discrimination. The relative smoothness of the two unitary curves, as compared with the combined (experimental) curve of discriminating power, is striking. To get the general character of my blue-yellow curve I have chosen those of her protanopic observers who had about half the normal number of steps in the spectrum, and I have allowed about 24 steps to make the connection between violet and red, not studied by her. If the exact neutral points of individual observers were available it might be possible to make even a better curve, as presumably these should fix the points where the two curves cross the base line. The very unequal number of steps between complementary colors according as one goes through yellow and green or through purple, indicates that the discriminating power in the yellow and green is in some way dulled. Perhaps the intensity of the white process in that range lessens the discriminating power for colors. Presumably all these experiments were done in the 'mesopic' range of the eye,[37] and part of this buried discriminating power might be uncovered at a higher adaptation level, especially in the yellow. As the average primary colors were also determined with weak lights, there is probably no extra error introduced in using them. I have used a red ¼ way to violet, yellow 575 mμ, green 500 mμ, and blue 475 mμ, being a fair average of the values determined by the peripheral retina, by introspective methods, and by the protanopic eye, which I assume has the blue-yellow process unharmed.

Night-blindness. There seem to be two distinct phenomena called night-blindness. In the more typical case the sensitivity of the retina becomes zero at a little below the level where for a normal person scotopic vision comes in. This type can be easily explained as due merely to the complete absence of the scotopic pigment. If that is the case there will be a zone where the eye can still perceive light, but much less than in the case of a normal eye, this being the zone in which the photopic pigment is still sensitive, but in the normal eye is overlapped and covered up by the vastly greater sensitiveness

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[36] O. Steindler, *Sitz. d. Wiener Akad.*, 115, 1906, 115; Parsons, *op. cit.*, 30.
[37] See note 31, above.

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of the scotopic; and a lower zone where in the normal eye the scotopic pigment alone functions. There would not necessarily be any change in the color threshold. Another possibility would be a subnormal quantity merely, of the scotopic pigment. Thirdly, there might be a subnormal supply of the restoring substance, either from actual lack of material or from deficient circulation. In this case, as in some known night-blind individuals, there should be slow adaptation, without necessarily any change in the final level of adaptation.

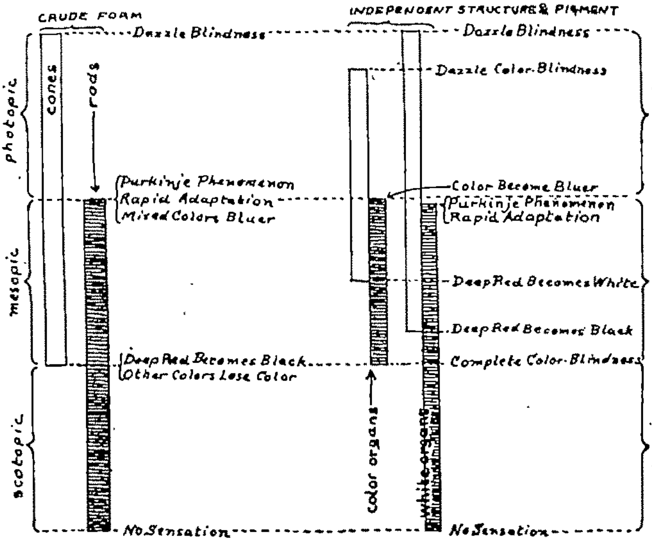

Fig. 5. Diagrams of the duplicity theory and its various thresholds, the left diagram on the usual basis, the right one on the assumption that structure (color process) and pigment (photopic and scotopic) are independent.

Thresholds. If in fact there are two types of end-organs, one of which at least is provided with two different pigments, there should be three or even four separate thresholds, representing where the color function changes to scotopic and then drops out, and where the white-organs do the same. Further, if the extreme red light is used, to which the scotopic pigment is insensitive, the two bottom thresholds of the photopic process might be identified.

In the ordinary form of the 'duplicity theory' (Fig. 5) it is assumed (often tacitly) that the two forms of end organs are wholly distinct, in which case there should be a single threshold where the change of function of one to the other type occurs, or at most two, with a region between them where both types are

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functioning together; and most formulations are on the basis of a single threshold. If light sense and color sense are independent this is by no means necessary. In fact, there is a well marked change in the color sense, marked most conspicuously by a decreasing yellow sensation in all colors of which yellow is a component, at a considerably higher level than that at which color drops out. This change seems to take place at the same level where with a momentary stimulus the Purkinje type of after-image takes the place of the Hering type, while the whole range of conditions where the Purkinje image is alone visible seems unprovided for by the usual theories.

The thresholds then are:

(1) Color becomes scotopic—Hering after-image drops out with moderately dazzling stimuli, yellow sensation weakens, deep red light appears white;[38]

(2) White sense becomes scotopic—adaptation suddenly becomes very rapid (12 min. in the dark), Purkinje phenomenon (relative insensitiveness for the red end of spectrum);

(3) Photopic white-sense drops out—deep red becomes black;

(4) Color sense becomes blind—all color sense vanishes, after-images also becoming colorless;

(5) White sense becomes blind and the eye is wholly insensitive.

These thresholds are obviously different in different zones of the retina. In the center of the fovea they are apparently at the same level, while toward the periphery, on account of the small number of color-organs, in the case of light of low intensity or saturation, the white sense overcomes the color before the latter is theoretically extinguished. I think all five thresholds have been identified in the outer parafoveal region.

Retinal color-fields.[39] One of the strongest supports of a two-color and white theory is the existence of four and only four colors (presumably the primaries) which do not change color as one passes out to the peripheral retina. These are identified by Hess and others[40] as an ultraspectral red (complentary to 495 $m\mu$), yellow 574.5 or 570 $m\mu$, green about 495 $m\mu$, and blue 460-470 $m\mu$. There is substantial agreement, especially as in general no allowance is made for eye adaptation—threshold (1) above— or for foveal pigmentation, and in some cases even fatigue does not seem to have been clearly eliminated.

Abney[41] stands out in contradiction to these results, as he finds no sharp color boundary in the outer retina; but he chose colors which were known not to be the invariant ones, and does not state whether his setting is the threshold for color, or the point of color change. He gets markedly similar curves for his 'red' and 'yellow,' which is hardly surprising when one considers that his 'red' was a spectral color, which is generally admitted to stimulate the yellow (or on the three-color formulation the green) sense, and his 'yellow' was an orange with a conspicuous percent of red (the sodium line). His green was definitely more yellow than the invariant color, and in his more careful experiments was strongly yellowish (530 mμ) while his blue, at first not far from the invariant color, was later a true and pure violet (430 mμ).

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[38] Parsons, *op. cit.*, 61.

[39] Parsons, *op. cit.*, 75 ff.

[40] Parsons, *op. cit.*, 68-70 gives references.

[41] W. de W. Abney, *Phil. Trans. R .S.*, 190 A, 1897, 155-195; *Researches in Colour Vision*, 190; Parsons, *op. cit.*, 74-80.,

After-images.[42] The generally recognized after-images of a short stimulus are of three types: the Hering is momentary, and positive both in hue and brilliance; the Purkinje is positive in brilliance but negative in hue; and the slow image is positive in brilliance, faintly positive in hue, and diffuse. By the present theory the color and white processes should be considered separately. After a momentary colored stimulus at a high adaptation level there should be in the white-organs an accumulation of the activating substance and partial exhaustion but a steady replenishment of the sensitive pigment. So long as the activating substance is undischarged there will be stimulus of the nerve-ends, and a brightness-sensation. I assume that the discharge takes a perceptible length of time, and longer as the amount of substance (which is dependent on the *product* of the amount of light by the amount of visual pigment at the moment of stimulus) is greater. If the activating substance is discharged before the exhausted pigment is restored to an average level, there will be a lowering of the idioretinal activity in darkness or of the sensitivity in light, and a negative after-image will result. Finally the third after-image, which is diffuse and shows the appearance of scotopic vision, is, I assume, dependent on reflex activity, and also possibly on some oscillation in the relation of the process of restoration and of idioretinal discharge.

If the stimulating light is colored, there will also be a similar state of the color-organs. In this case the pigment will be not bleached in the organ as a whole, but in a zone which throws the organ out of equilibrium. Suppose the stimulus light is orange. Then in the deeper side of the color organs there will be a region of accumulation of the active substance ('acid'), which will cause the organ to function positively for a greater or less period according to the amount accumulated, just as with the white-organs. When this is dicharged there may come on a negative period, just as with the white-organs, but not necessarily synchronous with it. If the negative period comes on while the white-organs are still positive there will be a Purkinje (− color, + white) following the Hering image; if they are simultaneous, the negative phase will be superposed on a black sensation and will be difficult to detect, though I have seen it, especially with a red stimulus.

If the stimulus were blue or green the effect would be reversed, inasmuch as the accumulated substance would cause inhibition, and when it was discharged the remaining unbalance would leave activity on the deep, positive side of the cone.

In my own case a flash of moderate intensity with day adaptation produces only a positive (Hering) flash; in this case no doubt the color process is restored before the active substance is exhausted, or else the effect comes after the white-organ has reached its negative phase and is overlooked. In the case of a very intense stimulus, both the activity of the white and the exhaustion are enormously increased, and after a Hering phase a period comes when the cone is negative and the white-organ positive, and the Purkinje image appears.

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[42] Parsons, *op. cit.*, 42 ff.; D. B. Judd, *op. cit.*

The flight of colors, which appears with a particularly intense stimulus, would appear to be due to the two sets of color organs having different periods. It is said to lack the yellow element,[43] but in my own case the yellow is very easily elicited, much more easily than green. Of course if the yellow falls on a negative condition of the white-organ it will be very dark and will appear as a brown. This is obviously dependent on the relative rate of oscillation of the color- and white-organs, and can be adjusted in a given person by changing the saturation, decreasing saturation naturally causing a relative greater stimulation of the white process and a slower relative oscillation.

I assume that if the cones are wholly saturated with pigment under dark adaptation the restoring process comes to rest. In that case a colored stimulus will produce a minimum of active substance, which will be soon discharged, while the restoring process, having been dormant just before the stimulus, will be slow in taking effect; therefore there will be a minimal and sub-threshold positive (Hering) image, but a marked negative (Purkinje) one. I take then the presence of the Purkinje image alone with a moderate stimulus as a sign that the cones at the moment of stimulus were saturated, and restoration inactive. With a very bright light, of course, there will be enough substance decomposed to persist and leave a Hering image preceding the Purkinje one.

Color Blindness.[44] As in other two-color theories I assume that the protanope merely lacks the red-green process and organs, being otherwise either normal, or possibly in some cases also lacking the visual violet, but not the blue-yellow organs or structures at the fovea. In that case the spectrum will be bisected at the neutral point for the blue-yellow organs, all wave-lengths below that giving blue and above it yellow. This should give a value for the neutral point the same as the criterion of invariance in a normal person and does so within the possible range of accuracy (498 mμ).[45]

I assume that in the case of the deuteranope there has been, not a loss of organs but a reversion of type in the color organs, so that there is a single type intermediate in properties between the usual two. There seems to be a slight change in the neutral point (about 5 mμ in the case of Steindler's measurements, where the two series seem to have been strictly comparable), and the sensitivity to the red end is markedly shortened, and would appear to correspond to the 'violet-blind' individual with no other defect, the layer of pigment in the color-cones not being deep enough to produce a red element in the violet sensation, (perhaps I should rather say a warm element in the extreme cold sensation). Whether the white-sensation is or is not abnormal in these two types should be determinable by a study of lights of varying saturation, which I think has not been done.

The tritanope would simply lack the blue-yellow organs. It is interesting that these should according to this structure be thinner in their 'working-parts' than the other sets, and so more susceptible, perhaps, to injury from the back of the retina. It seems in fact that tritanopia is wholly the result of a pathological process, and, normally at least, of temporary separation of the retina.[46]

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[43] McDougall, as quoted by Parsons, *op. cit.*, 111, 277 ff.

[44] Parsons, *op. cit.*, 169 ff.

[45] Steindler, *op. cit.*

Complete color blindness would be primarily due to a lack of color organs of either type. It is a very rare condition and usually seems to be correlated with other pathological changes. If this theory is true there should be a possibility of cases where the color blindness exists without the usual accompanying day-blindness due presumably to the lack of the 'visual violet.' I do not know of any such cases.

SUMMARY

The two leading theories of color vision at present are (1) the *three-color* theory, which is insufficient to psychologists as indicated by Ladd-Franklin and others; and (2) the *four-color*, which is unsatisfactory to physicists. Ladd-Franklin's compromise theory is also unsatisfactory because it postulates at least three separate processes in a single nerve fibre.

One nerve fibre can carry only one excitation, variable in only one direction, *i.e.* frequency. Three separate variables are necessary to supply the known range of color. Three nerves are, therefore, needed for a complete color sensation. As there is only one nerve fibre to every cone, three kinds of cones are required for theory.

The interference theory here proposed assumes that only one light-sensitive substance is concerned with photopic vision—a visual violet substance similar to the familiar visual purple of scotopic vision.

Three kinds of cones, however, are postulated: (1) *white*, with no special structures, in which the visual pigment is presumably diffuse; (2) *yellow*, with a reflecting surface at its further end and an insulating film at a distance back from it where interference causes a maximum intensity for *green*—the visual pigment being present in equal amounts on the two sides of the film; and (3) *red*, similar to the yellow in structure, but with the insulating film at the maximum level for yellow.

White light and light which fails of reflection produce equal activity on both sides of the film and no net action on the color cones.

Colored light has a level of maximum activity caused by interference of the direct and reflected waves. If this lies on the same side of the film as cell-body and nerve it causes increased activity of the nerve (above idioretinal level) and yellow or red sensation. If beyond the film it causes inhibition, and the blue and green sensations respectively. Oxidation, with its resultant acid formation and change of colloidal state, is tentatively suggested as a possible mechanism—acidity in contact with the nerve causing activity, and acidity away from the nerve producing a relative basic condition next the nerve and inhibition.

Visual pigment is restored at a relatively constant rate but ceases under a very dim light as the cones become saturated. Under constant illumination the pigment is exhausted to a point of equilibrium—to a point where it is restored as rapidly as it is exhausted. The color-cones tend, therefore, to produce a uniform color sense under any illumination if the ratio of unbalanced light (colored) to the total remains constant. (This may be somewhat modified by the reflex control of the restoring substance.)

The total capacity of white-cones is higher than that of the colored. Very dim light, therefore, produces no color-sensation.

Polarization of the cones is not discharged immediately; the lag causes the positive after-image. Purple is restored slowly, causing an unbalanced idioretinal activity and a negative after-image.

The exposition of the interference theory presented here is followed by a discussion of other theories and of published data.

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[46] See A. König, *Sitz. k. Pr. Akad. Wiss. zu Berlin*, 1897, 718 (collected papers, p. 396)-Parsons, *op. cit.*, 180-181, gives a full bibliography.

ON SUBSTITUTION AS A CAUSE OF ERRORS IN THINKING

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I

The aim of this study is to examine critically Selz's theory of errors in thinking.[1] On the positive side, Selz explains how errors occur; on the negative side he refutes the theory which depends upon the law of substitution. We intend in this paper to show that Selz's criticisms are not sound; that the law of substitution, properly understood, is valid; and furthermore, that the law of substitution provides, for at least one type of error, a necessary supplement to the explanation given by Selz.

Selz's own theory is based on the fact that, in the case of task-directed thinking, we really have in the *Aufgabe* a conscious aim before us, and therefore anticipate the solution. But the anticipation is not complete, *i.e.* it does not cover the solution sought for in all respects, such as might occur when we anticipate an event by means of an image of it. The anticipation of an aim of thought, of the solution of a present problem, is, on the contrary, a schematic anticipation. All the constituents of the result sought for are anticipated in some way or other. One of these constituents is, however, anticipated more or less indefinitely, viz., that which has to be found by the process of thought itself. But this indefiniteness of the constituent or part sought concerns only that part itself, not its relations to the other constituents of the solution. These are already anticipated in the consciousness of the aim. The solution itself forms a relational whole (*Sachverhältnis*). In the anticipation of the relations of the constituent sought to the already known constituents of this related whole, we have psychologically, in the consciousness of the aim, the indirect characterization of what is sought; for, because the anticipation is thus constituted, only one object will correspond to it as standing in these relations, and hence only one such object can be recognized as agreeing with the anticipation and as the solution of the problem or task.[2]

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[1] Otto Selz, *Über die Gesetze des geordneten Denkverlaufs*, 1913; *Zur Psychologie des Produktiven Denkens und des Irrtums*, 1922. These titles will hereafter be referred to respectively as Selz I and II.

[2] Selz, II, 371.

Arguing from the introspective protocols obtained from his subjects, as well as along other lines to be discussed later, Selz comes to the conclusion that incorrect reactions of the *S*s to the problems set them, were not due to strong reproductive tendencies issuing from the stimulus-word and running contrary to the direction of the task, but that such reactions were the result, at least usually, of task-directed intellectual operations serving the solution of the problem. The most common ground for these errors was, in his experiments, that a related problem was served instead of the one actually set. They were thus due to what may be called task-displacement.[3] Thus, the task actually set may be misunderstood; or, without the actual occurrence of a misunderstanding, only a part of the task may exercise sufficient directive influence on the ensuing processes of thought, some of the characteristics which the correct solution would have to satisfy being lost sight of to a greater or less extent.[4] In some cases, where a particular characteristic of the task remained too inactive, another took its place, the fact being unnoticed. In others, the characteristic was merely dropped, instead of being displaced by another.[5] The mistakes, in a further class of cases, were not due to the fact that the task undertaken, while clearly conceived, was understood in a sense related to but not identical with that of the task actually set. They were, on the contrary, due to the fact that the task set was not clearly thought, and so was merely shifted in the direction of a related one. Such lack of clarity with regard to the task may consist in the fact that only one or more of the characteristics of the task set come into the foreground of consciousness, while others have too low a degree of consciousness or of dispositional efficiency.

On the basis of the types of data described above, Selz formulates the law of complex-activity[6] or efficiency as follows: The lower the degree of consciousness or of dispositional efficiency of constituents (independent or dependent) of a complex, the more readily do these constituents remain without influence in the activity of the complex, *i.e.* the more readily the complex acts as if these independent or dependent constituents were not present. He calls this the *law of part-efficiency of complexes*. On the other hand, if a complex is acting with full efficiency, then all its dependent or independent constituents exert a directive influence as a whole on the ensuing process. The partial activity of a task, being thus due to task-displacement, takes place in the same way as does the full activity of the task. It leads to errors only because not all parts of the task actually set are sufficiently active.[7] If, however, the thinker has at his disposal a familiar solution, then a constituent of the task, having a low degree of consciousness, can still have influence on his reaction. Should such a solution not be at his disposal, reproductions will take place which correspond only to the other characteristics of the task.[8] Furthermore, if one task is displaced by another, the latter will, of course, in its turn form a complex in the sense defined above. If we give the name of residual complex to that part of the original or total complex which still retains enough efficiency, then partial activity of the original complex will take place the more readily, the stronger the tendencies are which proceed from the residual complex (as a whole) in comparison with the strength of the tendencies proceeding from the total complex. This part-efficiency may be the result, for example, of the familiarity of the solution corresponding to the *fully active* constituents of the anticipatory complex schema.[9] Where the meaning of a task is not clearly apprehended, that meaning of it may obtain the upper hand which offers an easier solution as the result of being more familiar.[10] Thus the task 'whole' displaced the task 'superordinate' only when the former offered an easier solution.[11] A similar phenomenon occurs in the case of motor activities.[12] In other cases a shifting of the meaning of the stimulus-word occurs, *i.e.* it is displaced in consciousness by another meaning closely related to it. Since the total task includes the meaning of the stimulus-word, we have to do in such cases with a part-efficiency of the total task. Favorable and contributory to this taking place is a fragmentary imaginal representation of the meaning of the stimulus-word, or also the fact that its meaning is present in an exemplificatory way, *e.g.* as when we think of a special case either in an imaginal or non-imaginal manner.[13] Every distraction of attention favors lack of clarity with regard to particular characteristics of the task. Especially if a part of the task has already a lower degree of consciousness than the others, a reduction of the degree of clearness of the task must affect such a part most of all.[14] Selz holds, furthermore, that the more abstract characteristics of the objects of which we have knowledge are, often at least, more readily brought to consciousness than are their more concrete features. Where a content comes to consciousness in isolation, or relative isolation, from others with which it is connected in our knowledge, we may say that it is abstracted from the full content. The abstract characteristics will, then, have a higher degree of abstractive preparedness.[15] If then the abstract characteristics come most easily to consciousness, *i.e.* if among the active constituents of the meaning of a task the most abstract characteristics have usually the highest degree of consciousness or efficiency, it is possible to see why the great majority of errors in the experiments were conditioned by a partial efficiency of the task. This partial efficiency was in all cases traced back by Selz to the fact that the more abstract or general characteristics possessed a higher degree of consciousness or of dispositional efficiency than the more special. Hence, according to the law of part-efficiency, the former acted alone. If, then, distractions of attention take place; if the observer is restless or hurried; if he underestimates the difficulty of the task, then these factors, in reducing the degree of consciousness or the degree of dispositional efficiency of the complex for the total task, must increase the danger that only the more general, more strongly present, characteristics of the task make themselves felt in the ensuing process. On this hypothesis we can explain how it is that these special conditions just mentioned are instrumental in bringing about errors.

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[3] Selz, II, 1 f. Selz speaks of *Aufgabevertauschung*, *i.e.* task-substitution. Since we shall, however, have to make use of the word 'substitution' in a special sense, we prefer the term given in the text for purposes of our discussion.

[4] Selz, II, 499.

[5] Selz, II, 32.

[6] According to Selz, the anticipation of the solution forms a complex in the sense of the consciousness of a whole, including the consciousness of the relations between the parts. Independent parts of a task are those which, taken by themselves, would form a full task and which may drop out without the meaning or psychological efficiency of the other parts being affected. Dependent parts are those which alone would form no full task, or, if they do so, cannot be dropped without altering the sense of the rest. Selz, II, 2.

[7] Selz II, 10, 12. "Je geringer der Bewusstseinsgrad bzw. der dispositionelle Wirkungsgrad von Bestandstücken (selbständigen Teilen oder unselbständigen Momenten) eines Komplexes ist, desto eher bleiben diese Bestandstücke ohne Einfluss auf die Komplexwirkung, d.h., um so eher wirkt der Komplex so, als ob jene selbständigen oder unselbständigen Bestandstücke nicht vorhanden wären. . . . Das Gesetz der Teilwirksamkeit von Komplexen. . . . Die Teilwirksamkeit einer Aufgabe aber vollzieht sich nach denselben Gesetzmässigkeiten wie eine volle Aufgabenwirksamkeit, sie muss nur zu Fehlreaktionen führen, weil bei ihr nicht alle Bestandstücke der Aufgabe zur Geltung kommen."

We are translating Selz's *dispositionelle Wirkungsgrad* by "dispositional efficiency." Selz contrasts the degree of consciousness of the task with its dispositional efficiency. We take "dispositional efficiency of the task" as meaning the directive influence which the task exerts on the ensuing process of thought at a time when the task is *no longer* in consciousness.

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[8] Selz, II, 25.
[9] *Ibid.*, 10 f.
[10] *Ibid.*, 6.
[11] *Ibid.*, 12-3.
[12] *Ibid.*, 572.
[13] *Ibid.*, 35-36.
[14] *Ibid.*, 15.
[15] Selz's *Abstraktionsbereitschaft.*

> In so far as the partial efficiency of the task is a result of the higher degree of consciousness of the task's most general characteristics, this higher degree of consciousness is itself the result of the superior abstractive preparedness of the most general characteristics of an active meaning-complex. The consequence is that these characteristics tend to determine the ensuing process according to the law of part-efficiency. In so far as the difference between the more active general characteristics and the special characteristics of the task's meaning is not a difference of degree of consciousness, but only a difference of dispositional efficiency, we have to do with a special case of difference of reproductive preparedness. As in all cases of such differences between parts of a content with regard to which abstraction can take place, this difference of reproductive preparedness implies a corresponding difference of abstractive preparedness.[16]

For the present the foregoing statement of Selz's own theory may suffice. It follows closely (in translation) his own words. We may merely remark that the factors he advances, do, according to our view, play an important rôle in causing errors in thinking. The question is, however, whether they hold to the exclusion of factors acting according to the law of substitution—the validity of which is questioned by Selz. We now turn to the discussion of these further possible factors.

II

A second theory, suggested but rejected by Selz, of the explanation of errors in thinking may be obtained by the application of the law of substitution.

> Müller and Pilzecker formulate the law of substitution as follows. If an idea a arises in consciousness according to one or other of the laws of association, and if there is connected with this idea a tendency to reproduce the idea b, then this tendency may forthwith (*ohne weiteres*) be effective also for the arousal of the idea α which is only similar to a; the idea α can, so to speak, be substituted in the association ab for the similar idea a. The probability that α will reproduce b is greater, the more similar α is to a, the firmer the association is between a and b, and the less α is itself associated with any other ideas. In the same way a reproductive tendency attached to an ideational complex may forthwith be transferred to another ideational complex which is only similar to the first. A process of this nature is called active substitution. In the case of passive substitution the idea a does not reproduce an idea b which is actually associated with it, but reproduces an idea β which is similar to b, with which a, however, is apparently not associated.[17]
>
> In carrying out experiments on memory, Müller and Pilzecker came to the conclusion that a considerable portion of the errors in reproduction that they observed were due to substitutions falling under this law. Passive substitution occurred especially where the incorrect syllable was more familiar (*geläufig*) than the correct one, or had occurred recently in the experiments. Possibly in some cases of substitution the substituted syllable actually came to consciousness in uttering the correct syllable when the series of syllables was being read. On account of superior reproductive preparedness it would then, at testing, be reproduced instead of the cor-

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[16] Selz, II, 498 ff.

[17] G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtnis, *Zsch. f. Psychol.*, Erg. Bd. 1, 1900, 212-16. The law, as formulated by these writers, specifies the arousal of the reproducing idea according to one or other of the laws of association. This limitation is not essential for purposes of our discussion.

rect syllable. In some cases active and passive substitution occurred together, superior reproductive preparedness of the substituted syllable with which the reaction took place, playing a similar rôle as in the type of cases mentioned above.

If, now, syllables have been presented to a subject in a certain temporal and spatial order of which the subject has become aware, then the syllables would have become associated with each other in our dispositional knowledge[18] of them as standing in these relations to one another, *i.e.* as forming a relational whole. On the basis of this dispositional knowledge later reproduction can take place.

In attempting now, to reproduce the second of a pair of syllables, the first being given, we are carrying out a task-directed process of thought. The anticipation is primarily characterized as "the syllable [illegible] coming immediately after the syllable *a* in the series." According to Selz's third law of complex-completion, the basal condition is now fulfilled for bringing the content anticipated into consciousness. This law states that an undertaking (*Determination*) directed to the completion of a schematically anticipated complex is causal for the reproduction of the whole complex.[19]

An *S* may be attempting to reproduce the second of two syllables and one of its letters may perhaps be brought to consciousness at first. Should the dispositional knowledge for another syllable also containing this letter, *e.g.* in the same place, be in a state of sufficiently superior reproductive preparedness, then this syllable would be reproduced instead of the correct one. Cases of this nature are reported by Müller and Pilzecker.[20] Clearly the correspondence between the fuller anticipation, arrived at by means of the reproduction of the common letter, would be an important factor for bringing about the substitution. It is, however, not necessary that a common letter need be brought to consciousness at first for substitution to take place, since other points of agreement between anticipation and substituted syllable may suffice, *e.g.* that consisting of two consonants and a vowel. It is clear, however, that the degree of similarity between the substituting and the correct content is of importance for bringing about an incorrect reproduction. The correct content is characterized schematically in the anticipation. The greater the correspondence between these characteristics of the correct content and any other one for which dispositional knowledge exists, the greater the chances, other things being equal, for a substitution to take place. The probability that the anticipation will correspond, in part, at any rate, to another content will clearly increase with the increase in correspondence between the incorrect and the correct one.

We have up to now, however, adduced only one reason for the necessity of a certain degree of correspondence between anticipation and substituted content, namely that referring to the possibility of the *reproduction* of the latter. Another factor comes into play, however, as soon as the substitution is carried so far that the error is not *recognized*. If the reproduced content disagrees, for the *S*, with the characteristics definitely given to the correct one in the anticipation in such a way that this disagreement can be recognized, the reproduced content will not be looked upon as correct. Similarly, for the substituted syllable to be looked upon as correct presupposes that the two were, for the *S*, similar to one another. Furthermore, in so far as lack of associations between the substituted and other contents decreases the chances of knowledge about it being reproduced, which stands in disagreement with the anticipation, lack of such associations becomes a favorable condition for the non-recognition of errors. Should such further knowledge not be reproduced, then the substituted content may be looked upon as correct, or as possibly correct, partly on the basis of such criteria of recollective correctness as the promptness or clearness with which it is reproduced,[21] partly because it has not, as reproduced, any characteristics standing in definite disagreement with the anticipation, but especially because it stands in positive agreement with the anticipation in some respects.

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[18] Selz's *Wissensdispositionen*, I, 154.
[19] Selz, I, 128.
[20] *Op. cit.*, 215.

The fact, however, that a certain degree of correspondence must have existed between anticipation and substituted content does not imply that the anticipatory conditions were equally favorable for the substituted and the correct content. Whenever the substituted content possesses less agreement with the anticipation than the correct content does, then the anticipatory conditions must in themselves have been more favorable for the reproduction of the correct content. The reason for the incorrect reproduction in such cases cannot thus, be found only in the anticipatory conditions, and we have to accept the presence of another factor for bringing about the substitution, such as, *e.g.* a relatively high degree of reproductive preparedness of at least a part of the dispositional knowledge for the content substituted.

The view here developed with regard to the mechanism according to which passive substitution takes place, at the same time explains a peculiarity of some of the cases which Müller and Pilzecker adduce. It might appear remarkable that so small a degree of similarity between the correct and the substituted syllable should form the basis for the occurrence of the error. In itself indeed this, or even a much larger, degree of similarity would not suffice. There is necessary, in addition, a sufficiently superior degree of reproductive preparedness of the syllable reproduced to overcome the disadvantage due to the anticipatory conditions.

We have now to proceed to the examination of the meaning of the law of active substitution and of its relation to that of passive substitution. We presuppose that the syllables a' and b' have been associated with each other as standing in the relationship of succession r'. The syllable a'' has furthermore been associated with the syllable z as standing to it in the relationship r'. Should the task now be undertaken of recalling the syllable following on a'', the anticipation so characterised may lead to the reproduction of b'. If we formulate in this way the primary conditions for the occurrence of the phenomena subsumed by Müller and Pilzecker under the heading of active substitution, it is clear that the following conditions will be favorable to the reproduction of the incorrect syllable.

(a) Lack of reproductive preparedness of the content z itself which is associated with a'' as standing in the relation r' to it, and weakness of the association between a'' and z.

(b) Relative prominence in the anticipatory consciousness of those characteristics of a'' with regard to which it corresponds with a', so that the anticipation approaches (although, by definition, it does not coincide completely with) that anticipation which is directed to the recall of this syllable succeeding a'. Other things being equal, the chances of this happening increase with the increase in similarity between a' and a''. This second condition is that of a partial efficiency of the anticipation directed towards the reproduction of z.[22]

(c) Should the condition given under (b) be realised, then the strength of the association between a' and b' as standing in the relation r', and high reproductive preparedness of b' itself, will be further factors favorable for the reproduction of b'. The chances for this reproduction taking place will thus increase with the increase of the total dispositional preparedness accruing to b', which will in this case depend not only on the reproductive preparedness of b' itself, but also on the strength of its association with a'.[23]

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[21] G. E. Müller, Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufs, *Zsch. f. Psych.*, Erg. Bd. 8, 1915, 224-240.

[22] Cf., however, Section IV, below.

In the cases of active substitution different classes of cases falling under the same general type have to be distinguished. (1) a' and b' are associated with each other in the relation r'. So, too, a'' and b'' are associated with each other as standing in the same or in a similar relation to each other. We now set to work with an anticipation which we may symbolize by means of $\frac{r'}{a''\ \blacksquare}$ and which is directed towards the reproduction of b''. Should b' now be reproduced we should have to call this a process of active substitution, since it is a', and not a'', which is associated with b'. We could, however, equally well call it passive substitution from the point of view that b' has been substituted for the similar content b''. In this type of case, then, the substitution does not take place on the basis of a direct association between a'' and b', and the chances for substitution taking place will be favored by the degree to which the characteristics prominent in the anticipatory consciousness agree with those in the anticipation $\frac{r'}{a'\ \blacksquare}$ directed towards the reproduction of b'. (2) Müller and Pilzecker include under the cases of passive substitution those in which direct associations between the substituted syllable and the one presented at testing may have been in existence. We may now suppose that, in the generalized cases we are here discussing, a'' has actually been associated with b' as standing in the relation r'', this relation being similar to the one existing in the dispositional knowledge between a'' and b''. The factors favoring substitution will obviously here again be of the same general nature as those given for the other classes.

III

It is clear that such associational conditions as those just discussed may be expected to be of especial importance in carrying out the task of finding the opposite to a given stimulus-word. Many words have, of course, more than one opposite, taking the term in a broad sense. A word may, because of different sides or constituents of its meaning, allow of opposites in different directions, or there may be different degrees of opposition in the same direction. In other cases, again, one word may stand in opposition to another with regard to a side or constituent of the latter's meaning, the two otherwise agreeing to a very large extent. Moreover, if these words are in common use, it may safely be accepted that they have to a very large extent become associated with each other as standing in these different relations of opposition. On account of the relatively subtle differences often existing between the different meanings of opposition, and on account of the fact that there will be differences of total dispositional preparedness for the different opposites to the same word, we may expect the hypothesis, developed above, as to the rôle which substitution plays in causing errors of thought, to obtain verification by examining the processes of thought involved in finding the opposite to a given stimulus-word. In the present and following sections we attempt a verification of this hypothesis. The data for this part of our discussion consist, in the first place of the introspections of our *S*s, on processes by which they reached solutions unsatisfactory to themselves in attempting to find the 'opposite' to the stimulus words presented.

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[23] Generally expressed, we mean by 'dispositional preparedness' the readiness for response of the memory after-effets to the influence of factors, such as the undertaking of a thought-task tending to bring the contents corresponding to the memory after-effects to consciousness. The memory after-effects include the dispositions for the contents as such, as well as any associations, whether depending on direct temporary contiguity or not, between the dispositions for the contents. Should no such associations be in existence, the total dispositional preparedness becomes identical with the reproductive preparedness of the disposition itself.

Procedure. The stimulus-words were given orally. The reaction-times were measured by means of an ordinary stopwatch. If the *S* had not reacted at the end of 30 sec. at most, he was interrupted and asked to proceed at once with as full and accurate an introspective report as possible. Usually, however, the interruption took place sooner. In all other cases the introspective report took place immediately after reaction. For the greater part the introspective reports were registered by the *S* phonographically. In all other cases they were written down by *E* at a speed which would cause a minimum of disturbing delay to the *S*. Few questions were put, and these in a form calculated to make them as little suggestive as possible. Any part of a report forming the reply to a question is quoted in parentheses, remarks by the author inserted in the body of the protocol being put in brackets.

Subjects. Five *S*s took part in the experiments. They were H. F. Verwoerd (V), and J. C. Conradie (C), both of whom had had several years of experience in introspective work; J. Mommsen (M), and P. Stander (S), both of whom had had about one year's experience; and C. Oberholster (O) who had had only a few months training. The *S*s were unaware of the special use to which their reports were to be put.

The experiments were carried out, and the introspective reports given in the mother tongue of the *S*s, namely Afrikaans. Care has, of course, been taken to give the sense of the reports as exactly as possible in translation.

In order to discover the factors which caused the arousal of incorrect solutions, whether overt or not, we must examine the general nature of the processes which took place in arriving at any solution at all, and especially in arriving at a solution which was found satisfactory.

One means by which a solution was reached agrees in all essentials with the process analyzed in the previous section, where one syllable of a series, which had been repeated, is given with the presentation of the task following. For example, x and y are two concepts already known to the *S* as standing in the relation of being opposites. The opposite of x is now asked for, and the answer is obtained as the result of task-directed complex-completion on the basis of the existing dispositional knowledge. The reaction takes place sooner and with fewer intermediary processes according to the freshness and familiarity of this dispositional knowledge. The absence of intermediary processes in such cases means that the complex-completion is of a direct type, and it is only what may be expected when we find the introspective reports and the reactions of the *Ss*, showing a high degree of uniformity in these cases. More for the sake of comparison with the complicated cases to be discussed later on than for the sake of illustrating these somewhat obvious points, we give the set of protocols where the opposite to the stimulus-word "large" was required. The reaction-word and the reaction-time are given after the stimulus-word in each case.

(1) V. Large–small; 1.0 sec. I had the thought in consciousness of giving the opposite, but this idea was not so prominent as in the previous case. I waited for the stimulus-word with a little tension, and as soon as it was given, 'small' immediately came up with the consciousness that it was correct and the opposite of "large."

(2) C. Large–small; 1.4 sec. As soon as I heard the word 'large' I had a visual image of it, and immediately after that the word 'small' came to consciousness as if written after the first. I was conscious that I was quite correct and there was an attendant feeling of pleasantness.

(3) M. Large–small; 1.2 sec. Immediately after the stimulus-word I reacted with 'small.' (I was conscious that I was quite correct.)

(4) S. Large–small; 1.6 sec. When I heard the word 'large' I had a vague sort of recollection of the instructions given just before the stimulus-word was named, and without any processes of which I am now still conscious the word 'small' came up immediately.

(5) O. Large–small; 1.0 sec. The opposite 'small' came up immediately after the experimenter had said 'large.' I cannot recollect that I thought anything further.

In all these and similar cases, the *S* sets to work with the anticipatory schema for the opposite of the stimulus-word and the answer arises immediately on the basis of familiar dispositional knowledge. We wish now to give a series of examples of protocols of another type of frequent occurrence and then to proceed with a discussion of them.

(6) S. Nothing–everything; 3.2 sec. As soon as I heard 'nothing' I at once attempted to fix its meaning and then I tried to carry out the task. The word 'much'[14] came up first of all. I noted however that this was not the correct word for the meaning of the opposite. Then the idea occurred of "everything that exists" and I reacted with 'everything,' strongly conscious that it was correct.

(7) M. Nothing–something; 5.8 sec. When I heard 'nothing,' I at once thought of Hegel's nothing and then I recalled the task to consciousness. Then the word 'much' came up, but I was at once aware that it was not suitable as the opposite of 'nothing.' Then the word 'something' arose for the first time, and after this idea had occurred to me, I had the feeling that I was now almost quite correct. While I was introspecting, 'everything' came up in consciousness and I at once saw that this was a much more suitable word than 'something.'

(8) C. Nothing–much; 3.2 sec. After 'nothing' had been given 'much' immediately came up in my consciousness, but before reacting I thought whether it was absolutely the opposite of 'nothing.' I reacted after it appeared to me that there was not another word for me personally which I could place over against 'nothing' as its opposite.

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[14] The Afrikaans word used was *baie* which means 'much' or 'many.'

(9) C. Nothing–something; 1.8 sec. When the word 'nothing' was given I had a vague idea of a hat without a brim of which the crown was gone. I felt half inclined to laugh, but at once thought of my task of giving a word with the opposite meaning. A kind of image (I cannot quite say that it was one) of the word 'something' came to consciousness and at the same time I had a clear image of the word 'nothing' as written. (I had a strong feeling at the time of reaction that my answer was correct.)

(10) V. Nothing–something; 1.6 sec. I sat and waited for the word with the idea that it would again be one which would at once cause difficulty. When the word 'nothing' came, I did not think of 'opposite,' but concentrated altogether on 'nothing.' In connection with the stimulus-word there was a very vague impression (I cannot say whether it was in the form of an image or not) of something which is erased and in connection with this last again I reacted with 'something' before I had tested and without any impression whether it was correct or wrong. But just after I had reacted I had the idea that it must be correct, because something is erased so that the one must be the opposite of the other. I did not now think of the *word* 'opposite.' I thought of the *idea* by means of 'what lies in another direction.'

(11) O. Embarrassed–[no reaction; interrupted after 8 sec.] Immediately after the experimenter had given the stimulus-word, I had a visual image of a person who is embarrassed—somebody standing with his head bent forward. Then I thought what the opposite of 'embarrassed' is. After that the word 'cheerful' came up, but this did not satisfy me as the opposite of 'embarrassed.' The word 'joyful' also came up but this also did not satisfy me.

(12) C. Love of ostentation [Afr. *praalsug*]–humility; 10.6 sec. I was again put out when I heard the stimulus-word. It was as if I had an impossible case before me for which I would not be able to get the opposite. First of all I thought I might perhaps answer 'humility' but the answer was not to my mind a satisfactory one. I then again concentrated on the stimulus-word but could, so to speak, get no further. Finally I answered 'humility' with the consciousness that it was incorrect.

(13) V. Desirous of honor [Afr. *eergierig*]–without ambition; 12.6 sec. I did not note at all, this time, that the experimenter said 'opposite of,' and had no idea—or at any rate no clear one—in consciousness before the stimulus-word came of the meaning of 'opposite.' When the stimulus-word was given, it seemed to me that it would be difficult to give the opposite. At the time I was not thinking clearly that what must be given is the opposite. After the stimulus-word had been given I thought of 'humble' and submissive,' but these were not satisfactory, because they did not deny that desire or urge for honor which was implied by the stimulus-word. I searched for the opposite of this word for a time. This searching consisted of the stimulus-word coming to consciousness every now and again, and of my attempting to grasp its meaning. I thought of the meaning formulated in words, namely, desire for honor. Further, the searching consisted of trying to get another word which would be the opposite. The idea of the opposite meant for me 'without desire for honor.' I was not able to get any single word for this. The idea of ambition came to consciousness while I was going through the process of searching, just described. I became conscious, however, at one point that 'without ambition' would fulfil the requirements of my task. I was not thinking of opposite, however, at the time but rather that the words 'without ambition' meant what I was thinking I must also get 'without desire for honor.' There was a vague idea, while I was searching, of people who are without any hope in life and a vague image of a person who is bent and hopeless. When I was thinking about 'without ambition,' I was not so conscious of my desire to get a single word with that meaning. After I had reacted with

'without ambition,' I noticed that I had not got a single word and that I could rather have given 'unambitious.' Before I reacted, however—I should say by way of testing my answer—the stimulus-word came fleetingly to consciousness with the idea of seeking the opposite to it, as if the stimulus-word were colored with this idea, and this double idea was put into relation with 'without ambition.' This satisfied me.

The majority of the examples (6) to (13) are cases where the *S*s were not satisfied with solutions which occurred to them. Examples (8), (9) and (10), in which dissatisfaction was not so definite, are adduced for purposes of comparison. The first question we wish to consider is: By what psychological process or processes do solutions impress the *S* as not being acceptable or not fully acceptable? Several different types of cases have to be distinguished, of which one especially interests us here. It is characterized negatively by the fact that the task was not misunderstood, the solution being, on the contrary, anticipated in accordance with the task actually set. The discovery of the error does not depend on the discovery of a discrepancy between the two tasks. It is, furthermore, characterized negatively by the fact that no special process of testing, either resulting from a decision to test after the solution had offered itself or occurring without such decision, intervenes between the occurrence of the solution and the first consciousness of its inadequacy. This type of case is characterized positively by the fact that a solution offers itself which does not correspond with the anticipation in some respect, and that this non-correspondence comes to consciousness in accordance with the law of conflict. This law, accepted by us, runs as follows: If one conscious event takes place instead of another which has been expected (or anticipated), then the first as well as the point of conflict or difference, tends to be noted without especial direction of attention towards them.[25]

If, now, we have an anticipation agreeing with the task set as far as content is concerned, but so constituted in consciousness that one or more of the constituents of the anticipation do not have sufficient directive efficiency, then this constituent may still be present to a degree sufficient to bring about the recognition of the relation of discrepancy between the result arrived at and the one anticipated. Thus the negative judgment, with regard to the correspondence between the anticipation and the solution offered, becomes an indirect criterion for the presence of the anticipation of the corresponding positive relational content (*Sachverhältnis*) in these cases of task-directed processes of thought.[26]

We hold that some of the protocols from (6) to (13) are clear cases falling under the law of conflict. In (6) the solution

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[25] Selz's *Gesetz der Widerstreitsbeachtung*, I, 275; and II, 217.
[26] Selz, I, 270; and II, 217.

'much' is followed by the judgment that it is not correct as the opposite of 'nothing.' In (7) the *S* states that this awareness came at once after the solution 'much' had occurred. In (11) the occurrence of the solutions 'cheerful' and 'joyful' as the opposite of 'embarrassed' is followed by the consciousness of their not being satisfactory, and we find a similar state of affairs in (12) with regard to 'humility' as the opposite of 'love of ostentation,' except that the answer perhaps made the impression of being a doubtfully possible one before being looked upon more definitely as incorrect. In (13), too, the first pair of solutions ('humility' and 'submissive' as opposites to 'desirous of honor') are followed by the consciousness that they were not suitable as not having that characteristic for the *S* which attached to the opposite which he was seeking. Applying Selz's indirect criterion, we thus come to the conclusion that in these cases the solutions arrived at, but found unsatisfactory, did not correspond in some respects, at least, with what was actually anticipated.[27]

It is even possible to indicate in some cases the direction which the anticipation was taking before the rejected solution occurred. In order to show that this is the case we must explain certain characteristics often present in our protocols and exemplified in some of the cases adduced by us. According to the law of conflict, a point of discrepancy between an anticipation and a solution offering itself tends to be noted. This solution must then have occurred notwithstanding the fact that there was actually present in the anticipation a constituent in which it differed from the solution. This constituent of the anticipation was however not present clearly enough in consciousness, and so not efficient enough, to prevent the non-corresponding solution from arising. The rejection of the solution according to the law of conflict implies that this constituent must now have attained to some degree of attention, although not necessarily in a clearly analyzed form. This in its turn means that the anticipatory conditions have now been improved for attaining a solution which corresponds better with the original anticipation, since that part of it which was not at first present efficiently enough has now been brought to clearer consciousness. Hence the conditions are more favorable now for the occurrence of a fresh solution corresponding to the first in so far as the new anticipation agrees with the original anticipation, but differing from the first solution with regard to the point of discrepancy, if this latter attracted sufficiently clear attention at the moment of rejection.

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[27] It may be pointed out that, on our view, the law of conflict does not imply that the point of discrepancy between anticipation and solution arrived at should be so clear to the *S* that he is able to formulate it clearly. In view of the subtle differences in meaning often existing between different opposites to the same word a considerable amount of thought specially directed to deciding the point of difference is often necessary before it can be brought clearly enough to consciousness to be formulated. One would then be carrying out a process of conceptual analysis quite different from the processes taking place in the *S*'s mind in such experiments such as ours.

In this way we can explain one important type of case common in daily life, where a rejected solution, so far from having merely played the rôle of a hindrance, has also fulfilled the function of assisting us to get to the correct one. We formulate our conclusion by saying that, *if an improvement of an anticipation takes place, in one or other respect as a consequence of the rejection of a solution which occurs, and if another solution offers itself as a result, then the second solution provides a further indication, which is absent from the first solution, as to what the direction of the original anticipation was.*

Protocol (6) shows the characteristics required by the view just developed. The solution first occurring in trying to find the opposite of 'nothing' was 'much.' After its rejection the solution, with which *S* eventually reacted as satisfactory, occurred, without an intermediate process being mentioned, in the form of "everything that exists." In protocol (7) the first solution for the opposite of 'nothing' was 'much,' and after this had been rejected the solution 'something' offered itself, without an intermediary process being mentioned, as being almost quite correct. The process by which a third solution even more satisfactory to the *S* was obtained, in this case shows a peculiarity to the discussion of which we must return. The following protocol is another case in point.

> (14) M. Lazy–industrious; 2.0 sec. Immediately after I had heard the stimulus-word I had the feeling that the task was easier than in previous cases. There was a pleasant feeling, of medium intensity, also present. Then the word 'industrious' came up, and I at once had the feeling that this was the correct word with which to react. Before this, however, I had thought of 'flink,'[28] but I was at once conscious that this was not the opposite of 'lazy.'

The *S* here again, as in the cases of (6) and (7), first of all came on a solution lying in the same direction as the one eventually accepted, the comparative shortness of the reaction-time also showing that the second solution must have occurred very quickly after the rejection of the first. In (8), (9) and (10) no second solution offers itself. In the case of (11), two solutions, both of which are rejected, are mentioned, without the *S* stating that he found one better than the other. Even if he actually did not, this fact would not contradict the view developed above as to the function of the process of rejection. The original anticipation may be especially poor, so that a point of discrepancy does not come to consciousness clearly enough to cause a noticeable improvement in the consequent solution. We shall note later a special reason for believing that this was the case here. In the case of (12) the solution first occurring was indeed found unsatisfactory, but the *S* was unable to find a better one. Since the finding of a solution which is satisfactory to the *S* clearly does not depend only on the anticipatory conditions, this protocol also is not inconsistent with the view developed above as to the function of rejection. In the case of (13), the closely connected solutions "humble" and "submissive" were rejected as not fulfilling the requirements of a satisfactory solution in a particular respect. A further process of searching then took place in which this characteristic was a prominent part of the anticipation. This is in clear agreement with our view of the function of rejection.

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[28] This Afrikaans word, which means approximately 'active' or 'smart,' agrees partially n sound and spelling with the reaction-word *fluks* (industrious).

We have now reached the stage at which we can put our second and fundamental question: Why is it that a solution can occur which shows a discrepancy with the direction of the anticipation? Several of the protocols adduced provide indications in support of the view that a certain amount of task-displacement in Selz's sense took place. Thus, it may be urged that, in the case of (9), a distraction of attention from the task, leading to task displacement, was produced by the tendency to laugh and the contents causing this tendency. In the case of (11), previously referred to in connection with this point, the indication is especially clear. The *S* thought the meaning of the stimulus-word 'embarrassed,' at least in part, by means of an image of a person with head bent forward. This is an attitude characteristic of sadness or sorrow, and in agreement with this we find that "cheerful" and "joyful" now offer themselves when the *S* tries to carry out the task. In the case of (12) the *S* states that he was "put out." In the case of (13); the *S* reports that there was at least a lack of clearness, with regard to the meaning of 'opposite,' before the first, unsatisfactory, solutions occurred. In the case of (14), again, the *S* mentions a pleasant feeling of medium intensity as having arisen along with the impression that the task just given was an easy one. In addition, it may be said that particular characteristics as to the kind of opposite wanted were not present clearly enough in the anticipation, but that the abstract characteristics common to the different meanings of the word 'opposite' were unduly prominent. We thus have present a number of those factors which Selz advances as leading to task-displacement and partial efficiency of the task set. In (13), furthermore, there is an example of a type of occurrence, also present in other protocols than those adduced, namely, where a mistake is discovered, or a better solution offers itself, at, or just after, the moment of overt reaction. In this case the *S* finally gave "without ambition" as the opposite of the stimulus-word, and the protocol explicitly states that a particular characteristic of the solution sought, viz., that it should be a single word, had by now been in a measure lost sight of, although it had evidently been present in a higher degree at an earlier stage. The fact that the overt reaction is, as it seems to be, favorable for the discovery of a mistake and a subsequent improvement, is, we hold, due to a special concentration on the part of *S*, previous to reaction, on certain characteristics of the solution to be found, with the result that other characteristics are neglected. When this concentration relaxes at overt reaction, the neglected characteristics can more readily make their presence felt, *i.e.* the discrepancy may now be discovered according to the law of conflict. Possibly a somewhat similar factor may also be partly responsible for the belated "everything" as the opposite of 'nothing' in the case of (7). Still another answer is suggested by protocol (6). Here there are no positive indications, of the kind adduced above, that the *S*'s attention was distracted from the task. But it may be argued that there was a lack of clearness in his mind as to what was really required and that, consequently, a shifting of the task in the direction of a related one took place. Selz himself holds that the early discovery of an error, as was the case in (6), is an indication that a task-displacement of the above nature has occurred.[29] This, however, exhausts the list of cases adduced up to now where solutions occurred which were rejected or definitely found unsatisfactory by the *S* himself. The conclusion might, therefore, be drawn that the explanation of the errors is to be found among the anticipatory conditions.

As against this it must now be emphasized, according to the conclusions reached previously, that even admitting that the anticipations were not in all respects clear, still the anticipatory conditions themselves were more favorable for solutions other than those which occurred first of all and were rejected. Beside the anticipatory factors, there must, therefore, have been another factor, or other factors, present which favored the early occurrence of solutions appearing unsatisfactory to the subject according to the law of conflict. These must, moreover, have been present to such an extent as to more than compensate for relative disadvantage accruing to the rejected solution from the nature of the anticipation.

According to the analysis given in Section II we may expect such compensating factors to be present in the form of a high dispositional preparedness of the rejected solution. This expectation, following deductively from general principles, is corroborated by the examination of the cases adduced. In the case of (6) the solution first of all offering itself but rejected was "much." This was also the case in (7), and in (8) the *S* actually reacted with this word as the opposite of "nothing," though there are distinct indications in the protocol that he, too, did not find it completely satisfactory. In 3 of the 5 cases where our *S*s were given this stimulus-word we find the same error occurring. The two other protocols with the same stimulus-word show a similar uniformity, for in both (9) and (10) we find the *S*s reacting with "something." No process of previous rejection took place and the reaction-times are very short, corresponding closely to those of protocols (1)–(5), where the answer found satisfactory was arrived at by means of direct complex-completion on the basis of familiar dispositional knowledge. In addition, protocols (6)–(10) show essential agreement on this point, up to the moment where the first solution offered itself, with what is reported in (1)–(5), that is, this solution occurred very soon after the task as a whole had been undertaken. Certain kinds of opposition are evidently in concrete cases oftener used in daily life than others. This is clearly so with the pairs 'nothing–much' and 'nothing–something' as compared with 'nothing–everything.'[10] The fact that in the case of (10) the reaction took place before consciousness of the correctness or incorrectness of the solution had developed, is itself typical of a reaction occurring as the result of strong associations, since reactions then tend to become automatic. In the case of (12) the *S* reacted with the first solution occurring to him, namely "humility," although he did not find it satisfactory. Two of the other *S*s (M andO) also happened on the same, or essentially the same, solution as the opposite of the stimulus-word concerned.

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[29] Selz, II, 18.

(15) M. Love of ostentation (*praalsug*)–humility (*needrigheid*); 12.6 sec. After I had heard the stimulus-word I thought of ostentation (*praal*) and magnificence (*prag*) and of the well-known phrase in which these words occur. I then thought of the task given me, and then tried to react as quickly as possible. First of all the word 'modest' occurred and then the word 'humility,' with which I reacted with a vague idea that it might perhaps be correct.

(16) O. Love of ostentation–[no reaction; the *S* was interrupted after 15 sec.] I sought for the opposite of the word. Then a word like 'simple' came up first of all, but this did not satisfy me as the opposite of the stimulus-word. Then a word like 'humble' occurred which also did not satisfy me as the opposite of the stimulus-word. I then began thinking which of the two, 'simple' or 'humble,' would be best suited as opposite but I could not come to a decision.

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[10] The Afrikaans words *niks–alles* (nothing–everything) have not the similarity in sound and spelling which the English equivalents have and which would probably aid the reproduction of 'everything' by 'nothing.' In addition, the Afrikaans *baie* ('much' or 'many') because of its two meanings, both of which stand in a relation of opposition to 'nothing,' would for this reason be used more frequently as the opposite of *niks* than 'much' would be used as the opposite of 'nothing' in English.

Thus, in three of the five cases the *S*s happened upon essentially the same solution for the given stimulus-word. As in the cases of protocols (1)–(5) and (6)–(10), this uniformity is indicative of a relatively high state of reproductive, or dispositional preparedness of the solution, resulting from the concepts being used frequently in daily life. The *S*s, V and S, did not show a similar uniformity with the other *S*s in reacting to 'love of ostentation.' Similar preparedness is, however, indicated for V by protocol (13) where 'humble' formed the first rejected solution on another stimulus-word but related in meaning to 'love of ostentation;' furthermore, C also reacted with 'humble' on this last stimulus-word, the protocol giving, in addition, further evidence of the rôle which such preparedness plays with regard to the solution "modest," which occurred first of all but was rejected.

(17) C. Love of honor–humble; 13.2 sec. When the stimulus-word was given, I was at once put out altogether by the abstract conception. I then tried to think of the [*immediately*] preceding case of 'modesty' and its opposite, and I wanted to react at once with 'modest,' since it seemed to me that a modest person is one who is the opposite of one who is desirous of honor. I thought, however, that it did not seem to be a sufficient opposite and so I did not wish to answer at once. I waited quite a little time in a fairly unpleasant state of mind because it seemed to me that I must answer 'humble,' and again there was a moment of doubt. I felt that my answer was wrong and I gave it being quite uncertain with regard to its correctness or incorrectness.

It is true that in protocol (14) the solution which first occurred and then was rejected by the *S*, does not present itself in any of the other cases with the same stimulus-word, and in the case of protocol (11) there is a similar lack of uniformity with the solutions of the other *S*s, the protocol showing in addition clear indications of task-displacement as the result of poor anticipatory conditions. As in the cases already discussed this latter fact does not fully explain the arousal of the solutions rejected by *S*, and the absence of uniformity in reaction as an indication of the reproductive or dispositional preparedness of the answer does not necessarily imply the absence of such preparedness, since there are, naturally, individual differences with regard to the latter.

In some cases there are additional indications that the *S* had no satisfactory answer readily at his disposal, the result of the latter fact being, again, favorable for the occurrence of solutions running contrary to the anticipation. Such indications are the impression of difficulty which the *S* mentions, the length of the reaction-times and the fact that a special method is made use of by *S*.

This method contains the following as essential steps: The stimulus-word or concept(A), or one or other of the characteristics (B) of A, is first of all negatived, so that the *S* attains, *e.g.* to non-B. He then proceeds to search for an expression of a more positive nature or meaning (C) which contains non-B as constituent, since a *mere denial* is often not considered to be a satisfactory solution. If, now, non-B forms part of a previously existing dispositional knowledge of C, then C may itself be brought to consciousness on the basis of this knowledge. The recognition of a relation of opposition between A and C may now follow.

The following protocol is adduced as an illustration of this type of case.

(18) V. Love of ostentation (*praalsug*)–slovenly; 6.6 sec. There was a tendency present with me now to look for a word in the direction of not loving ostentation. This arose in conjunction with a very vague consciousness of opposite. When I had got to the idea of not loving ostentation, I was however no longer thinking of opposite but I was trying rather to get a word which meant that idea precisely. This searching consisted of keeping the meaning 'not loving ostentation' continually in consciousness and of waiting for another word which included this meaning to come up. While I was busy with this process of searching, the idea suddenly came up that it was not absolutely the opposite if some one merely did not love ostentation, but that he must also be dirty. I had this idea of dirty—which I think was imageless—in the form of thinking of a dirty person with slovenly clothes. There was no word in my consciousness but just this impression of dirty. I had this idea in consciousness for a short time, and then I had the impression of words like 'untidy,' 'slovenly,' or 'dirty.' The words did not come clearly to consciousness but there was just the impression that I knew a word in that direction. I can not describe it exactly. In conjunction with this consciousness the word 'slovenly' came up suddenly. I compared 'slovenly' with the stimulus-word by thinking of the two as far as possible at the same time and I thought of contrast and opposite. I saw that the contrast consists of the fact that the one includes everything that the other one does not include. Then I was satisfied.

The following protocols are further examples of this procedure, but the solutions run contrary in some respect to the anticipation directed towards obtaining a more positive result after the negation has been reached.

(19) M. Answer–[no reaction; interrupted after 20 sec.] As soon as I heard the stimulus-word, I saw that it would not be easy to get a suitable opposite in this case. I then had word-images of 'no answer' and of 'keeping silent' [*swyg*]. I was then conscious that 'answer' was a noun and 'keeping silent' a verb, and that it would thus not be possible to give the latter as opposite to 'answer.' Then the word 'silent' occurred, but it was at once clear to me that 'silent' would not be the correct word with which to react. Then I made vain attempts to get a suitable opposite at once. . . .

(20) V. Reason–reasonless; 5.0 sec. Immediately after the stimulus-word had been given I thought of 'unreason' [*onrede*], but this did not satisfy me, because it was, for me, a mere denial of 'reason.' I then thought that 'reason' could be taken in the sense of 'cause' and I then sought for a word for 'without cause.' I could not get such a word at once. Then I thought of 'reasonless' [*redeloos*]—I think in connection with the idea mentioned above of 'without reason,' though I am not certain. When

'reasonless' came to consciousness I was in doubt for a moment because it did not stand for the idea I wanted to give. It meant something in the direction of a person who is mad, whereas I wanted 'without reason' [in another sense]. I then thought, however, that 'reasonless' could be taken in two senses and that I could use the word as I wanted. There was just the idea present in passing that 'reasonless' was the denial of (but also meant the opposite *class* to) 'reason' and was in so far the opposite.

In these cases, then, we find present, first, the process of negation. In (19) the *S* attains to the negation "no answer" in seeking for the opposite of "answer" and in (20) to what is for the *S* a mere denial, namely "unreason," or "without reason." Then, in (19), in connection with "no answer" the *S* reaches "keeping silent" and "silent." In (20) "reasonless" offers itself. It has indeed, also a negative form but also, for *S*, a further positive meaning not agreeing with the anticipation. We may accept that 'reasonless' with the positive meaning attached did occur in connection with the negation 'without reason,' although the *S* was unable to recollect in giving his introspective report that such was certainly the case. In both (19) and (20) the recognition of the more positive form or content as not agreeing with what was to be obtained, evidently takes place according to the law of conflict.

As in previous cases, we may, therefore, regard protocols (19) and (20) as providing examples of the type of case where a satisfactory answer corresponding to the task and anticipation is either (a) not present at all in the form of dispositional knowledge, or (b) not present in a state of preparedness sufficiently high to prevent an unsatisfactory answer occurring.

Even granting that the anticipations were lacking in clearness in at least some respects, obviously the occurrence of non-corresponding solutions cannot be explained by this fact alone. Just as the solutions anticipated will be reached, as Selz points out, if the dispositional knowledge of them is sufficiently fresh and firm, even though the anticipatory conditions do lack clearness and prominence in all respects, so, too, solutions not corresponding to the anticipation will present themselves, if that part of the dispositional knowledge of them which does not correspond to the anticipation is sufficiently fresh or firm. These constituents of the dispositional knowledge will then act in the direction of *overcoming* the influence of the anticipatory conditions *already realized* in so far as these latter tend to act in another direction. We have thus to distinguish this type of factor from disturbances which weaken the anticipation itself *initially* and are thus also favorable to the occurrence of solutions not corresponding to the anticipation in all respects.[31]

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[31] Cf. Selz's statement that "Abweichungen der intellektuellen Prozesse von der schematischen Antizipation ihres Erfolgs können trotz ihrer richtunggebenden Funktion allerdings dann entstehen, wenn einzelne Bestandstücke der Antizipation, z. B. infolge von Störungsreizen, unwirksam bleiben. . . ." O. Selz, *Die Gesetze der Produktiven und Reproduktiven Geistestätigkeit*, 1924, 14-15.

We thus, reach the conclusion that the data advanced in this Section demand the assumption of the validity of the law of substitution in order to be satisfactorily explained. This result is in full agreement with that already obtained in Section II. We have, however, not yet examined Selz's objections against the law as stating a causal condition for the occurrence of errors in thinking. This will form the subject-matter of the following section.[32]

IV

As indicated in the first section, Selz also found cases in his experiments where solutions occurred which did not correspond to the anticipation. He holds, however, that this was due to the fact that the anticipation was not fully efficient, and maintains that his observations thus justify the hypothesis that in the case of task-directed processes at least, the so-called law of substitution is not valid for the relation of anticipation and result obtained, whether this result be of a reproductive or abstractive nature; or, should the law have validity, that it holds within such narrow limits of similarity that the differences of anticipation do not in general act as sources of error. If the anticipation is fully efficient, according to Selz, only those processes of reproduction (and abstraction) occur which correspond to the anticipation, and those which would correspond to a similar but partly deviating anticipation are absent. If, by way of exception, a result does occur which does not completely correspond to the anticipation, then this result is due to the fact that those constituents of the anticipation to which the result fails to correspond, had a lower degree of efficiency than those to which the result does correspond. For example, according to the law of part-efficiency, this might be occasioned by a reduced degree of consciousness or of dispositional efficiency, so that the anticipation acted as if the constituents to which the result does not correspond had not been present at all.[33]

The expression, "full efficiency or activity of the anticipation," may be taken in two senses, and, unless these are kept clearly apart, it might well appear that the law of substitution plays no part in the causation of errors of thought.

In the first sense, it may be used merely to indicate the *fact* that complete correspondence between anticipation and result exists. Where this is so, substitutional processes have by hypothesis, of course, played no rôle. This is naturally no argument against the validity of the law of substitution for the case of processes consequent on the undertaking of a task, unless it can be proved—which, of course, it cannot be—that all anticipations are fully efficient in the sense just given.

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[32] We need not discuss an objection which Selz raises (II, 376) against the law of substitution as an explanation of *another* phenomenon, viz., the fact that, if given tasks having the same general characteristics, we make use of operations of the same general nature. Even granting, as we do, that this objection is well founded, we may still consider the law a valid explanation for the type of case here under discussion.

[33] Selz, II, 409 f.

The second sense in which the expression "full efficiency of the task" can be taken is an *explanatory* one, namely, that all the constituents of the task were so clearly and strongly present, and thus so causally active, that a solution not corresponding to the anticipation in all respects was actually prevented from coming to consciousness and that a solution having this correspondence did occur.

It is clear that the sphere within which processes taking place according to the law of substitution can act will decrease, other things being equal, with increasing strength and clearness of the constituents of the anticipation. On the other hand, however, a decrease of clearness or strength of one or another anticipatory constituent will increase the sphere within which the law of substitution may act.

A similar ambiguity attaches to the expression "part-efficiency of the anticipation." It may, first, be used merely to indicate the *fact* of non-correspondence between anticipation and result. *All* cases of the result showing such non-correspondence are then cases of part-efficiency of the anticipation. But part-efficiency in this sense is then merely a *name* for the phenomenon of non-correspondence, just as full-efficiency in the first sense is then merely a *name* for the phenomenon of correspondence between anticipation and result. But it is just these phenomena of non-correspondence which have to be explained, and the law of substitution is an explanation offered. Part-efficiency in this sense and substitution are then not rival explanations. The acceptance of the one does not imply the non-acceptance of the other. We may, secondly, take part-efficiency in an explanatory sense. It would then mean that there are constituents of the anticipation which are not clearly and strongly enough present to prevent solutions not corresponding to the anticipation in all respects from coming to consciousness. For the latter case, again, we see that the sphere within which the law of substitution may act will vary inversely with the strength and clearness of the constituents of the anticipation. In short, if we take the expressions "full efficiency" and "part efficiency" as explanatory, and not merely as names for correspondence or non-correspondence between anticipation and result, we are not justified in setting the law of part-efficiency over against the law of substitution in such a way that the acceptance of the one implies the rejection of the other, or in holding that because part-efficiency of the anticipation can be shown to have been present, therefore substitutional processes are excluded.

Selz, furthermore, states that it is incomprehensible under the law of substitution why the idea a'', being similar to a', should arouse associations connected with a' only in the case of distracted attention, and not also in the case of full attention, and advances this as an argument against the law of substitution and in favor of an explanation by means of his law of part-efficiency.[14] We may reply that distraction of attention provides just those conditions of part-efficiency which are favorable for the occurrence of processes in accordance with the law of substitution.

How, then, does Selz explain the experimental results of Müller and Pilzecker which were presented in Section II? He holds that if a syllable has largely been forgotten, so that only those parts to which special attention has been given can be reproduced, then if another syllable should present itself which corresponds completely with this part, it may be looked upon by the subject as probably correct. It is, therefore, not the similarity of the two syllables which makes the substitution possible, but the complete correspondence with the correct one so far as it is still remembered. So far Selz's view is certainly to be accepted. We have ourselves followed it in the discussion in Section II. Such acceptance does not, however, prevent acceptance of the law of substitution, since it is clear that the valid portion of Selz's argument really explains only why errors arising *according to the law of substitution* are sometimes not recognized as errors. It is, furthermore, clear that the complete correspondence to which Selz refers, will occur, other things being equal, with a probability which increases as the degree of similarity between the syllables increases.

Another argument of Selz's against the validity of the law of substitution as an explanation of Müller's and Pilzecker's results appears on further examination to be an argument in its favor. He points out that the series of syllables were not thoroughly learned and holds that, if errors should arise at testing in this case according to the law of substitution, then such errors should also occur with a series which has been thoroughly learned, if the substituted syllable lacks strong associations of its own. The fact that such errors increased with lapse of time is viewed by him as pointing rather to the supposition that the cases of substitution occurring are not processes falling under the law of substitution but are due to the result of weakness of the memory after-effects (*Reproduktionsgrundlagen*).[35] As against this argument of Selz's, it must be pointed out that the weakness of the dispositional knowledge of two syllables which have been associated as standing in a certain relation, is merely a factor favoring the occurrence of mistakes according to the law of substitution. Weakness of the dispositional knowledge by means of which the correct answer would be obtained is not a rival explanation to that by means of the law of substitution, but is, on the contrary, a part of the latter.

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[14] Selz, II, 555.

Finally, Selz holds that, if the law of substitution were valid for a large sphere of similarity—and the examples of Müller and Pilzecker show similarity of only one letter—then we should expect mistakes to occur more frequently, *e.g.* when the *S* is required to give a rhyme to a nonsense-syllable.[36] We may reply that there are special conditions present in such a task as rhyming which prevent an incorrect reaction from being looked upon as correct, and that these conditions are not present in the same degree in the case of the reproduction of syllables, especially if these have been insufficiently or not recently repeated. In rhyming we have, in the stimulus-syllable, a content actually present or easily recalled, on the basis of which the incorrectness of a reaction, resulting from poor rhyming mechanism, may be recognized at once. In the reproduction of syllables such knowledge, by means of which the incorrectness can be recognized, is very often absent. In addition to this it appears to us that in rhyming the anticipation will on the whole be fuller than in the case of reproducing a syllable from a series which has been learned. The syllable which has to be matched with a rhyme is actually present at the stage of anticipation, so that the characteristics of the reaction to be made are comparatively fully given. This fact would also materially increase the chances against the occurrence, overtly or otherwise, of an incorrect reaction.

Our arguments in favor of the validity and sphere of application of the law of substitution do not, however, imply that processes taking place according to this law occur in all cases of errors of thinking. The type of case which we have examined and in which such processes play a rôle is characterized by the fact that *one solution is anticipated but that another offers itself on the basis of existing dispositional knowledge.* Errors do, however, also occur where the anticipation is at fault, either in the sense that some constituent of the problem whose solution should have been attempted was not present in the anticipation at all, or in the sense that additions were unwittingly made to this problem. In such cases substitutional errors, taking place after the anticipation has been formed, are not necessarily involved in the causation of the error.

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[35] Selz, II, 411 ff.
[36] Selz, II, 410. Cf. the discussion in Section II.

The individual cases adduced by us were characterized by the fact that the *S*s became aware of the non-correspondence between the anticipation and the result arrived at. However, not all thought-processes in which substitution takes place, need have this characteristic. Constituents of the anticipation may, indeed, conceivably have been present in consciousness, but, for example so slightly that they were not able to make their presence felt sufficiently for the non-correspondence between anticipation and solution to come to consciousness. In such cases processes of substitution would assist in bringing about undetected errors in thinking.

If there are dispositional factors exerting an influence running contrary to the direction of a constituent of an anticipation actually present, then the relative strength of these two kinds of factors will determine whether or not a result corresponding in all respects to the anticipation is reached. Competition between various directions of complex-completion is not limited, as Selz holds,[37] to those cases where each of the different completions corresponds to the schematic anticipation, but occurs also between directions of completion where the one does fully correspond to the anticipation and where the other does not do so. If one adduces, as Selz does, familiarity (*Geläufigkeit*) of the non-corresponding result as a condition making for errors, one implies the truth of the statement just made.

The examination of the meaning of the laws of part-efficiency and of substitution, of the data advanced in this essay, and of Selz's own arguments against the second of these laws, has led to the same result. Substitution and full efficiency of the anticipation are indeed opposed; not, however, as conflicting theories, but merely as causal factors, working in different directions. Part-efficiency of the anticipation, on the other hand, does not limit, but rather enlarges, the sphere of the processes taking place according to the law of substitution.

SUMMARY

(1) The problem treated is that of errors occurring in task-directed processes of thought, the particular point at issue being

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[37] "Entsprechen dem Komplexschema mehrere Komplexe, so findet allerdings eine Konkurrenz mehrerer Prozesse der Komplexergänzung statt. Diese Konkurrenz bewegt sich aber in dem engen Bereich der durch die schematische Antizipation festgelegten aufgabegemässen Reproduktionen. . . . ." O. Selz, *Die Gesetze der Produktiven und Reproduktiven Geistestätigkeit*, 13.

Selz's denial that processes take place in such cases according to the law of subsitution.

(2) After a statement of Selz's own theory and an analysis of the meaning of the law of substitution, experimentally obtained introspective data are advanced to prove that the validity of the law of substitution must be assumed in order to explain errors of thinking arising in a certain type of task-directed cognitive processes. This type of case is characterized by the fact that solutions occur for which the anticipatory conditions are relatively unfavorable since they do not agree with the anticipation. As against Selz it is shown that such facts make it necessary to assume the validity of the law of substitution as a basis of explanation, supplementary to that advanced by him, namely, partial efficiency of the *Aufgabe*.

(3) A number of arguments advanced by Selz against the law of substitution are then analyzed with a result in full agreement with that stated in the previous paragraph.

ATTENTION AND CLEARNESS IN THE PERCEPTION OF FIGURE AND GROUND[1]

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PERCEPTION AND ATTENTION

In the course of the numerous attempts in the history of psychology to weave the ever-growing masses of data into a comprehensive whole there have arisen many schools or systems; it is significant that these schools or systems have been measured very largely by virtue of their treatment of one problem, the problem of perception. Perception has presented, in this long course of system-building, a dire, entangling difficulty, a difficulty that is grounded in the general fact that whenever, according most strictly to the established principles of a system, there is wrought a description of a perceptual experience, the disturbing conviction nevertheless remains that something still is lacking, that a 'something' was present in the phenomenon that has not yet been caught up into the descriptive account. The conviction of this incompleteness of the perceptual record has led over, characteristically, to a theory of attention, or of something closely allied to attention, as some factor that 'selects,' or 'discriminates,' or 'points out' these particular aspects of the total environment for occupation of central place in the experience, and which shapes, imbues, or colors the experience in some manner.

As a result of this situation it has come almost to be the rule in traditional psychological systems to call frequently upon some theory of attention. Whenever, in the face of a demand for adequacy to a complete description, the perceptual account has found itself in sore straits, then the services of the attentional concept have been summoned. At least, such has been the claim, and the loudest bewailers of this condition of affairs go farther in maintaining—with more than a modicum of justice—that in the theory of attention has too readily been found the surcease of systematic sorrows, that the satisfaction gained from the completion of a description by invoking the attentional principle is too often premature and without substance. Such challenging of the use and validity of the concept of attention has proceeded in most emphatic formulation from the school of *Gestaltpsychologie*, a school that has found its beginnings in the theory of perception, and proposes to get along here, and in the remainder of its system as well, without recourse to any attentional theory, and proposes further—since it has no use for it, and perceives a danger in it—that attention with all its various appurtenances be unreservedly cast away.

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[1] From the Harvard Psychological Laboratory. To Professor Edwin G. Boring the writer wishes to acknowledge his deep indebtedness.

Although it is plain that the theory of attention which the *Gestalt*-psychologists have been concerned about is one particular theory, namely, that which has known its growth in the Wundtian tradition, yet the spokesmen of their doctrine have taken no care to deal discriminatingly in the matter, but have condemned the attentional concept in a fashion that is nothing if not whole-hearted. But one might very well take a little time to this question of attention as it relates to perceptual theory before placing a comprehensive taboo upon it, and seek to demonstrate cogent reason before condemning uncompromisingly all possible conceptions concerning it. This is a large order, of course, for theories of attention have been many. Since one must be compelled to limit the field, let there be selected for consideration in respect to perceptual theory a concept of attention that was framed in cognizant view of the difficulties and dangers in the field, a concept which sought to be based, not upon some hypothetical factor outside of experience, but upon discoverable aspects of experience itself. Such is the theory developed and defended by Titchener, and characterized by the contention that the clearness of the perceptual contents is the fundamental mark of attention as it plays its part in psychological phenomena.

Upon the envisagement of a problem-situation such as this—the relationship between the perceptual phenomenon and attention in one of its leading formulations—one must at once cast about for a suitable method of attack. Fortunately, in the present instance, a crucial point is immediately presented, since what has with some authority been maintained as the simplest type of perceptual experience has also been employed to demonstrate the fundamental nature of attention in the theory to be considered. This is the experience of figure and ground. In the *Gestalt-theorie* a figure standing upon its ground is held to be the most primitive of perceptual phenomena. Koffka, in a discussion of perceptual theory and the foundations of *Gestaltpsychologie*—the same in which he has inveighed against the attentional concept—has expanded this point in regarding the figure-ground experience as a simple, elementary *Gestalt*.[2] Titchener, some time ago, employed what is in essence the phenomenon of reversal of figure-ground fields to illustrate the fundamental aspects of attention. Here direct controversy enters, for Koffka made specific his general objection to attention by critical reference to Titchener's use of it in relation to figure-ground perception. The objection of Koffka carries the greater weight because it follows upon Rubin, who in his classical study of figure and ground voiced the opinion that theories of attention lend no assistance in accounting for this type of perceptual phenomena.[3] A consideration of Rubin's objections and criticisms can wait until after a careful examination of the theory of attention as clearness of conscious content.

This attempt of Titchener to employ attention as a descriptive concept with clearness as its cue is not without its historical background, for Wundt in particular has had a good deal to say about *Klarheit*, and a clear-obscure differentiation of the contents of consciousness has had at least casual mention by a great host of authorities. By the historical prominence of the clearness-theory Titchener considered his position strengthened: he pointed out that clearness is "the first thing that men lay hands on, when they begin to speak about attention."[4] "Clearness," he went on to explain, "varies between a liminal value and the maximum set by the limit of attentional concentration." It is one of the intensive attributes of sensation, the attribute that distinguishes the "focal" sensation from the "marginal." It is an independent attribute, moreover; in a measure it may vary independently of the other attributes.[5] All sensations and images are more or less clear. Feelings, however, lack this attribute of clearness: they cannot be attended to. Any given consciousness contains two levels of clearness, central and obscure: this is the "dual-level" formation of consciousness—and it is this formation that is found in correlation with the figure-ground differentiation, as is shown by the following illustration:

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[2] Kurt Koffka, Perception: an introduction to the *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 553-570.

[3] Edgar Rubin, *Visuell wahrgenommene Figuren*, 1921, 96-101.

[4] E. B. Titchener, *The Elementary Psychology of Feeling and Attention*, 1908, 171-188, esp. 186.

"Take one of the familiar picture puzzles, a picture which represents, we will say, a house and a garden, and somewhere in which is concealed the outline of a human face. As you search for the face, the contents of the whole picture are at the conscious focus. Suddenly you find it: and what happens? Why, as you do so, the picture drops clean away from the focus; the face stands out with all imaginable clearness, and the house and garden are no clearer than the feel of the paper between your fingers. . . . There is no poising of the picture, after the riddle has been read, midway between crest and base of the wave of consciousness."[6]

In his discussion of this theory of attention Titchener was careful to state that he was dealing with clearness of the attributive variety; such clearness must not be confused with *cognitive* clearness. A tachistoscopic field, *e.g.* may be observed under concentrated attention and thus have a very high degree of sensory or attributive clearness, but the observer may be unable to 'make out' anything at all. What he failed to do was to 'cognize.' Cognition is not attentional clearness, but "an associative process of the assimilative kind."[7] In a later statement Titchener made the distinction between the attributive and cognitive types of clearness even more explicit:

"We must, above all things, distinguish between attentional clearness and intrinsic clearness of definition,—sharpness, precision, cognitive clearness. A process may be transversing the very centre of consciousness, and therefore from the point of view of a psychology of attention may be maximally clear: yet it may be so weak, so brief, so instable, that its whole character is vague and indefinite."[8]

Titchener's distinction of sensory and cognitive clearness was given experimental confirmation in a study by Dallenbach, a study expressly devised to substantiate the concept of sensory clearness in the face of a severe attack by Britz.[9] In a tachistoscope Dallenbach exposed visual stimuli consisting of white cards on which were pasted 5 (in the second series, 6) colored rectangles, and the *Os* were instructed not only (a) to name and describe what they had seen, but also (b) "to give as exhaustive a description as possible of the processes involved during the course of the experiment, particular emphasis being placed upon the report of attributive clearness."[10] The first part of the instructions was expected to evoke a *cognitive* report, the second part a report on *process*. The task set by this dual instruction was difficult, and the *Os* found it trying to attempt to distinguish between attributive and cognitive clearness. In spite of this difficulty, and of other considerations which presently will be noted in detail, Dallenbach found his results to confirm his and Titchener's original position that sensory or attentional clearness is a descriptive affair to be distinguished from the clearness of cognition.

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[5] *Ibid.*, 219.

[6] *Ibid.*, 228-9.

[7] *Ibid.*, 238.

[8] *Experimental Psychology of the Thought Processes*, 1909, 17. It is worthy of note that this position that experience may be regarded in two different ways—from the point of view of description (sensory clearness) and from the point of view of cognition (cognitive clearness)—is but one phase of a duality that pervades Titchener's system, a duality of process and meaning, description and statement of meaning, and—as here mentioned—sensory and cognitive clearness. See his *Beginner's Psychology*, 1915; also Description *vs.* statement of meaning, this JOURNAL, 23, 1912, 165-182.

[9] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 183-230. *Cf.* C. A. Britz, *Eine theoretische und experimentelle Untersuchung über den psychologischen Begriff der Klarheit*, Saarlouis, 1913.

[10] Dallenbach, *op. cit.*, 187.

Rubin's objection to attention and clearness as pertaining to figure-ground perception was broad enough to include both the types of clearness defined above. He objected to the use of attention in Wundt's sense, and the argument is applicable in the same way to Titchener's attentive clearness (though Wundt's *Klarheit* was not so refined a term). A given field of an equivocal stimulus, after reversal so that it is perceived as figure, has not by that fact changed from a low to a high degree of clearness; it is now a particular experiential object, an object which before the reversal was not in consciousness at all. Or, in other words, the change of a field from ground to figure is not a change of a single object from low to high clearness-status: a field as figure is one thing, and the same field as ground is another. Rubin maintained further, in explicit criticism of Lehmann's use of attention as degree of cognition, that figure-ground reversal is in no better case in being explained as a change of cognition. If, with an equivocal stimulus, a field is at one time perceived as figure, and at another as ground, there is in the typical instance no resemblance whatever between the two experiences; the experiences can not be regarded as one affair with two phases differentiated by the degree in which they are cognized, simply because cognition characteristically indicates no relationship, no connection, between the two. If a stimulus has been, perhaps many times, perceived in one manner, and is later presented so as to be experienced with the fields reversed, it is not ordinarily recognized as having ever been seen before.[11]

This is significant criticism, but the question cannot be regarded as settled; it is possible enough that the bearing of clearness-relations upon perceptual experience is not crucially shown in the phenomenon of reversal of figure-ground fields. Rubin can be right in the contention that reversal is not to be explained primarily on the basis of an exchange of clearness-status, yet at the same time the clearness-differentiation of conscious contents in a simple perceptual experience (as, *e.g.* figure and ground) may be a very real and significant affair. The figural field may always, or nearly always, stand on a high plane of clearness as compared with the groundal field, yet in a reversal the situation may be so complex that the change of clearness-relations looms low in the total experience and therefore readily escapes any but the most careful scrutiny.

This whole situation points to the need of experimental investigation. There is needed, before any solution can be admitted, a more careful understanding of the nature of clearness. Furthermore, the problem should be reduced to its simplest terms. Instead of considering clearness-relations as they obtain in the perhaps rather complex matter of reversal of figure-ground fields, one should first investigate the comparative clearness of figural and of groundal fields as these are experienced in close contiguity. The primary question, and the question upon which the following experimental series are directed, is whether there is an experiential difference in the clearness of the two fields in figure-ground perception. A problem that bears immediately upon this one is that of the precise nature of clearness; one must know, for example, if there really are two kinds of clearness, which of them is pertinent to the figure-ground phenomenon, and how in this relation it is to be distinguished from the other. Incidentally, an investigation of clearness in its relation to figure-ground experience ought to be of some interest in regard to the dual-level theory of clearness: if in the first place a simple figure standing forth upon its ground can be shown to be the fundamental type of perceptual experience, and in the second place figure-ground status can be correlated with high and low degree of clearness, then it is apparent that the theory of the two levels is placed in a peculiarly significant position as the formulation of a fundamental differentiation of consciousness. The primary problems, then, that this experimental investigation will hold up for solution, are three in number: (1) the pertinence of attention as the clearness of experiential content to a simple perceptual phenomenon; (2) the nature of this clearness, with particular reference to the sensory-cognitive distinction; and (3) the validity of the so-called "law of the two levels."

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[11] Rubin, *loc. cit.*

I. CLEARNESS OF FIGURAL AND GROUNDAL FIELDS

Series 1: Relative judgments. It was desired in this series to present successively two stimuli which could beforehand be determined to be perceived each in a particular manner—with a given field as figure—so that the subject could be required to compare as to clearness the figural field in the one stimulus with the ground in the other, or the figural field in the one with the figural field in the other, or similarly the ground in the one with the ground in another. This was made possible by the fact, which had appeared in preliminary investigation, that all the *O*s, under passive instructions and under the instruction to attend to black, when presented with stimuli with one field black and the other white, got an overwhelming predominance of black figures. Therefore, with the quality of the figure determined, it was only necessary, in order to obtain the desired comparisons of figural and groundal fields, to control the judgment by position, and then to vary the combination of stimuli at will. This procedure will be clear upon an understanding of the form of the stimuli.

The stimuli were developed with view to equivocality. One of the factors in equivocality is equality of the areas of the two stimulus-fields. The stimuli consisted of white cards upon which were pasted black strips so that, when the cards were placed in the exposure-apparatus, the visible area was divided vertically into right and left fields, one black, the other white; in a central area, 1.5 x 1.5 cm., the line of separation underwent various convolutions. The visible stimulus-area was 3.7 cm. wide and 6 cm. high, and was framed by an intermediate gray 1.5 cm. in width. This gave a stimulus that could be perceived either as black protrusions on a white ground or as white extensions on a black ground.[12] Black occurred as often on the left as on the right. The first arrangement (black on left, white on right) is symbolized as 'Bk-W,' the second as 'W-Bk.' The preparation of the stimuli was standardized by a stencil which marked the general boundaries of the cutting, so that after the black strip was pasted on the white card the convolutions were centrally situated, and, typically, extended in about equal areas to the left and to the right of the central vertical axis. The convolutions of no two stimuli were the same. A stimulus was presented to a given *O* but once.

The stimuli were exposed in pairs in a form of the Dodge tachistoscope;[13] the instrument permitted successive presentation of two objects, and also control of the fixation-field. The time of exposure was regulated by means of a pendulum operating through contacts situated along the arc of its excursion. As sources of light, two 150-watt high-speed lamps were employed for the exposure-fields and a 75-watt Mazda lamp for the fixation-field. Intensities were equated by means of diaphragms. The fixation-field consisted of a light-gray field with a black vertical and a white horizontal cross-line.

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[12] Cf. Rubin's Figs. 2 and 13, in his *Visuell wahrgenommene Figuren.*

[13] See E. G. Wever, Figure and ground in the visual perception of form, this JOURNAL 38, 1927, 200 f. This instrument was a modification of that described and figured by H. R. De Silva, this JOURNAL, 37, 1926, 470-471.

At a signal *O* placed his head in the hood of the instrument and fixated the cross lines. The fixation-field was then removed, and 0.3 sec. later the first stimulus was exposed for 0.2 sec. After another 0.3 sec. interval the second stimulus was exposed for 0.2 sec. After 0.23 sec. the fixation-field was replaced.

The *Os* in the experiment were Mr. K. E. Zener (Z), Assistant in Psychology, Mr. J. G. Beebe-Center (*B*), Tutor in Psychology, and Dr. F. A. Pattie (*P*), National Research Fellow in Psychology. All were experienced observers.[14]

The subjects were instructed as follows:

"At the signal 'Ready,' fixate the point of intersection of the cross-lines. After the word 'Now,' will be presented a pair of stimuli. For each of these two stimuli your attention is to be concentrated upon the *black* stimulus-field.

"You are to make a relative judgment of the clearness of the *left-hand* fields. A report of 'much greater' (or 'greater') means that the clearness of the second left-hand field greatly exceeds the clearness of the first left-hand field. A report of 'slightly greater' means that the clearness of the second left-hand field is slightly greater than the clearness of the first left-hand field. A report of 'equal,' 'slightly less,' and 'much less' (or 'less') means that the clearness of the second left-hand field is equal, slightly less, or much less than the clearness of the first left-hand field.

"Clearness, often called sensory clearness, attributive clearness, attensity, and (sometimes) vividness, is to be taken as a designation of that property which distinguishes the focal mental process from the marginal. It is not to mean sharpness of outline, or distinctness of demarcation, or value of intensity-interval, unless (and only insofar as) these properties are inseparable."

The results are shown in Table I. There are two sets, A and B, of 100 judgments each, equally divided among the four arrangements of the comparison pairs: (1) Bk-W, W-Bk, (2) Bk-W, Bk-W, (3) W-Bk, W-Bk, and (4) W-Bk, Bk-W. Assuming that Bk was always figure, this gave clearness-comparisons of figure with ground, figure with figure, ground with ground, and ground with figure.

That black was figure was given careful checking. The *Os* were instructed that they were expected to get Bk as figure in all cases and were to report immediately whenever W appeared as figure in either or both of the presentations, at which the judgment was thrown out. In set A, *Z* reported 7 such cases, *B* and *P* none; in set B, *Z* reported 17, *B* 2, *P* none. Further, the clearness-series were always preceded by a short series in which the *O* was asked to report merely which field was figure and which was ground in the two cases. Counting these preliminary series, *Z* reported 25 W figures out of 311 pairs, *B* 5 out of 302, and *P* none out of 364. In spite of these precautions it is possible, though improbable, that one or two undetected cases occurred in which W was figure either wholly or in part.

The table shows for every *O* the actual number of judgments under each of the five categories for each set, as well as the

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[14] They had acquired special training in regard to the figure-ground phenomenon in a previous experiment. See Wever, *op. cit.*, 194-226.

TABLE I

Comparison fields	*O*	Set	Clearness of second field				
			'much greater' or 'greater'	'slightly greater'	'equal'	'slightly less'	'much less' or 'less'
(1) Bk-W (2) W-Bk	*B*	A				1	24
		B					25
		%				.02	.98
First field is Figure;	*Z*	A		1	1	10	13
		B				4	21
		%		.02	.02	.28	.68
Second field is Ground	*P*	A					25
		B			7	14	4
		%			.14	.28	.58
(1) Bk-W (2) Bk-W	*B*	A		2	12	11	
		B		6	17	2	
		%		.16	.58	.26	
Both fields are Figure	*Z*	A	1	4	14	7	
		B	.02	7	14	3	
		%	.02	.22	.56	.20	
	P	A			25		
		B			25		
		%			1.00		
(1) W-Bk (2) W-Bk	*B*	A		4	21		
		B		10	14	1	
		%		.28	.70	.02	
Both fields are Ground	*Z*	A		9	8	7	1
		B		15	9	1	
		%		.48	.34	.16	.02
	P	A			25		
		B			25		
		%			1.00		
(1) W-Bk (2) Bk-W	*B*	A	23	2			
		B	25				
		%	.96	.04			
First field is Ground;	*Z*	A	10	13	1	1	
		B	25				
		%	.70	.26	.02	.02	
Second field is Figure.	*P*	A	25				
		B	22	3			
		%	.94	.06			

percentages for both sets taken together. The 'slightly greater' and the 'slightly less' categories are very close to the 'equal;' these were inserted because it early became evident that some distinction must be made between the enormous differences of clearness experienced when one field was figure and the other was ground, and the minor differences when both were either figure or ground.[15]

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[15] There is evidence that *P* did not make this finer distinction. His 'slightly less,' *e.g.* is near the 'less' category.

The results show that for all *O*s a figural field is, in the great majority of cases, judged as more clear than a groundal field. In no instance was a figural field judged less clear than a groundal field. In only two instances was a figural field judged as slightly less clear than a groundal field, and in nine instances equally clear. Minor differences of clearness are shown when both fields were either figure or ground.

Frequent protocols were taken, and the *O*s were required at every session to characterize clearness and the judgment of clearness. Valuable data were obtained on the nature of 'sensory' clearness; they will be considered after the next series in the general discussion of clearness.

Series 2: Absolute judgments. The data of the first series showed plainly that the clearness of figural and of groundal fields is a reportable affair, and that the relative clearness of these fields follows a general tendency, namely, that of the higher clearness-status of figural fields; the protocols revealed further the pertinence of clearness-relations to an account of the figure-ground phenomenon and something of the precise nature of clearness itself.

This second series was arranged for a number of reasons. In the first place, it was desirable to inquire further into the matter of clearness; and in the second place, it is always of interest to compare with obtained data the results of a new procedure involving somewhat different stimuli and an altered manner of presentation and report. Moreover, it must be plain that the results of the first series show the clearness-relations of figural and groundal fields in general, and not in any specific case, because the fields compared were always of different, successively occurring experiences, and not of a single experience. From such data one can with safety argue that in general the clearness of a figure will be greater than that of its ground, but one can not decide whether every figure is clearer than its ground or whether the relation is a general but not a necessary one. For the determination of this point the method must be more direct.

The stimuli were of the same form as before, but were wholly altered in quality: here one field was blue, the other orange-red (symbols 'b' and 'o'). The stimulus cards were prepared by pasting a 'b' strip on an 'o' square, or an 'o' strip on a 'b' square, and affixing the square to a card of a form to fit the object-holder of the tachistoscope. Blue appeared equally often on the left and on the right. Milton-Bradley 'spectral' standard blue and orange-red colored papers were used. The chromas appeared the same; the blue was the darker.

The stimuli were presented singly in the instrument used for Series 1, but with one channel unused. The fixation-field and other conditions of exposure were controlled as before. Reports were taken in continuous fashion, with a pause of about 1 min. after every ten reports, thereby out-lining a 'group;' a rest-period of about 5 min. followed each set of four such groups. The stimuli presented and the 'attitudes' under which they were observed were haphazardly determined.

The instructions were as follows:

"Under a given attitude (announced by the experimenter) observe the presented stimulus, and report (*1*) whether the experience was 'o' figure, 'b' figure, 'o' mixture, 'b' mixture, 'o-b' alternation, 'b-o' alternation, 'o' with 'b' alternation in image, 'b' with 'o' alternation in image, both, or neither; and (*2*) make an absolute judgment of the clearness of each of the two fields according to the following scale: '9' maximally clear; '8' very clear; '7' clear; '6' fairly clear; '5' fair; '4' fairly obscure; '3' vague; '2' very vague; and '1' absolutely obscure."[16]

The first part of the instructions will bear some explanation. Preliminary investigation had indicated, and the first series had shown plainly, that figure-ground experiences are of many types, ranging all the way from the perception of a simple figure standing upon its ground to the perception of a mere flat pattern of qualities involving no figure-ground differentiation at all. Between these extremes stand the intermediates, or 'mixtures,' in which both objective fields possess the property of figure but one strongly or slightly predominates. A 'blue mixture,' for instance, is such an experience in which portions of both the 'b' and the 'o' fields enter into the figural experience, but the 'b' predominates; the ground is made up typically of the peripheral portions of both fields. At times a reversal occurs during the period of presentation, so that, *e.g.* an experience of 'b' figure on 'o' ground may give way to an experience of 'o' figure on 'b' ground ('b-o' alternation). Such alternation may take place in an image immediately following the presentation (when it is named a " 'b-o' alternation in image"). At times both 'b' and 'o' fields possess equally the property of figure (reported as 'both'); and, as just suggested, figure-ground differentiation may fail, giving a flat mosaic or pattern (reported as 'neither' or 'none').

The attitudes under which the stimuli were observed had been defined as follows:

"Unless before the presentation of a particular stimulus you are expressly instructed to the contrary, you are to (*1*) keep fixation central; (*2*) keep attention central; and (*3*) keep attitude (set, expectation) passive. You will be reminded frequently of this set of conditions by the words 'Passive Conditions.'

"Frequently, before a given presentation, the experimenter will modify the above instructions in one (or more) of the following ways:

"(1) (a) 'Fixation Left.' (Fixate the mid-point of the left horizontal line of the fixation-field, and maintain that position during the presentation.)

"(b) 'Fixation Right.' (Fixate the mid-point of the right horizontal line, and maintain that position.)

"(2) (a) 'Attention Left.' (Attend to the left-hand field.)

"(b) 'Attention Right.' (Attend to the right-hand field.)

"(3) (a) 'Left-Figure Attitude.' (Try to see as figure the left-hand field.)

"(b) 'Right-Figure Attitude.' (Try to see as figure the right-hand field.)

"(c) 'Black-Figure (or White-Figure, or any other quality of figure) Attitude.' (Try to see as figure the field of the quality named.)"

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[16] Adapted from L. R. Geissler, Measurement of attention, this JOURNAL, 20, 1909, 511.

The main function served by the various attitudes of observation was the prevention of the operation of a single *Einstellung*. If the *O* is left to his own devices, or if a single attitude is instructed for, he very soon is gripped by a most powerful determination through which all or nearly all of the presented stimuli are perceived with the same quality of field as figure, and everything that is gained in making the stimuli equivocal is thereby lost again.

The *O*s showed themselves able to make the desired judgment. They maintained, however, that while as *relative* indices of the clearness of fields within a given consciousness the judgments were reliable, the *absolute* values of the judgments of clearness were not of great certainty. Therefore the data have been regarded merely in the relative sense. The results from the presentation to each *O* of 40 groups of stimuli are shown in Table II. Alternations and the occasional failures do not appear in the table.

TABLE II

Field clearer	*O*	Nature of the experience and direction of attitude														
		'b' figure			'b' mixture			both or none			'o' mixture			'o' figure		
		b	*no*	*o*	*b*	*no*	*o*	*b*	*no*	*o*	*b*	*no*	*o*	*b*	*no*	*o*
'b'	*B*	75	50	80	25	12	10	1	1	1	0	0	1	0	0	0
	P	78	78	24	7	6	2	1	2	2	0	0	0	0	0	0
	Z	18	2	1	0	0	0	1	0	0	0	0	0	0	0	0
'b' and 'o' equal	*B*	2	3	7	6	7	8	1	5	3	1	1	0	0	1	0
	P	14	22	51	3	7	6	11	20	11	2	0	0	0	1	0
	Z	3	0	0	1	0	0	3	0	2	0	0	0	0	0	2
'o'	*B*	0	0	1	1	11	5	2	4	1	8	4	5	13	0	9
	P	0	0	9	0	0	4	0	6	10	0	2	4	1	0	0
	Z	1	0	0	0	0	3	3	0	2	5	1	1	85	95	131

The first row of the table shows the nature of the experience as reported. The second row indicates the field toward which the attitude had been directed: *b* indicates that the attitude was directed toward the blue, *o* that it was toward the orange-red, and *no* that attitude was passive; aside from this no differentiation is here made between the various attitudes of observation. The remainder of the table shows, for each *O*, the actual number of cases, for every kind of experience, that the 'b' field was more clear, that the fields were equally clear, and that the 'o' field was more clear. The 'none' cases were infrequent, and since they showed as to clearness no differences from 'both' cases, they were thrown in with the latter.

The table shows plainly a prevailing tendency for the figural field to be judged as more clear. As the first and last divisions of the table show, the groundal field was more clear in but 11 cases out of 927, of which 10 occurred when the attitude had been directed to the field which was experienced as ground.

Cases of equal clearness were more frequent, especially where the field experienced as ground was that toward which the attitude had been directed. In the experiences of 'b mixture' and 'o mixture' the prevailing tendency was for the clearness to be greater for that field reported as dominant in the figural experience. The distribution of clearness in the 'both' and 'none' cases was fairly even, with a predominance of cases in which both fields were of the same clearness.

TABLE III

O	Probability that		
	figúral field is more clear	the fields are equally clear	groundal field is more clear
B	.9419	.0539	.0041
P	.6511	.3166	.0324
Z	.9822	.0148	.0030

The next table (Table III) is computed from the data in the first and last divisions of Table II. It gives the probability,[17] for each *O*, that the figural field is judged more clear, equally clear, or less clear than the groundal field. The probability is high that a figural field occupies a clearness-level higher than that of its ground. There is a small probability that figural and groundal fields occupy the same level of clearness. The probability that a groundal field is clearer than the figure with which it occurs is extremely slight.

The general tendency shown by these results is obvious enough, but the situation must not be made to seem too simple; certain facts of the experimental setting must be considered. The various figural experiences do not come already catalogued to the *O*, do not fall into discrete groups, but vary by innumerable gradations from a most definite experience of unified clear figure standing forth from undifferentiated ground to the experience of a plane pattern. Between stand the 'mixtures,' in which portions of both fields have figural properties, 'mixtures' ranging from a type in which one quality definitely dominates to one in which both qualities are of like figural value. Frequently two 'figures' are inextricably interwoven, several levels of localization are present, and the various parts show differences in the area of the experience which they involve. Difficult, therefore, is the task of the *O* in reporting his experiences in any arbitrary, necessarily limited, set of categories. When called upon to report the clearness of 'the two fields,' in all except the pure figure-ground experiences he is in a difficult position. In the cases of intermediates there are not two fields frequently, but three or four or five different fields. The *O* in this latter situation may do one of two things: he may give a gross judgment of the 'average' clearness of the 'o' and 'b' areas, or he may designate in detail the clearness of the various parts. In the above experiment the *O*s were allowed to follow their own inclinations in this matter: *P* gave 'average' judgments, *B* and, to a somewhat less extent, *Z*, reported upon the clearness of the separate parts, usually designating the principal fields.

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[17] The 'empirical' probability, computed by the formula, a/(a +b), where a = the number of times the event occurred, and b = the number of times it failed.

This produced a minor difficulty in the preparation of Table II, for it was necessary to form for the record in the table a rough numerical average of the clearness of the separate areas reported. The difficulty is present because it is questionable whether this does justice to the data; but it is a minor difficulty because cases of this kind were relatively few, and the alteration of the data by their complete omission would be hardly detectable.[18]

A more serious difficulty in the experiment arises from two considerations. In the first place, it seems that in general, but in the intermediates particularly, the portion of the groundal field adjacent to the figure is perceived as more clear than the peripheral portion. In the second place, there was shown a constant tendency for the more clear portion of the groundal field to be the cue for the judgment of groundal clearness, while the less clear portion failed to enter, either separately or in the gross, into the determination of the judgment. These things, taken concurrently, mean that there exists a general tendency, over a number of cases, for the groundal field to be judged as having a higher clearness-status than it would have if its periphery were involved. The situation is simply that the periphery in many cases is not such as to elicit report. The effect of this tendency upon the data, if it has any effect at all, is to reduce the frequency in which the clearness of figures is found to be greater than that of their grounds. Since this frequency has been shown to be very great under even the highly equivocal conditions of this experiment, it follows that such a tendency certainly has not been operating with any great effectiveness; yet these data are not such as to remove the opportunity of arguing that the failure to evince an invariable relation between clearness-values and figure-ground status is due to the operation of some such factor. If this were the case, however, one would expect to discover evidence of the operation of the factor in the protocols; nevertheless an examination of the protocols revealed no such evidence—in fact, it showed evidence which substantiates the data at their face value. There were convincing cases, few in number to be sure, but, it would seem, none the less crucial, in which the groundal field was reported as clearer than the figure, yet the report showed the groundal field as a whole to function for the report. Such a case is the following, in which an experience was reported as a 'b' figure on an 'o' ground, yet with 'o' on a higher level of clearness:

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[18] Of really equivocal cases there were but 8, so that the results can have been subjected to but little violence by this arbitrary averaging procedure. It will be recalled that *P* did his own 'averaging;' with this *O* the difficulty is the same, though not so well known.

"Blue figure; 'o' clearer. The 'o' seemed to take up the main part of the field. My attention was on the 'o.' The 'b' outline was very indefinite. The figure was sort of jagged, and it lay above the 'o.' " (*P*).

It would seem, then, from the evidence, that the relation that has been shown between clearness-degree and figure-ground status in a perception is merely what it has been called in this report, namely, a general tendency, and not an invariable relation. This merely adds weight to the contention that in any complete account of figure-ground phenomena the clearness-relations must be set forth; if the variation of clearness were univocally dependent upon figural or groundal status it might be neglected in a description without serious consequences, since for any field or part-field it would be necessarily involved in the assignment of figure-ground status. However, as here is shown, the probability is that such is not the case, and one therefore to be on the safe side of adequacy must include in a description of figure and ground a report on clearness-relations.

Extracts from the protocols of both *Series 1* and *2* will serve to illustrate this point of the pertinence of attentional status to an account of figure-ground phenomena and to indicate as well something of the nature of the various possible figure-ground experiences.

(*a*) *The double-field figure.*

" 'Both.' An experience of two 'b' blobs, fairly far apart, on an 'o' background. They stood out. Then one 'o' blob on a 'b' background; that stood out" (*B*).

" 'Both.' There were two groups, one of Bk into W, and one right below it, W into Bk, and the two prongs both seemed to overlay the other fields. The big blotches of Bk and W seemingly were on the same plane. No predominating quality" (*P*).

" 'Both.' The thing seemed to be two members of one pattern. The contour seemed to belong as much to one as to another. It looked like a matter of the fingers interlocking, not interlying. The 'o' was clearest" (*P*).

" 'Both.' The clearness was exactly the same. The attention was greatest along the contour. There was a little prong of 'o' into the 'b.' That was the clearest thing in the field" (*P*).

" 'Both.' W fingerlings and Bk; both distinctly figure" (*Z*).

(*b*) *The experience of pattern.*

" 'None.' The experience was of a mosaic of 'b' and 'o.' The central part of the experience was more clear and more outlined. But aside from that there was no distinction of fields. A plane, in which the different portions were distinguished by color but not by spatial position with reference to that plane" (*B*).

" 'None.' Simply a bunch of 'o' and 'b' striations across the field, the 'o' being thicker. The clearness of the whole central area was rather high. The 'o' was slightly brighter. There wasn't any definite contour. Neither was definitely a unit" (*Z*).

" 'None.' The field consisted of 'o' and 'b' strips; rather unstable; no definite contour bounding the thing. Neither one came out as figure. The 'o' was brighter than the 'b.' It seemed to possess slight figure-quality, but no definite contour developed. Definitely on the same plane of localization. The main thing is lack of definite contour" (*Z*).

(*c*) *The intermediate experience.*

" 'b mixture.' The 'b' experience was central. There was a 'b' bump which was central and stood over everything else except a little 'o' blob that stood out terrifically over everything. The rest of the 'o' vanished more or less" (*B*).

" 'b' mixture.' An experience of two 'b' tongues standing out against an 'o' background, one in the upper, one in the lower left quadrant. The 'o' blob stood out against the 'b' background in the middle of the left half of the field. The 'standing-outedness' of the 'b' was greater where it stood out than the 'standing-outedness' of the 'o' where it stood out" (*B*).

" 'b mixture.' Both fields seemed to have figure-quality, but the 'b' overlapped the 'o,' especially with respect to one long prong which seemed to stand out very much—'b' seemed to have the upper hand" (*P*).

" 'o mixture.' General experience of 'o' on the right, of 'b' on the left. The most figured thing was a little 'o' blob slightly below the horizontal cross-line. It stood out from everything else; was very sharply contoured. Above that was a little 'b' blob which stood out from the rest of the 'o' and the rest of the 'b.' It was figure too" (*B*).

" 'o mixture.' There the 'o' was clearer and appeared localized a little bit ahead of the 'b,' yet the 'b' seemed still to be figure. On the extreme left some of the 'b' was not seen; seemed shady, fragmentary. The contour was good" (*P*).

" 'Bk mixture.' Central, in the middle of the field and in attention, was an experience of a Bk blob distinctly superposed on a W background. To the left were two smaller blobs of W, superposed over Bk" (*B*).

" 'Bk mixture.' The W took some time to come up to the clearness that it finally had. I saw the W localized a little bit in front of the Bk. The Bk didn't seem to extend behind the W. I had a sort of simultaneous apprehension of the contour of the W and the contour of the Bk" (*P*).

" 'W mixture.' Central was a W tongue on a blackish background. Below that was a Bk tongue sticking over in front of the W background. The W was central and more impressive" (*B*).

(*d*) *The experience of simple figure.*

" 'b figure.' There was a 'b' blob that was the central thing of the whole experience. That stood out on a background of 'o,' and graduated out on a background of 'b' " (*B*).

" 'b figure.' The 'b' field wasn't half the field; wasn't localized very well in front of the 'o.' The 'o,' probably because of larger expanse, was on the same level of clearness as 'b.' The figure was rather small, pressed over toward the right half. It extended from the side to two-thirds the distance to the half-way line" (*P*).

" 'o figure.' There were three levels: the highest level of clear[illegible] was a little 'o' blob on a 'b' background. The 'b' blob itself was on an [illegible] background for the rest of the experience" (*B*).

" 'o figure.' The 'o' figure uniform, continuously contoured. Differences in localization between figure and ground. Considerable difference in intensity: figure brighter. The figure was surfaced, though rather voluminous rather than glassy. The ground was more filmy; unclear" (*Z*).

" 'o figure.' The 'o' part figure; 'b' ground. The texture of the two was not very different. Very slight localization of 'o' in front of 'b.' The 'o' seemed to be contoured, while the 'b' wasn't; 'o' was more definitely a compact unit—that was the main thing. Brightness in each field was even; 'o' was more *eindringlich*. It was not a particularly good figure" (*Z*).

" 'Bk figure.' A rather filmy whitish experience on the left; on the right a Bk, definitely localized experience with little blobs sticking out into the W. The W was both filmy and translucent. There was a certain depth, not thickness" (*B*).

" 'Bk figure.' Bk figure on the right; perfectly definite contour. On the left was nothing but 'fuzz'—*i.e.* ground—not localized." (*B*).

" 'W figure.' Figure was W; was on the right; and clearest in attention. Bk was relatively unclear" (*P*).

" 'W figure.' W figure with jagged edges. A deep Bk inlet on the main part of the figure seemed almost part of the figure. There was a jagged W in front of the Bk, and then this Bk inlet over the W" (*Z*).

(*e*) *The experience of alternation.*

" 'b-o alternation.' The 'b' seemed just for a moment to be figure, but the 'o' swelled up and the 'b' retired in space. The end of the experience was a distinct 'o' figure, with 'b' in the background" (*P*).

" 'Bk-W alternation.' Experience of a Bk object, or surface, with some recessions, and in that recession some pseudopodia over a W background. Background not focal in any sense. Then some of the W between the pseudopodia seemed to overlap the pseudopodia. The rest of the Bk area remained figure, except the pseudopodia that seemed to recede while the W came forward" (*B*).

" 'Bk-W alternation.' The Bk seemed to be figure for just a moment, then the W, especially where it jutted into the Bk, seemed very prominent, and the Bk seemed to drop back to the background" (*P*).

" 'Bk-W alternations.' The Bk figure consisted of the protruding Bk portion; it was quite circumscribed. It had nothing to do with the rest of the field, was of surface-character; the ground was film. Indefinite localization, but quite uniform texture. Then the part of the W field immediately below this Bk portion assumed surface-texture and became a good deal brighter. In both parts both Bk and W stood out. Part of the W field became figure. I don't know what happened to the contour along that portion that belonged to the Bk and W portion" (*Z*).

" 'W-Bk alternation.' The W figure was very brief and not complete; a certain W protrusion was brighter than any other part of the W—it was the clearest part of the field. Then a Bk finger differentiated itself and came out, had surface-texture, was continuously contoured; and the W seemed to recede and grow more film-like" (*Z*).

Alternations were reported by all *O*s; in most cases the *O* was able to report the clearness of the fields in both stages of the experience. The most frequent failures in such reports were for the groundal fields. The general tendency was for the clearness of the second figural field to be greater than that of the first figural field. This relation held also for the groundal fields, in the same order. Without exception, the figural field for each stage was reported as clearer than its ground, or the clearness of the ground could not be reported at all.

An intimate relationship has been demonstrated between figure-ground differentiation and the clearness-degrees of the visual areas. The results of the two series are in agreement. A field which is perceived as figure stands in general on a clearness-level higher than that of a simultaneously or successively perceived ground. In an intermediate experience portions of both fields are likely to be figure, of a high, and approximately equal, degree of clearness, while the ground consists of the remaining areas of the two fields and is on a lower level of clearness. Reversal of ambiguous stimulus-fields is found to be accompanied by a reversal of the order of clearness-levels. Figure and ground are thus respectively correlated with higher and lower degrees of clearness.

II. Sensory and Cognitive Clearness

It is time, after so much has been said about clearness and its pertinence to the figure-ground problem, for something specific to be recorded as to the nature of clearness itself. Since it is of apparent necessity to know what is the clearness that one is dealing with, let the first approach be an examination of the sensory-cognitive distinction for clearness.

In this experiment there was found no intelligible distinction of sensory and cognitive clearness. As the instructions show, the *O*s were expressly warned against a confusion of these supposedly existing affairs, and all the presumption was in favor of a distinction, but nevertheless a distinction failed to appear. The conclusion from the results is that in this experiment, and so far as the figure-ground phenomenon is concerned, the differentiation of attributive and cognitive clearness does not exist. The clearness obtained in this experiment is no more what has been called "sensory" than it is what has been termed "cognitive;" it is simply that property which distinguishes focal and marginal mental processes, a property which can be reported by an observer as a portion of a figure-ground experience, and a property which correlates highly, but, according to the evidence, not absolutely, with figure-ground status in a perception.

The nature of this clearness is shown in detail in the reports of the *O*s. The *O*s had very frequently been asked, during the preceding series, to deliver introspective reports as to the nature of clearness and its pertinence to the experiences judged. Their remarks are recorded in detail:

B: "For me at present, clearness is definite value with respect to reportable attributes. The greater the number of such attributes that have a value in the experience, the greater the clearness. I might say that it is 'figurehood,' but I am wary of that as 'blanketing.' It is a definite experience that I can place in the scale of hue, intensity, etc. If it isn't clear I can only roughly localize it; its hue is vague; its brilliance is variable; homogeneity may also come in. It is *definite* things; you can only approximately describe the ground." "The field that is more clear is more brilliant, has some characteristics of texture, etc. An unclear field is 'fuzzy.' An unclear field is on the fringe of the picture. The clear field I can say something about." "That object is more clear which is the spatial reference-point in the experience."

Z: "The more clear field has more structure in it." "Clearness is the presence of something that can be reported upon." "The Bk was not clear; it was only vaguely present. I don't know that you can say very much about it. I can't say very much about its intensity, because it wasn't in consciousness in a form in which a thing like intensity would be very well represented: it couldn't be reported upon. The farthest, *i.e.* peripheral, ground doesn't seem to be in consciousness at all. The ground near the figure seems to be more clear. The clearness of the figure is uniform to a large extent in from the contour. The ground *near* the contour has a *poor* clearness; that portion away is almost lacking in clearness. The determination for the W seems almost to eliminate the Bk. The only thing that stands out, and can be reported upon, is the W. The Bk is non-existent. Lack of clearness is inability to report." "This clearness-judgment seems a sort of secondary judgment. It is certainly not the same thing as a judgment of intensity; it is a little bit farther removed from the actual properties of the experience as such. It is somehow connected up with the ability to report, with being able to hold the thing and to give a report upon it after it is past."

P: "I could draw the Bk [Bk is figure], but not the indentations of the W, so I call the figure clearest." "The figure is always clearest. . . . . The only thing I could draw would be the Bk part. To draw the W I'd have to sit down and think what was the Bk. That's what I mean by clearness." "Clearness is something like vividness. My attention is caught by the Bk figure always. The thing looks more vivid, more prominent. W doesn't amount to anything." "The Bk was on the right side; Bk was figure, was more clear. My eyes and attention were attracted by Bk. W had no particular quality at all. I see what was meant by the thing-quality of figure. It is as good an experience of clearness as you get anywhere." "Bk had projections somewhat in front of the ground. Bk was figure. I had no knowledge whatever of the projections of the W. I couldn't draw the W; I would have to draw the Bk. Bk was figure and Bk was clearer."

At the end of *Series 2*, the *O*s were requested to give an introspective characterization of clearness. Their accounts are appended:

B: "The characteristics of clearness are *centrality in the field of experience* and *memory-value*. That is to say, that portion of the field which is clear has existentiality in memory-image. Those are the only characteristics of clearness, but there are several characteristics of clear objects: intensity; contoured-ness; localization is space, especially depth; and heterogeneity relative to the rest of the field—large qualitative difference, which may correlate with contoured-ness."

Z: "A thing is clear in proportion as it can be reported upon. If it is indistinct, very little can be said about it." "The thing stands out in the experience—it has figure-quality, it is the important thing in the experience. . . . In the experiential field one part of the field will be distinct, and will seem to stand out more, is more *eindringlich*, like the focus as opposed to the margin."

P: "In that last one the judgment ['o' more clear] meant that I simply saw the 'o' and the 'o' took up most of consciousness; 'b' was less vivid." "The thing just seemed to be the most important."

The focally clear portion of a visual experience is, in effect, that part of the field which stands forth in the experience, which obtrudes itself upon consciousness, which is distinct and prominent above the undifferentiated background, which can be pointed to, grasped, described, reported definitely upon, which persists in memory. The marginally clear portion, on the other hand, is recessive, subordinate, it slips from one's grasp, eludes description, can be reported but hazily, is soon lost to memory. If in the above description for 'focally clear portion' and 'marginally clear portion' one writes 'figure' and 'ground,' the general features of figure-ground status will be set forth. Here is no mention of cognitive clearness. This is but one kind of clearness.

This result is, of course, in contradistinction to the conclusions which Dallenbach has drawn from his experimental findings. In Dallenbach's experiment, mentioned above,[19] the criterion of 'sensory' clearness was the *O*s own designation of the clearness-status of the various portions of the experience during the presentation. Such reports fell into four classes: (1) no report of clearness; (2) report of one clearness-degree—one level; (3) report of two clearness-degrees—dual-level; and, infrequently, (4) report of three clearness-degrees—multi-level. But the *O*s were also required to describe as fully as possible the experience itself. These 'meaning' reports (so-called) varied, as one would expect, from most complete designations of the nature of the part-experiences—hue, intensity, position, and even texture of the color square—to nothing at all. Fullness of report was then taken by Dallenbach as the criterion of cognitive clearness, and cognitive clearness thereby classified in 17 steps or degrees. For example, report of the form, hue, and tint of a color-square gave degree number 14; if, however, the *O* reported chroma instead of tint the cognitive clearness was of degree number 15. Since, by this procedure, (1) sensory clearness had been given but three or four classificatory steps, with none or one or two real degrees in a given consciousness, and cognitive clearness, on the other hand, had been discovered in 17 different degrees, with many and various steps in a given consciousness, and, moreover, since (2) cases occurred in which a high degree of attributive clearness was reported for a process described very poorly, and in which a low degree of attributive clearness was reported for processes described in detail, Dallenbach came out with the conclusion that sensory clearness is distinct from cognitive.[20]

It is significant that in this experiment in which Dallenbach has justified the distinction between sensory and cognitive clearness he was fearful lest his own *O*s were confusing the two. His fearfulness had a real foundation. He, who was one of the *O*s, said that "neither the method nor the instruction was conducive to good psychological description." Another of the *O*s maintained that "the report is so complicated that it seems foolish to talk about attributive clearness." He had then no criteria for distinguishing attributive clearness from cognitive.

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[20] Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 04-5.

This *O* was much disturbed by after-images, and believed that relative clearness in the after-images influenced his report of the processes which had occurred during the exposure. The third *O* said that any side of the report could be predetermined without affecting the process of the exposure-period. However, these considerations apparently did nothing to reduce Dallenbach's conviction of the separateness of sensory and cognitive clearness, for in data obtained under these conditions he found proof of the distinction. His *O*s were in error when they reported one clearness for the other. Britz had got one kind of clearness when he should have obtained another. Even recently the faith in the reality of the 'attributive-cognitive error' has shown itself in the literature.[21]

Before entering upon a criticism of Dallenbach's position on this matter one must look to the premises of the whole discussion. These are two in number: the first is that the *O* is able to report upon the clearness of a given part-content accordingly as it stands out to his grasp in the total experiential content; the second is that the clearness of conscious processes determines the fullness of report upon these processes. Dallenbach has stood upon these two premises, but he has read one kind of clearness in the first, and another kind in the second; this means that one goes but part of the way with Dallenbach if he agrees to these premises but refuses to recognize the necessity of any distinction of clearness. That these premises are valid is shown not only by data of Dallenbach's, but also by results from the present experiment. The *O*s were able to render reports upon the clearness of their experiences; and they agreed in maintaining that the essential thing about this clearness is its positive relation to the ability to describe the experiences themselves.

What Dallenbach found in his study, as shown above, was that reports of the 'sensory' clearness of the experiences for the period of presentation did not correlate perfectly with a more or less arbitrary tabulation of the quantity of things that the *O* was able to say in description of the experiences after they were past. But why should the fact that these two things do not give a one-to-one correlation be any proof that the observer has had within his consciousness two types of clearness? The situation may be simply—and this is the present contention—that Dallenbach was dealing with one kind of clearness, but dealing with it by two methods; and that the divergence in the results of these two methods, the divergence from which he has argued for two clearnesses, arose from the fact that these two methods were applied, actually, to successive and therefore sometimes divergent consciousnesses.

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[21] See Dallenbach's criticism of Johnson, in this JOURNAL, 37, 1926, 153. See also, N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, *ibid.*, 247-256.

This fact was shown in the introspective reports of Dallenbach's *Os*. One *O* declared: "The instructions ask too much for an adequate report, and any side of the report (naming, describing, number order, form, etc.) could, it seems to me, be 'set for' without making the least difference in the processes which go on in the actual exposure period.'

And later: "You know a lot about the colors after the exposure is over, but I am perfectly sure you lose a lot; as you start writing other aspects become non-reportable. It isn't so much the way you set yourself before the exposure that determines the report, but how you go to work after the exposure. I feel pretty sure that if *E* should call out after the exposure, 'Report on chroma,' I could report all chromas adequately, whereas usually by naming colors, mentioning clearness, brightness, etc., I lose certainty about chroma."

Another *O* said: "The colors persisted in the after-period, and at present I have the notion that the relative clearness in the after-period influences my report of the processes during the time of the exposure. That is to say, it is very difficult to draw a line, with everything else that I have to do, between these two periods."

The third *O* maintained: "What I report doesn't depend so much upon what I experience as upon where I start the report. When I concentrate upon the naming and description of the colors, my report on process is uncertain, and *vice versa*."[22]

TABLE IV

Degrees of Cognition	*Observers*						Totals for all *Os*	
	W		*F*		*D*			
	U	L	U	L	U	L	U	L
A 0–11 (15 rows)	5	324	2	63	10	1[illegible]6	17	583
B 12–17 (6 rows)	219	20	101	11	242	[illegible]1	562	52

That the distribution of clearness in consciousness is subject to change, both during a short presentation-period and during the after-period, was definitely shown by results obtained in the present experiment with cases of alternations, in which a reversal of clearness-levels was found to coincide with the reversal of fields. It is not improbable, in those instances in which Dallenbach found the judgment of clearness and the data of the report to be inversely related, that some such comparable change of clearness-status had occurred.[23] One would not expect such redistribution of clearness to occur very often. Actually, in Dallenbach's experiment, it happened very infrequently. As is shown by Table IV, compiled from Dallenbach's data[24] there exists a high correlation between "levels of attributive clearness" and "degrees of cognition." Dallenbach's table shows for each *O* the number of color-square experiences that occurred for every combination of sensory clearness-status and cognitive clearness-status. The first column of his table gives stages of cognitive clearness from none to degree 17; Table IV is simply a condensation of Dallenbach's data in which two levels of sensory clearness were present. In row A is summed, for each *O* and for all *O*s taken together, the number of reports of experiences on the upper level of sensory clearness (U) and on the lower level of sensory clearness (L) in the first 15 rows of Dallenbach's table (cognitive steps 1-11 and 4 stages presumably below the level of cognitive clearness). In row B are the corresponding data for the last 6 rows of Dallenbach's table (cognitive degrees 12-17).[25] It is apparent from the table that most (97%) of the experiences judged low in 'sensory clearness' were relatively poorly described, and conversely that most (92%) of the experiences judged high in 'sensory clearness' were relatively well described.

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[22] *Op. cit.*, 198. Cf. also the correlations of upper level of clearness and order of naming, Dallenbach's Table IX.

[23] Rubin found figure and ground to be reversible in the after-image. Katz had obtained what was probably the same result in his experiments with the localization of after-images. Rubin, *Visuell wahrgenommene Figuren*, 1921, 88–91; David Katz, Die Erscheinungsweisen der Farben, *Zsch. f. Psychol.*, Erg. Bd., 7, 1911, p. 59 esp.

[24] *Op. cit.*, 195, Table II.

Dallenbach's experiment, if interpreted on the basis here outlined, failed to show that clearness in human consciousness is to be conceived as of a peculiarly dual nature; the criticisms here advanced, if they are valid, leave the distinction of sensory and cognitive clearness without adequate experimental support. Dallenbach's experiment has shown merely that an *O* is capable of rendering a report upon the clearness of the various portions of his experience, and that when in a complex situation (like Dallenbach's) he sets out also to describe the entire content of his consciousness, changes may occur in the clearness-relations, so that what he ends with on paper will not correlate perfectly with that he may originally have included within his conscious range.

This correlation, one must add, while not perfect, is nevertheless high, and points therefore to some intimate connection of clearness and report. Here the results of the present experiment are in complete accord. The reports of the *O*s have been found to agree in showing that the outstanding thing about clearness is ability to report. This leads to the further suggestion that here, in the linking of clearness and description, is a criterion for clearness that is inherent, and, moreover, objective: the relative adequacy of the separate descriptive accounts which an *O* is able to render of the various components of a given consciousness may be used as an index of the clearness of these components. This measure of the clearness of the mental processes of an observer will hold reliably enough if it is employed for nothing more than it is a measure of, namely, immediate processes; if it is to be used as an index of the clearness-relations of processes that are past, one must proceed with a caution commensurate with Dallenbach's data and the present data, which reveal that changes may occur in the clearness-relations for successive conscious experiences.

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[25] The separation between cognitive 'degrees' 11 and 12 is determined by the point of intersection—the same for all three *O*s—of the two slightly overlapping curves obtained by plotting cognitive degrees as abscissae and frequencies as ordinates for each level of sensory clearness.

These criticisms of Dallenbach, if they are valid, leave the distinction of sensory and cognitive clearness without experimental foundation, but it would be incautious to go on to deny utterly the existence of any such differentiation; evidence in favor of a distinction may sometime be brought forward. It is difficult to prove the negative of such a question as this; one can hope merely to establish a probability in favor of one position or the other. Since, in the experience of figure and ground, an experience which has reason to be regarded as typical among perceptual phenomena, the differentiation has failed to appear even when the setting of the experiment was in its favor, it is apparent that the evidence of the present study does not support any distinction of sensory and cognitive clearness.

III. THE THEORY OF THE TWO LEVELS

This review of clearness would not be complete without a consideration of the 'dual-level' hypothesis. This hypothesis was set forth in detail by Titchener in his *Feeling and Attention* in 1908.[26] Consciousness at any one time seems to show two levels of (sensory) clearness. These levels vary reciprocally; when the upper level is particularly clear the lower level is correspondingly obscure. When attention is less fixed the difference between the two levels is less marked. This duality, then, ranges between maximal and minimal difference. There are slight differences of clearness in the upper level, possibly also in the lower.

This theory Titchener modified slightly after Geissler's experiments.[27] Geissler, in an experiment on the measurement of attention in terms of degrees of clearness, found that the attentive consciousness is of two types, the dual-level and the multi-level types. This latter, rarer type manifests several (three or more) degrees of clearness in a given consciousness,[28] a conclusion which agreed with the earlier contention of Wirth that at times he had found more than two levels of clearness in his own consciousness.[29] Dallenbach then sought to do with auditory and with cutaneous presentation what Geissler had done with visual; none of his *O*s showed the multi-level type of consciousness.[30] In the course of the above-mentioned experiment on attributive and cognitive clearness one of Dallenbach's *O*s reported multi-level types of consciousness, but it was later found that the levels were of cognitive, not of sensory clearness; and Dallenbach raised anew the question whether there are ever more than two degrees of clearness observable in the processes of a given consciousness.[31] He did not believe Geissler's evidence to have proved the possibility of a multi-level type.

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[26] Pp. 220-242.

[27] *Experimental Psychology of the Thought Processes*, 293.

[28] L. R. Geissler, *op. cit.*, 527 f.

[29] W. Wirth, Zur Theorie des Bewusstseinsumfanges und seiner Messung, *Philos. Stud.*, 20, 1902, 493 f.

[30] Measurement of attention, this JOURNAL, 24, 1913, 506 f; Measurement of attention in the field of cutaneous sensation, *idem*, 27, 1916, 460. The 'law of the two levels' was further borne out in an experiment by N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, *idem*, 37, 1926, 247-256.

The results of the present experiment are applicable to clearness in general and not particularly to sensory clearness, for indeed the experiment never revealed a clear-cut distinction between a sensory clearness and some other kind of clearness. The results do show that some perceptions occur with but two levels of clearness as clearness is here defined, and also that other perceptions occur with three, four, or even more levels of clearness. In the simplest, least equivocal case of figure-ground experience one field may be figure and the other ground; here typically the one field is of a high degree of clearness and the other of a low degree; here are but two levels of clearness. In the case of an intermediate experience, however, there may be, not two, but several fields in the perception, each on its own level of clearness; here is a multi-level condition. The presence of intermediates is a function of the ambiguity of the stimulus; when conditions are equivocal an intermediate perception is more likely to occur. It follows, therefore, that whether a given perception of figure and ground belongs to the dual-level or to the multi-level type depends at least in part upon the nature of the stimulus. If one argues from the figure-ground form of perception to perceptual experiences in general he will say that consciousness may contain at times two levels of clearness and at other times may contain more than two, and, moreover, that the equivocality of the conditions of the experience may play a part in determining the type of consciousness in a given case.

It may be possible to explain the variety of results that have been obtained in this problem of the two levels by pointing out, by analogy with the figure-ground phenomenon, that one would expect to find different conditions of stimulation, attitude, setting, etc., to vary in degree of equivocality and for this reason and perhaps other reasons also to give rise to perceptions sometimes of the dual-level type exclusively, at other times of both dual-level and multi-level types. If this is the case, agreement cannot be expected in experiments on the problem until the conditions are subjected to more adequate control.

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[31] Attributive *vs.* cognitive clearness, *op. cit.*, 226-7. In a footnote (p. 227) Dallenbach reviewed the case for the multi-level type of attentive consciousness.

SUMMARY

In the perception of figure and ground attention plays a prominent rôle. The field that is figure is in general also the portion of the experience which stands forth as attentively clear, while the groundal field occupies a subordinate position in clearness. When conditions are equivocal, the experiences are frequently of an intermediate order. In this case the figure is made up neither of the one nor of the other stimulus-field exclusively, but of central portions of each field. The ground here, as indeed may be the rule, consists of peripheral portions of both fields. With such intermediates a close correlation is found to exist between the clearness-status and figure-properties of the several portions of the experience. If the two fields share about equally the property of figure, they are found also to occupy in most cases the same level of clearness. If one field predominates in the figural experience, its clearness is usually on the higher plane. A reversal of figure-ground fields may occur either during a presentation or in the after-image following the presentation; such an alternation of fields is accompanied by an exchange in clearness-status. Thus has been established a high, though not a perfect, correlation between figure-ground status and degree of clearness in a perception.

This clearness, it must be noted, does not appear to be either what has been called sensory clearness, or what has been termed cognitive clearness, for any differentiation of sensory and cognitive failed to be revealed. Whatever distinction Dallenbach found experimentally may have been a function of his setting. This clearness, moreover, does not seem to be limited by the dual-level hypothesis, for it may occur in consciousness not merely in two, but in three or four or many levels, according to the nature of the experience in question.

That attention in terms of the comparative clearness of the various portions of the experience is relevant to the problem of perception has been made apparent in the case of the figure-ground phenomenon by the demonstration of a correlation between a report upon clearness and a designation of the fundamental components of the perception. That this correlation has not been found to be perfect is but a point in favor of the view that an account of the phenomenon cannot be held to be complete if it does not include a description of the clearness-relations present in the experience. Thus a definite and objective formulation of the concept of attention comes into a very intimate and highly significant relation with a specific problem of perception, and points further to a generalization by which such a relationship between attention and the nature of the perceptual experience should be expected to obtain for psychological situations as a whole. Not only would such a generalization give a concrete and workable theory of attention, but it would lend itself immediately for application to the solution of the old and yet-remaining problem of descriptive adequacy in the field of perception.

THE BEST FITTING FREQUENCY FUNCTION FOR URBAN'S LIFTED-WEIGHT RESULTS

By H. M. LUPKIN, University of Pennsylvania

Urban,[1] in assuming the $\Phi(\gamma)$-hypothesis, gives as the conditions which must be fulfilled (1) that the probabilities for a 'heavier' judgment must be expressed by a function f (x), which is asymptotic both to the x-axis and to the line $y=1$, or that

$$\operatorname*{Limit}_{x' = -\infty} f(x) = 0 \quad \text{and} \quad \operatorname*{Limit}_{x' = \infty} f(x) = 1$$

(for judgments of 'lighter' the limits are reversed); and (2) it is evident that this function must be expressed by means of an integral

$$y = \int_{-\infty}^{\infty} F(x)dx$$

which must exist and have a finite value greater than zero for all values of x from $-\infty$ to $+\infty$, and that the probability for any particular comparison stimulus shall be given by

$$p = \frac{1}{M}\int_{-\infty}^{x} F(x)dx.$$

There are many functions which would fulfill these conditions, but the one selected is

$$F(x) = e^{-G(x)}.$$

This function G(x) is expressed in the form

$$G(x) = h^2(x-a)^2$$

where x is the variable stimulus, a the limen, and h is a measure of precision. In this form

$$p = \frac{1}{M}\int_{-\infty}^{x} e^{-h^2(x-a)^2}dx$$

where $M = \int_{-\infty}^{\infty} e^{-h^2(x-a)^2}dx$. Urban shows that

$$M = \frac{\sqrt{\pi}}{h}.$$

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*Accepted for publication April 7, 1927.

[1] F. M. Urban, Die psychophysischen Massmethoden als Grundlagen empirischer Messungen, *Arch. f. d. ges. Psychol.*, 15, 1909, 261-355; 16, 1910, 168-227.

This hypothesis has as its basic assumption that the probabilities arrange themselves in the form of the integrated probability function. If such a condition is true it will simplify the computations necessary to determine the constants which are desired. This can be done by the Method of Least Squares, but it must be remembered that this method is based on the assumption that the *arithmetical mean* is the most probable value that can be assumed for a series of observations.[2] From a series of observation equations, each of which is to be properly weighted,[3] the constants a and h can be obtained. The difference between the limens for observations of 'heavier' and 'lighter' is termed the 'interval of uncertainty,' in which neither the 'heavier' or 'lighter' judgments have a probability as great as 0.5. Urban[4] has found a rather distressing condition here, due to the fact that his probable errors for this interval are surprisingly large. This has led to the belief that the fundamental assumption of the $\Phi(\gamma)$-hypothesis is not valid and also that Urban's material, instead of being homogeneous, is heterogeneous. It is the purpose of this paper to determine these two points.

Since the publication of Urban's first paper a vast amount of work has been done in the field of statistical analysis, particularly in that branch devoted to 'frequency functions.' Karl Pearson[5] has developed his types of frequency functions, which are quite well known but as yet little used in psychometric work. There is also another type of function which is now coming into prominence, *i.e.* the Gram-Charlier. It is our purpose to examine Urban's original data and see if either of these functions are fundamentally better than the assumption of the 'probability integral.'

Since the frequencies of a judgment of 'heavier' or 'lighter' for any given comparison stimulus are in the form of an integrated function it is necessary to take the successive judgments for each value of the comparison stimulus. This gives us our values so that they can be handled as a frequency distribution. We may either work on the basis of the number of judgments or on the percentage of judgments. Whichever method is used, the results will be the same. Here the work has been done on the number of judgments, in order to simplify to some degree the work of curve fitting.

The use of the normal or Gaussian function necessitates only the computation of the first and second moments, that is, the Arithmetic Mean and the Standard Deviation or σ. In the case of the Pearson and Gram-Charlier functions it is necessary to compute in addition to these the third and fourth moments about the mean, which is done in a manner analogous to the computation of the first and second moments. This raises at once a serious objection to the use of those two types of functions. Fisher[6] quoting Charlier says that it is generally useless to compute moments of a higher order than the second when the number of observations is less than 1000. Thiel gives the following rules:

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[2] L. D. Weld, *Theory of Errors and Least Squares*, 1921, 65-69.

[3] Urban, *loc. cit.*; W. Brown and G. H. Thompson, *Essentials of Mental Measurement*, 1921, 65-69.

[4] Urban, *loc. cit.*

[5] Karl Pearson, Skew variation in homogeneous material, *Phil. Trans. Roy. Soc.*, A, 186, 1895, 343 ff.; 197, 1901, 443-459; 216, 1916, 429-457.

For the first and second moments rely exclusively on the observed data.

For moments between the second and sixth rely partly upon theory and partly upon observations. These objections could be overcome in the case of the Gram-Charlier by the use of the method of Least Squares, but this would increase the amount of necessary computation to a very considerable extent.

TABLE I
Constants for the Pearson Curves

O	Type	β_1	β_2	k_2	Range	Skewness	Mode
I H	J_1	1.052±.168	3.488±.220	−0.67±.06	82−106	−1.56	108.0
II H	I_1	0.452±.123	3.461±.342	−0.87±∞	36−112	−0.39	102.1
III H	I_1	0.735±.125	3.433±.260	−0.01±.16	76−109	−0.73	104.2
IV H	III(VI)	0.354±.164	3.566±.515	+4.13±∞	−116	−0.30	101.2
VI H	I_1	0.730±.185	3.618±.409	−0.07±.58	64−110	−0.60	103.7
VII H	I_1	0.711±.250	3.985±.717	−3.87±∞	−0−111	−0.44	102.9
I L	I_1	0.109±.038	2.656±.116	−0.08±.03	82−109	+0.25	90.3
II L	I_1	0.046±.020	2.465±.079	−0.00±.01	80−106	+0.17	93.1
III L	I_1	0.103±.052	3.153±.233	−26.4±∞	69−2630	+0.16	95.2
IV L	I_1	0.157±.071	3.019±.295	−0.28±.36	78−137	+0.23	92.9
V L	I_1	0.629±.166	3.550±.401	−0.70±.56	84−135	+0.52	90.4
VI L	I_1	0.010±.004	2.608±.113	−0.00±.00	76−117	+0.07	93.3
VII L	I_1	0.007±.004	2.658±.120	−0.01±.00	73−118	+0.06	93.1

Brown and Thompson[7] advise the use of Shepherd's correction for grouping, stating that the corrected moments are "probably still valid even without high contact, if the terminal frequencies are zero," but since high contact involves the vanishing of the first n derivatives, where n is large, this does not seem to be an advisable procedure in this case and the moments have been left uncorrected for grouping.

Table I shows the principal constants for the Pearson curves for thirteen of Urban's sets of observations.[8] The data of one *O* (V) were omitted for the 'heavier' since they gave negative frequencies when the differences of the judgments were taken. The determining factors in selecting the proper type of curve to fit the observations are the β's which are obtained from the moments. These quantities are always positive. If $\beta_1 = 0$ and $\beta_2 = 3$, the Normal or Gaussian Curve is the best fitting, while if only $\beta_1 = 0$, the curve is symmetrical. The probable errors of the β's are of significance, for it will be noted that in all cases it would require more than twice the probable error of the β's to carry them over to zero, or the chance of a symmetric curve in any case is less than 4.5 to 1. The criterion k,

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[6] Arne Fisher, *The Mathematical Theory of Probability*, 2nd ed. 1, 1923, 217.

[7] Brown and Thompson, *loc. cit.*

[8] For explanation of the symbols see W. P. Elderton, *Frequency Curves and Correlation*, 1906.

$$k = \frac{\beta_1(\beta_2 + 3)^2}{4(4\beta_2 - 3\beta_1)(2\beta_2 - 3\beta_1 - 6)},$$

is in all cases, except IV 'heavier,' negative, which means that a Type I Curve is the most probable. In the case of IV 'heavier,' k is greater than one and is finite, which would indicate Type VI, but in this case since the skewness, −0.30, is not large and $2\beta_2 - 3\beta_1 - 6$ is small, the curve fitted was Type III. This was done for purely practical reasons, since the data were of such a nature that in determining the maximum ordinate Gamma Functions[9] of large numbers entered into the computations if Type VI was used.

In four cases, II, IV, and VII 'heavier' and III 'lighter,' the probable errors of the criterion k approached infinity which indicated that the β's were of such a nature that they were near critical values where the types change. This is best seen from Chart I, which shows the location of the β's on about one-fourth of Pearson's Diagram.[10] The lines on this chart are obtained from the values of the β's which are typical for the various types of Pearson's curves. These curves are derived from a consideration of the point binomial $(p + q)^n$, where $p + q = 1$, which when expanded gives a series which represents the probabilities of the happenings of an event, $n_1(n-1)$ ....0 times when the probability of a single event is known; thus when $p = q$ we have a symmetric function which when n is large approaches the Gaussian Curve and when $p \neq q$ gives us a skewed curve, the first case being analogous to that of tossing a coin and the second that of tossing a die or a tetotem; but this does not represent all the probabilities that arise for there is another series which gives the chances of getting r, $r - 1$, ... 0 black balls from a bag containing pn black and qn white balls when r balls are drawn.[11] From differential equations derived from these series the critical values of the relationships between the β's are obtained which differentiate the types of curves and the lines on the chart are the graphs of these relationships. The point on the chart marked G repre-

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[9] E. B. Wilson, *Advanced Calculus*, 1911, 378.

[10] Karl Pearson, *Tables for Statisticians and Biometricians*, 19 4, lxii ff., 66.

[11] Elderton, *loc. cit.*, 36-46: Karl Pearson, Skew variation in homogeneous material, *loc. cit.*, 1895.

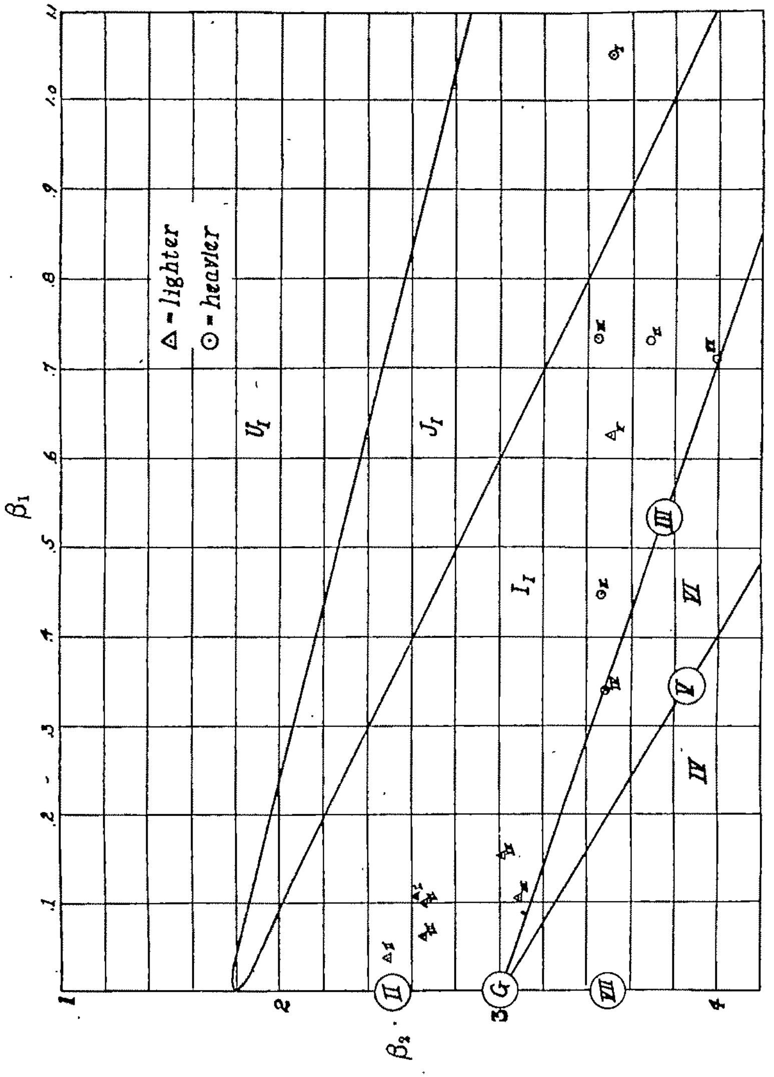
β₁
.1
.2
.3
.4
.5
.6
.7
.8
.9
1.0
1.1
β₂
1
2
3
4
Δ = lighter
⊙ = heavier
U_I
J_I
I_I
II
G
VII
III
V
VI
IV

CHART I

sents the normal curve. When the point represented by the β's is very close to one of these lines the probable error of the criterion approaches infinity, *i.e.* IV 'heavier.' Quoting from a letter from Pearson in regard to this set of data, "Probably you would find it quite adequate to use a Type III curve for your data, but if you look at the chart on page 66 of the Book of Tables you will see that any one of the Types III, VI, or V, is likely to fit your data, and accordingly the probable error of the discriminating k becomes very large indeed." In this chart it will be noted that all the points are relatively close to the 'G,' *i.e.* Gaussian point, especially in the case of 'lighter' judgments.

As was to be expected all these curves are skewed, the 'heavier' in the negative direction and the 'lighter' in the positive direction.

The equation of the Type I curves is:

$$y = y_o \left(1 + \frac{x}{a_1}\right)^{m_1} \left(1 - \frac{x}{a_2}\right)^{m_2}, \text{ where } \frac{m_1}{m_2} = \frac{a_1}{a_2}.$$

In this case the origin is taken at the 'mode' and the curves are not asymptotic, but cut the x-axis at $-a_1$ and $+a_2$. The curve for I 'heavier' presents a peculiar form of Type I, where the curve starts at a finite point on the x-axis, rises rapidly, and has an infinite ordinate, but the area under the curve is finite. The equation of Type II is:

$$y = y_o e^{-rx} \left(1 + \frac{x}{a}\right)^{ra}$$

where the curve is asymptotic to the x-axis at one end but cuts at a fixed point at the other end.

The Gram-Charlier frequency function is given by the equation

$$y = N[\phi_0(x) + k_3\phi_3(x) + k_4\,\phi_4(x)],$$

where N = the number of observations, $\phi_0(x) = \frac{1}{\sigma\sqrt{2\pi}}\, e^{-\frac{1}{2}\left(\frac{x}{\sigma}\right)^2}$,

and $\phi_3(x)$ and $\phi_4(x)$ are the third and fourth derivatives of $\phi_0(x)$. In this case the origin is taken at the mean, and k_3 and k_4 are constants involving the third and fourth moments about the mean. These are called respectively the 'skewness' and the 'excess,' the first having a tendency to produce asymmetry, while the second tends to flatten the curve or to make it top-heavy.[12] In order to fit this curve it is necessary to have a table giving the derivatives of $\phi_0(x)$, one of the best being that of N. R. Jørgensen in his Untersøgleser over Frequensflader og Korrelation,[13] which contains also other useful tables for curve fitting. Table II shows the values of k_3 and k_4 for the same data as the Pearson curves. It will be noted that these constants do not vary greatly in the various sets, an indication of homogeneity of the data.

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[12] Arne Fisher, *loc. cit.*, Parts II and III.

TABLE II

Constants for the Gram-Charlier Curves

O	k_3	k_4
I H	+0.171	+0.020
II H	+0.118	+0.027
III H	+0.145	+0.019
IV H	+0.109	+0.023
VI H	+0.142	+0.026
VII H	+0.141	+0.041
I L	−0.056	−0.014
II L	−0.031	−0.013
III L	−0.052	+0.006
IV L	−0.062	+0.001
V L	−0.131	+0.028
VI L	−0.016	−0.016
VII L	−0.038	−0.013

TABLE III

Sum of the Squared Differences of the Observed and Theoretical Values Divided by the Theoretical

O	Normal	Pearson	Gram-Charlier
I H	60.7	24.8	65.1
II H	20.1	32.8	28.5
III H	69.9	17.7	25.6
IV H	75.7	76.3	45.5
VI H	20.2	6.1	4.0
VII H	21.9	10.9	13.3
I L	27.4	41.1	21.0
II L	9.0	25.1	6.0
III L	52.6	56.9	74.7
IV L	16.6	82.7	9.7
V L	69.8	45.0	63.5
VI L	14.9	30.6	8.6
VII L	32.5	21.1	27.0
Average	37.8	36.2	30.2

Table III gives the values of the sum of the squared differences of the theoretical and actual values divided by the theoretical value for the Normal Curves, the Pearson Curves, and the Gram-Charlier Curves. There does not seem to be very much choice as to which type of curve gives the best fit. In the case of the Normal curve, it is seen that twice it has the best fit, six times the second best and five times the poorest; while the Pearson Curves fit best five times, twice the second best, and six times the poorest. The Gram-Charlier Curves give the best fit six times, a second best fit five times, with a poorest fit only twice. If the higher moments had been adjusted by the Method of Least Squares the Gram-Charlier function would undoubtedly have given the best fit in all cases. The averages of the squared differences does not show a great amount of variation but the Gram-Charlier shows a superiority over the Pearson or the Normal forms.

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[13] N. R. Jørgensen, *Untersøgleser over Frequensflader og Korrelation*, 1916.

A STUDY OF THE AUTOKINETIC SENSATION

By J. P. Guilford, University of Illinois, and
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Introduction

In the study of the autokinetic sensation, we have to do with an illusion which was first noticed and reported by the astronomers. Von Humboldt[1] (1799) observed that a star appeared to move about when steadily fixated, and that the movement lasted several minutes. Schweizer[2] (1858) demonstrated that this movement was subjective by the simple fact that there was no agreement on the time and direction of the movement as reported simultaneously by several observers. He gave the illusion the name of *Sternschwanken*. He discovered at the same time that a black dot on a light background moved similarly—though through angles of smaller magnitude. The latter phenomenon became known as *Punktschwanken*.

Charpentier[3] (1886) made the first laboratory study of these movements. He found that a faint light in a dark-room wandered when fixated. De Parville[4] (1886) called attention to the similarity of the phenomena reported by Von Humboldt and Charpentier. Charpentier, however, objected to the comparison because he regarded the two phenomena as very different.[5] He held that the movements noted by Von Humboldt were oscillatory and of short excursion, whereas those he reported were uniform and steady and of long excursion.

The phenomenon reported by Charpentier was independently discovered by Aubert[6] (1887), while he was working in a dark-room on the perception of movement, and it was called by him the *autokinetische Empfindung*—by which name it has since been known. Since Aubert, special studies of these phenomena have been made by Exner,[7] Bourdon,[8] Carr,[9] Adams,[10] and Schilder.[11]

We became interested in the autokinetic sensations during the study of eye-movements in the fluctuation of attention.[12] Conditions were favorable in that study for the illusory movements, and as an apparatus for photographing eye-movements was already set up, we decided to test the eye-movement theories of the autokinetic sensation by photographing the movements of the eyes during the illusion. Before reporting the results of our photographs we first summarize the facts which must be accounted for by any theory, and then we review the theories and state the arguments that have been advanced both for and against them.

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*Accepted for publication March 1, 1927.

[1] A. von Humboldt, *Kosmos*, 3, 1850, 73-74; *Voyage aux regions équinoctiaux*, 1, 125.

[2] G. Schweizer, *Ueber das Sternschwanken*, 1858.

[3] A. Charpentier, Sur une illusion visuelle, *Comptes rendus*, 102, 1886, 1155-1157.

[4] H. de Parville, Sur une illusion visuelle et l'oscillation apparente des étioles, *ibid.*, 1309.

[5] Charpentier, *ibid.*, 1462-1464.

[6] H. Aubert, Die Bewegungsempfindung, *Arch. f. d. ges. Physiol.*, 40, 1887, 459-480; Nachtrag zu den Artikel, "Die Bewegungsempfindung," *ibid.*, 623-624.

[7] S. Exner, Über autokinetische Empfindungen, *Zsch. f. Psychol.*, 12, 1896, 313-330.

[8] B. Bourdon, *La Perception visuelle de l'Espace*, 1902, 333-342.

[9] H. A. Carr, The autokinetic sensation, *Psychol. Rev.*, 17, 1910, 42-75.

[10] H. F. Adams, Autokinetic sensations, *Psychol. Mon.*, 14, 1912, no. 59, 1-45.

[11] P. Schilder, Über autokinetische Empfindungen, *Arch. f. d. ges. Psychol.*, 25, 1912, 36-77.

[12] J. P. Guilford, 'Fluctuations of attention' with weak visual stimuli, this Journal, 38, 1927, 534-583.

A summary of the results of the previous investigations reveals the following.

(*a*) *Conditions of movement.* The conditions favorable to the illusion are: (1) a dark-room; (2) a small stimulus-object; (3) a simple stimulus-object; (4) a stimulus-light of low intensity; (5) a steady fixation—sudden voluntary eye-movements destroy the illusion; and (5) fatigue—either general mental (Charpentier), or ocular (Bourdon).

(*b*) *Direction of movement.* The direction of the movement is reported as (1) a curved zigzag path which leads upward and outward (Charpentier), and return (Humboldt); (2) a circular or an elliptical path out toward the periphery and back again (Schweizer, Adams); (3) peripheral in 88% of the experiments regardless of the starting point (Adams), but the further the starting point is from the center of fixation the greater is this peripheral tendency (Carr); (4) in the direction of eye-strain in 26-91% of the cases—depending upon the distance of the starting point from the fovea (Adams); (5) dependent upon the preceding position of the eyes If the eyes have just previously been directed to a point which is less than 30° from the fixation-point, the movement, upon fixation, will be toward that point. If the line of regard has been previously directed to a point which is more than 30° from the normal, the movement will start rapidly in the opposite direction, will decrease in speed and then return (Carr).

(*c*) *Rate of movement.* The rate of movement varies considerably among the observers. It is estimated as (1) 2°-3° per sec. (Charpentier); (2) 15°-20° per sec. (Carr); and (3) 10° or less per sec. (Adams).

(*d*) *Extent of movement.* The extent of the movement may be 30° or more (Charpentier); 20°-30° (Exner); 65°, under extreme fatigue (Carr); and 40°-50°, in the periphery (Adams).

(*e*) *Time of movement.* Movement begins, on the average, 6.1 sec. after fixation (Adams)—but the time varies considerably with the size of the stimulus-object.

(*f*) *Voluntary control of movement.* The movement is subject to voluntary control (1) in direction (Charpentier, Bourdon)—merely by the idea of a certain direction; (2) in duration, velocity, and extent (Carr); and (3) in form—it traces letters thought of by the observers (Adams). The voluntary control could not be verified by Aubert.

The first eye-movement theory of the autokinetic movements was proposed by Hoppe (1879). He came to the conclusion, in his treatment of the various kinds of illusory visual movement, that all such phenomena rest upon the functioning of the extrinsic eye-muscles.[13] The illusion, he thought, might arise either from the innervation of these muscles, or from actual involuntary eye-movements with the accompanying shifts of the image upon the retina.[14]

Charpentier (1886) tested the influence of gross eye-movements by fixating a group of dots, only the central (foveal) ones of which were visible. The group appeared to move in the usual manner without any change in the pattern of the dots. The dots were so arranged that if eye-movements had occurred the visible pattern would have changed. He eliminated abnormal tonicity of the eye-muscles as a condition of the phenomenon by a series of experiments in which he held his eyes in various positions in their sockets. No correlation was found between these abnormal positions and the occurrence of the apparent movement. He believed, however, that the brief oscillatory movements of the *Stern-* and *Punktschwanken* were due to slight displacements of the eye caused by unequal fatigue of the eye-muscles.[15]

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[13] J. I. Hoppe, *Die Scheinbewegung*, 1879, 84.
[14] *Ibid.*, 23.

Exner (1896) brought as evidence against the eye-movement theory the results of an experiment in which concentric fixation points were used—in the center of a dark disk, 1 cm. in diam., there was a bright pinhole. When these were fixated he found that the pinhole and the disk moved independently—the pinhole darted about within the disk, never reaching the boundary, and the disk moved in its own fashion.[16] Simon (1904) could not, however, verify Exner's results. He found that the pinhole and disk moved together.[17]

Bourdon (1902) showed that no gross displacements of the eyes occurred during the apparent movement. He let the spot move to an apparent position about 20° to the right, and then he turned on the light only to find that he was still fixating the stimulus-object. As a further test, when the spot had moved outward he brought his fixation back to the position which he thought was the starting point, and then turned on the light only to find that he was staring in a direction opposite to that of the apparent movement and displaced to about the same extent.[18] He believed, however, that the phenomenon was produced by an integrative effect of the slight but numerous movements of the eyes which were made in the direction of the apparent movement, and that these slight movements were brought about by the idea of that direction. When the outward movements of the eyes were slow, no movement of the stimulus-object occurred; but when the reflex backward movements were abrupt, an apparent displacement of the stimulus-object resulted.[19]

Carr (1910) distinguished three types of the illusion: (1) a type in which the fixation-point and stimulus-object moved together; (2) a type in which they moved independently; and (3) a mixed type in which they participated unequally—the fixation-point often lagged behind the stimulus-object.[20] He found, using various methods of testing for eye-movements, that there were no eye-movements during the illusions of type (1),[21] and that eye-movements were always present during the illusions of type (2) "whose direction, speed and extent always bore some definite relation to the corresponding characteristics of the illusion."[22] Carr, after an exhaustive study came to the conclusion that eye-movements played only a minor rôle, and that the effective factors were "(1) the position of the eye in its socket; (2) the after-effects of eye-position; (3) motor strains as exemplified in the tests on voluntary control; and (4) the after-effects of such strains."[23]

Schilder (1912) verified Exner's results with the pinhole and disk.[24] He also found that eye-movements, although they appear shortly after the beginning of fixation, do not in most cases coincide with the beginning of the apparent movement.[25]

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[15] Charpentier, *op. cit.*, 1462.

[16] Exner, *op. cit.*, 327.

[17] R. Simon, Über Fixation im Dämmerungsehen, *Zsch. f. Psychol.*, 36, 1904, 192.

[18] Bourdon, *op. cit.*, 339.

[19] *Ibid.*, 341.

[20] Carr, *op. cit.*, 43.

[21] *Idem.*

[22] *Ibid.*, 46. It is indeed strange that an involuntary eye-movement should produce a displacement of the stimulus-object and not be accompanied by a corresponding displacement of the fixation-point. There is also a discrepancy in his report of the extent of the movements of type (2). He says in one place (p. 43), "the average extent was approximately 10°" and in another (p. 60), "no movement greater than 1 cm. (16′) was observed."

[23] *Ibid.*, 69.

[24] Schilder, *op. cit.*, 52.

[25] *Ibid.*, 65.

From the results of these early studies, we were led to doubt whether eye-movements were the cause of the illusion. A theory of the autokinetic sensation based upon eye-movements alone can not account for all the facts that have been reported. The end result of the early investigations seems to be that eye-movements do not play an essential rôle. This is certainly true in the cases of gross illusory movement, and in the cases in which two stimulus-objects move independently; for, in the first instance, gross illusory movements are not paralleled by gross eye-movements, and in the second instance independently moving stimulus-objects could not be, at one and the same time, produced by eye-movements.

The effectiveness of short involuntary eye-movements has not, however, been so clearly disproved. It may very well be, as Charpentier and Exner suggested, that the two kinds of movement—the short oscillations and the long excursions—are conditioned by different factors, and that eye-movements play an important rôle in the former, if not in the latter; or, as Bourdon suggested, that the large illusory excursion is a summation (under abnormal ocular circumstances) of small illusory movements which are conditioned by eye-movement.[26]

These questions of fact can not be settled until more accurate records, such as those given by the photographic method, have been made of the movements of the eyes during the illusion. It was in the hope of obtaining records which would incontrovertibly establish the relation between eye-movements and the autokinetic sensation that the present study was undertaken.

APPARATUS AND PROCEDURE

Apparatus. The apparatus and technique which we employed in photographing the eye-movements were fundamentally the same as those of Dodge.[27] The slight variations from Dodge's technique are described in detail by the junior author in his study of eye-movements during the fluctuation of attention.[28] The principal variation consisted in the substitution of a moving film of a panoramic camera, after the method of Miles and Shen,[29] for Dodge's falling plate.

The stimulus, a circular red spot of light 2.5 mm. in diam., was 170 cm. distant, and in the direct line of regard. The room was completely dark except for the stimulus and the beam of light that was directed for photographic purposes upon the cornea. The corneal reflection was focused upon the film, and the light from a signal lamp within the camera box was adjusted to fall close beside it. (See Plate I.)

Procedure. The stimulus was exposed, in one series of experiments, to the left eye—the eye whose movements were photographed; and in a second series, to both eyes.

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[26] For logical proofs that apparent movement cannot be due to a nystagmus of small amplitude, see W. S. Hunter, The after-effect of visual motion, *Psychol. Rev.*, 21, 1914, 250-253.

[27] R. Dodge, An experimental study of visual fixation, *Psychol. Mon.*, 8, 1907, no. 35, 1-95.

[28] Guilford, *op. cit.*, 557-559.

[29] W. R. Miles and Eugene Shen, Photographic recording of eye-movements in reading Chinese, *J. Exper. Psychol.*, 8, 1925, 344-350.

The senior author (D) acted as the observer. He sat with arms resting on a table, and with head firmly held by a biting-board and a head-rest. *O* fixated the stimulus and signaled the appearance of the illusory movement by pushing a button that closed the circuit of the signal-lamp. When *E* saw the signal, he started the panoramic camera and allowed it to run either until *O* released the button as a signal that the movement had ceased or until the end of the film was reached. The signal light not only marked the beginning and end of the illusory movement, but it also gave a line on the film that served as a base of reference for the eye-movements.

At the close of the experiment and during its course, *O* made drawings of the path of the illusory movement. Six photographic records and drawings were made when the left eye alone was used, and seven were made when both eyes together were used. *O* also described at the end of the experiments any other phenomena that he noted during its course.

RESULTS

The path of the illusory movement in the various experiments is shown in Figs. 1 and 2, and a typical section of the photographic record of the eye-movements is shown in Plate I. Records L1, L3, L4, and L6 of Plate I show respectively the eye-movements during the first part of experiments 1, 3, 4, and 6 with the left eye; the path of the illusory movements in the same experiments is shown in Fig. 1. Records B1, B3, B5, and B6 show similarly the eye-movements during the first part of those experiments with both eyes, whose illusory movements traced the paths shown in Fig. 2.

A detailed study of the photographic records shows that no gross movements of the eyes were made during the illusion; the eye-line paralleled very closely the base-line established by the stationary signal-lamp. As the photographic method was delicate enough to detect very small movements of the eyes—in special test-experiments it was found that a voluntary eye-movement of 1° displaced the eye-line approximately 0.7 mm.—eye-tremors were recorded. These movements are the only ones shown in the records; they are, however, too limited in extent to account for the gross illusory movements, and too irregular and too few in number to account for the small illusory movements, *i.e.* the oscillatory movements. We are forced, therefore, to the conclusion that eye-movements are not essential conditions of the phenomenon.

A positive and more significant result came from *O*'s reports. As the curves of Fig. 1 show, there is a remarkable uniformity in the pathways of the illusory movement. The typical path seems to be circular and along one of the diagonals of the visual field. Other investigators have described the paths as circular and elliptical, but no one has described the course more fully. Reports from other *O*s who noted the illusion in the experiments on the fluctuation of attention,[30] agree closely with those of D.

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[30] Guilford, *op. cit.*, 582.

Since the course of the illusion seemed to be uniform, it occurred to us that the movement might be explained by the streaming phenomena,[31] which were frequently observed in this experiment. The movement of the streaming field might, we thought, be transferred to the fixation-point, just as in the case of the apparent movement of the moon behind floating clouds, the movement of the field is referred to the object. Evidence for such an explanation can be found also in comments made by another *O* during the previous experiments: "the stimulus seemed to be moving in a downward right direction behind a veil of some opaque material. It almost covered up the stimulus, and yet I had the impression that the stimulus was moving."[32] This *O* was undecided whether the stimulus was moving or the mists which obscured it.

It is significant that the patterns of the illusory movement reported by D, lie in the direction of the most frequent patterns of the streaming phenomenon which were found by Ferree,[33] and by Edridge-Green.[34] It is significant, too, that when both eyes are exposed to the light, the patterns become irregular, as shown in Fig. 2. We are told by Carr[35] and Adams[36] that the direction of the illusory movement varies for the two eyes, and that when the eyes are used together something like rivalry in direction occurs. We are told by Edridge-Green that the streaming phenomenon has a different direction in the two eyes—a circular direction to the right with the right eye, and to the left with the left eye;[37] and by Ferree that the streams are continually changing, with a few typical patterns. The streaming movement, in any case, would be different for the two eyes; hence the conflict in direction when both eyes are used, the direction being determined at any moment by the dominant eye.

Will the streaming phenomenon account for all the facts of this illusory movement? We believe it will. A small, simple stimulus-object in an otherwise blank field is most favorable to the illusion because the object then loses the anchorage of other objects in the field. The illusion is not apparent in a field full of objects; since the streams take various directions, they should exert a distorting or wrenching effect upon the objects; but the field, being comparatively stable, resists distortion; the streams are thus neglected, just as are other entoptic phenomena. In all similar illusions of movement it is the small, 'detached' object which gives way to spatial displacement. A weak stimulus contributes to the illusion for the same reason—it is not a stable object.

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[31] C. E. Ferree, The streaming phenomenon, this JOURNAL, 19, 1908, 484-503.

[32] Guilford, *op. cit.*, 581 f.

[33] Ferree, *op. cit.*, 491.

[34] F. W. Edridge-Green, Visual phenomena connected with the yellow spot, *J. of Physiol.*, 41, 1910, 263-275.

[35] Carr, *op. cit.*, 56.

[36] Adams, *op. cit.*, 19.

[37] Edridge-Green, *op. cit.*, 270.

Steady fixation is a favorable condition because it favors the loss of orientation in the visual field. Orientation is regained by a sudden voluntary eye-movement, and *O* often 'wakes up' to find the stimulus back at the starting point, although it had made no return excursion. An example of this is shown in Fig. 1; the broken line in 4 marks a shift in orientation without perception of the movement.

That the direction of movement is explained by the streaming phenomenon, we have already pointed out. The zigzag paths, *i.e.* the minor oscillations, have not, however, been accounted for. We have shown reason for doubting that eye-movements are a factor even of these smaller movements. They may be due to cross currents of the streaming phenomenon, but since the currents, according to Edridge-Green, have well-defined paths, this explanation does not seem likely. Pulse may have something to do with the oscillations, as on some occasions an oscillation of the stimulus was observed in about the same rhythm as the heart beat. The pulse in the retinal arterioles may have its immediate effect upon the currents, accelerating them for a moment in various directions.

The almost universal tendency for the illusion to go in a peripheral direction agrees with the observations of Edridge-Green that the streaming phenomenon flow toward the fovea. Of course there must also be outward movements of the retinal fluids, but none was observed by Edridge-Green. This means, at any rate, that the centripetal currents are more noticeable, and likewise that they have a primary effect upon the illusory movement. It is to be remembered in this connection, however, that Edridge-Green's theory of color vision demands such inward-flowing currents, and that Ferree described currents which extended through and beyond the fovea. In point of fact, a spot within the fovea could move, if our theory is true, only if the streams extended through and beyond the fovea.

The independent movement of two objects in the field, as observed by Carr, can easily be explained by the streaming phenomenon—the two objects have retinal positions in two different streams. Exner's pinhole and disk experiment is, however, a different matter (as they occupy identical retinal positions) unless one wishes to invoke cross currents, for which there is some evidence. We know that there are various other forms of entoptic phenomena besides the currents of Ferree and Edridge-Green, some of them of vascular origin. The pinhole, being so small, could suffer phenomenal displacement due to the movement of the blood streams in the retina.

That the movement is in the direction of eye-strain seems to offer no difficulty to our theory. It is as likely that the eye-strains are a result as a cause of the movement. The strains may arise under the difficult conditions of steady fixation. The illusory movement may be just as effective as a real movement in setting into motion innervation of the muscles for the eyes to follow the light; while inhibiting innervations, playing under the effect of instruction to keep fixation constant, prevent the eye-movements. We know of no evidence which will decide between this interpretation and that of Carr and Adams.

The effect of previous position of the eye is not so easily accounted for because we do not know all the conditions of the experiments in which this was found. Assuming that the observer brought the eyes to the fixation-point from the previous position abruptly and immediately observed the apparent movement, we may invoke the effect of eye-movement upon the retinal currents. Ferree found that voluntary eye-movements accelerate the currents in the retina in the direction of the eye-movement. Edridge-Green verifies this so far as the peripheral branches of the streams are concerned. We have, then, the conditions for the apparent movement which Carr reports in consequence of a previous position of the eye; the movement is along the line of eye-movement just executed.[38] The two cases, of apparent movement in one or both directions (in the direction of previous fixation or else away from it and return) cannot be safely interpreted unless more of the facts are known. It may be, however, that the currents are forced to flow in one direction for a time and then there is a rebound or backwash in the opposite direction.

The apparent rate of the movement is very significant for the streaming theory. The rate is much exaggerated as compared with the distance actually traveled by the stimulus-object. Whereas it moves from 10°-20° per sec., according to Carr and Adams, it rarely travels over 30° in all, and requires considerably more than 2 sec. to do so. This reminds one again of the moon which glides rapidly along but gets nowhere.

There has been some disagreement on the matter of voluntary control, but the consensus of opinion seems to be that one can vary the direction, velocity, extent, and duration of the movement, to some degree, at will. So far as velocity, extent, and duration are concerned, the control may come from sheer self-instruction; we see what we want to see or expect to see. The movement of the moon may be controlled in the same way. But this could hardly hold for the direction, if direction is conditioned by the streaming phenomenon which has its own direction. Is it conceivable that voluntary effort in some way determines the direction of these currents? This would be possible in that voluntary effort means the innervation of certain eye-muscles and in this way exerts certain stresses and strains upon the eyeball and hence puts pressure upon the retina at certain points. This becomes even plausible in view of Carr's results. He found that an ocular strain exerted voluntarily in any direction resulted in apparent movement in that direction and that the velocity could be controlled by the degree of muscular innervation. As an after-effect of strain in one direction the apparent movement went in the opposite direction and this after-effect lasted from 30-120 sec. Does release of strain allow the retinal currents to run back in the opposite direction?

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[38] Carr, *op. cit.*, 63-66.

All the facts can thus be explained by the streaming theory. Will observations bear out all our interpretations? We have reports from two observers to the effect that the field of vision about the moving stimulus-object seems to be moving in the opposite direction from that of the apparent movement. Our photographic records show that the eye-movements are not sufficiently large, numerous, or regular to account for the illusory movement. From our knowledge of other illusory movements we know that a small object yields to phenomenal displacement when its background is in motion. The patterns of movement observed by D coincide approximately with the pattern of the retinal currents found by others. We still need to determine the direction of the retinal currents for D, and perhaps to correlate the two kinds of patterns in a few other *O*s.

Loss of orientation in the dark is a well-known phenomenon and needs no further observation in this connection. The relation between the pulse and the oscillatory movements, however, must be more closely examined. The effects of previous positions of the eye upon the streaming processes should be determined. We should know what the currents are doing for at least 2 min. after the eyes are brought to fixation from previous positions. We should also test the effect of voluntary effort and strain exerted in different directions upon the direction of the retinal currents. Our theory stands or falls upon the results of these investigations.[39]

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[39] This theory is closely related to similar theories for apparent movement of different types. Cf. Schilder, *op. cit.*, 70-71; A. H. Pierce, *Studies in Space Perception*, 1901, 331-338; and W. S. Hunter, Retinal factors in visual after-movement, *Psychol. Rev.*, 22, 1915, 484-486.

THE HISTORY OF THE EXPERIMENTAL STUDY OF THE KNEE-JERK

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Perhaps no other reflex presents so variegated a history as does the knee-jerk.[1] Because of the ease with which it may be elicited, the objective definiteness of the response, the relative simplicity of the neural mechanisms involved, and its usefulness to clinical neurology, it has been almost a constant object of clinical and experimental investigation since its discovery in 1875. The extent of these investigations is indicated by the comprehensive monograph of Sternberg,[2] published in 1893, in which over 500 titles are listed. The history of these investigations and those of the succeeding twenty-five years is of especial interest to students of neuro-muscular phenomena since it parallels and illustrates certain major technical developments in the study of these phenomena, and offers data pertinent to their fundamental problems. These developments are concerned especially with methods of registration of muscular response, particularly as they relate to the technique of the study of reflex temporal relationships, and to the problems of inhibition and facilitation.

Wilhelm Heinrich Erb (1840-1921) and Carl Friedrich Otto Westphal (1833-1890) independently described, in 1875, the phenomenon which occur when the patellar tendon is stimulated. In Erb's paper[3] the phenomenon is referred to as the "Patellarsehnenreflex," while Westphal[4] designated it as the "Unterschenkelphänomen." Grainger and Gowers appear to have been among the first of the English neurologists, and Charcot and his students among the first French neurologists, to investigate the phenomenon. In addition to the designations of Erb and Westphal, the following are some of the English terms used in referring to the reflex: "tendon phenomenon," "tendon reflex," "knee-jerk," "knee-reflex," "tendon jerk," and "patellar reflex."

Erb and Westphal immediately disagreed as to the essential nature of the newly discovered phenomenon. The former

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[1] This study was begun at Stanford University.

[2] M. Sternberg, *Die Sehnenreflexe und ihre Bedeutung für die Pathologie des Menschen*, 1893.

[3] W. Erb, *Ueber Sehnenreflexe bei Gesunden und Rückenmarkskranken;* cf. O. Westphal, *Arch. f. Psychiat. u. Nervenkr.*, 5, 1875, 792.

[4] O. Westphal, Ueber einige Bewegungserscheinungen an gelähmten Gliedern, *Arch. f. Psychiat. u. Nervenkr.*, 5, 1875, 803-834.

regarded the muscular contraction in response to the stroke on the patellar tendon as a 'true' reflex. In opposition to this Westphal regarded the response as being the direct mechanical response of the muscle to the stroke on its tendon. For Westphal a state of reflexly maintained tonus was essential for the appearance of the muscular response to the stroke on the patellar tendon. Brondgeest[5] in 1860 performed the fundamental experiment in which it was demonstrated that when a spinal frog is suspended the legs hang in a more relaxed condition if the afferent roots of the sciatic plexus have been cut, than when the nerve supply is intact. Johannes Müller and Marshall Hall had suggested previously that the muscles were always in a state of tension which was dependent upon a nervous connection. Gowers, Waller, and others suggested that a reflexly maintained muscular tension is essential to the appearance of the jerk.

It was urged against the reflex theory (1) that the time of the knee-jerk, *i.e.* the time between the stroke on the tendon and the beginning of the muscular contraction, is too brief to allow for conduction to and from the spinal cord; (2) that the knee-jerk does not show the characteristic 'purposiveness' of 'true' reflexes; (3) that the knee-jerk is not inhibited, but only affected through the action of antagonists; and (4) the knee-jerk is not affected by narcotic agencies.[6] Of these four, the major discussion centered about the time-relationships involved in (1). To understand this discussion it is necessary to review the interesting development of experimental techniques in the study of neuro-muscular phenomena—a development which is of significance for the understanding of present-day investigations in this field.

DEVELOPMENT OF EXPERIMENTAL TECHNIQUES IN THE STUDY OF TENDON PHENOMENA

The experimental techniques in connection with the study of the tendon responses have undergone an interesting metamorphosis. The method of Erb and Westphal—and the method used today in the neurological clinic—was simple; the subject was required to sit either with the leg swinging freely, or with the legs crossed, and the operator struck the patella a smart blow either with the side of the hand or with a percussion hammer. The height and rapidity of the resulting upward kick of the leg was noted by the operator. In the experimental studies instrumental aids became necessary in recording the contraction, and in standardizing the stimulus to be applied to the tendon.

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[5] P. J. Brondgeest, *Arch. f. Anat. u. Physiol.*, 1860, 703-704. A summary of the work, both early and recent, on tonus may be found in J. F. Fulton, *Muscular Contraction and the Reflex Control of Movement*, 383 ff.

[6] See Sternberg, *op. cit.*, 55.

In recording the knee-jerk, three indicators (to use the phrase of Dodge),[7] may be listed in the order of their historical development: (1) thickening of the muscles in the stimulated leg; (2) movement of the leg directly recorded; and (3) registration of the action current in the muscles by the galvanometric technique. Brissaud in 1880, ter Meulen, Jarisch and Schiff in 1882, and others used the registration of the thickening of the muscle as the indicator of the response.[8] Bowditch and Warren,[9] Lombard,[10] and others recorded directly the movement of the leg. The registration of the action current in the stimulated muscle by the galvanometric technique made possible the exact study of the time relationships. Among the first to make use of this technique in the study of knee-jerk was Dodge. Jolly and Snyder in 1910, Hoffman in 1922, and Golla and Hetwer in 1922 are among those who have used the string galvanometer. The direct registration of the movement of the leg may be accomplished by a system of levers and pulleys, which actuate electrical or pneumographic time-markers. Dodge, in his monograph, found this method the least satisfactory. Bowditch and Warren, however, made use of this technique in a study, the results of which are quoted in nearly every modern physiological text. The method of recording the thickening of the muscle has been adopted in many of the recent investigations, *e.g.* Miles,[11] and Dodge and Benedict.[12] It is interesting to note, however, that Tuttle[13] in a series of investigations in 1924 recorded the excursion of the leg by means of a lever system. In recording directly the thickening of the muscle, it is possible to obtain a record of the moment of stimulation of the tendon, since the blow of the stimulating instrument causes a slight mechanical twitch in the muscle which precedes the reflex response proper and is recorded on the kymographic records. This feature is an important advantage of this method.

The registration of the response has for its object the demonstration of one or more of the following aspects of the jerk: (1)

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[7] R. Dodge, A systematic exploration of a normal knee-jerk, its technique, the form of muscle contraction, its amplitude, its latent time and its theory, *Zsch. f. allg. Physiol.*, 12, 1911, 1-58.

[8] See Sternberg, *op. cit.*, for a review of this work.

[9] H. P. Bowditch and J. W. Warren, The knee-jerk and its physiological modification, *J. Physiol.*, 11, 1890, 25-64.

[10] W. P. Lombard, The variations of the normal knee-jerk and their relation to the activity of the central nervous system, this JOURNAL, 1, 1888, 8.

[11] W. R. Miles, Alcohol and human efficiency, *Carnegie Inst., Pub.*, 1924, No. 333.

[12] R. Dodge and F. S. Benedict, Psychological effects of alcohol: an experimental investigation of the effects of moderate doses of ethyl alcohol on a related group of neuromuscular processes in man, *Carnegie Inst., Publ.*, 1915, No. 232.

[13] W. W. Tuttle, Apparatus for eliciting and recording the patellar tendon reflex, *Amer. J. Physiol.*, 68, 1924, 338.

the time between the stimulus and the beginning of the contraction—the reflex time; (2) the form (height, steepness of ascent, etc.) of the contraction curve; and (3) the duration of the contraction. The various methods of registration described emphasize one or more of these factors.

It is essential that the form of stimulation be standardized. A percussion hammer is usually used and may be actuated by a coiled spring or electrically released from above the knee and actuated by gravity. It is probable that the latter method secures the most satisfactory mechanical control.

TEMPORAL RELATIONSHIPS IN THE TENDON PHENOMENON

In 1875 the discussion regarding the reflex nature of the newly discovered tendon phenomenon chiefly centered about the time relationships. It was asserted by those who adhered to Westphal's point of view that the jerk was not a true reflex, *i.e.* did not involve neural conduction to and from the spinal cord, because the time was too brief to permit such transit. Foster, in 1897, some twenty years after the discovery by Erb and Westphal, gives perhaps the best statement of this view.

> "If the time which elapses between the blow on the tendon and the beginning of the movement be measured, this is found to be very short, .02 sec., far shorter than that of any known reflex act; and indeed if the measurement be made between the tap and the very beginning of the contraction of the muscular fibres themselves (as may be done by a special arrangement), the interval is actually less than that which occurs when the contraction is brought about by stimulating the motor nerve at some distance from the muscle. It seems impossible, in view of this fact, to regard the act as a reflex act; and we are led to the conclusion that the contraction is idiomuscular, is brought about by the vibrations started in the tendon running up to the muscle and directly stimulating the muscular fibres, and not by nervous impulses making the reflex circuit through the spinal cord."[14]

Foster proceeds to state, however, that the irritability of the muscle and its readiness to respond to direct stimulation is dependent upon the spinal cord, *i.e.* the muscle is in a state of tonus which is maintained by a nervous arc involving the cord.[15]

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[14] M. Foster, *A Textbook of Physiology, Part III: The Central Nervous System*, 7th ed., 1897, 999.

[15] Cf. C. S. Sherrington, Note on the knee-jerk and the correlation of action of antagonistic muscles, *Proc. Roy. Soc.*, 52, 1893, 556-564. In this paper Sherrington demonstrated that the knee-jerk could be *inhibited centrally like other spinal reflexes.* He says: "It would thus seem clear that the exaggeration of the knee-jerk produced by severance of the branches given from the great sciatic nerve to the hamstring muscles is not due to the fact that the resulting relaxation of those muscles simply leaves the joint mechanically more free to move. The exaggeration would seem due rather to the severance of the nerves in question interrupting a stream of centripetal impulses that passes up from the hamstring muscles and enters the spinal cord by certain afferent roots, and in the cord exerts a depressing or restraining influence on the jerk . . . . Further, it would seem that at the knee joint excitation of the afferent fibres coming from one set of the antagonistic muscles induces reflex tonic contraction of the opposing set with extreme facility, despite the fact that the opponent muscles are not innervated from the same spinal segments . . . Thus the degree of tension in one muscle of an antagonistic couple intimately affects the degree of 'tonus' in its opponent, not only mechanically, but also reflexly, through afferent-efferent channels and the spinal cord" (p. 563). This is strong evidence in favor of the reflex nature of the jerk.

In the period from 1875 until 1895 a number of investigators measured the time of the tendon phenomenon, that is, the time elapsing between a blow on the patellar tendon and the beginning of the muscular contraction. Table I summarizes these findings.[16]

TABLE I

Investigator	*Date*	*Time* (sigma)
Tschirjew	1879	58–61
Gowers	1879	15–90
Brissaud	1880	50
ter Meulen		35–90
Waller	1880	30–40
James	1880	25
Eulenberg	1882	24
De Watteville	1882	20
Rosenheim	1884	31–33
Jendrassik	1894	23

Exner[17] in 1874 had measured the time of the wink-reflex in response to an electrical stimulus, obtaining values between 57 and 66 sigma. From these times Exner subtracted the conduction time in the nerve and the time of the muscle latency. He thus obtained the 'reduced reflex time' between 47 and 55 sigma.

Although there were great differences in the times obtained by Tschirjew, Gowers and others in measuring the tendon response—due in part, of course, to differences in methods used—it is clear that these times are of an order of magnitude in general much below that obtained by Exner in measuring a 'true' reflex. Waller, in discussing the times that he obtained, states in 1890, that in man any reflex movement of the leg must take 'at least' 100 sigma. Exner's are less than this, but Waller says:

> Moreover it may be urged that we do know that Exner's reflex, viz., the closure of eyelids consequent upon stimulation of the conjunctiva, which is the nearest approach we know in man to a pure reflex, is only .05 second. This item does not, however, seem to me to incline the balance in either direction; it is the shortest known 'reflex time,' and it is the reaction of a very mobile muscle of small bulk, via very short channels of innervation. Admitting the measurement (and I have verified it) I do not think that it weighs either way; it is longer than the 'direct' intervals quoted above, but not sufficiently so to contrast with them; it is probably shorter than a true reflex time of the leg or thigh but not sufficiently short to allow us to admit a rectus femoris reflex time of only .03 second. In sum it cannot be used as evidence in either sense.[18]

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[16] Cf. Nagel, *Handbuch*, IV, 1909; Sternberg, *op. cit.*, 29 ff.; E. Zeising, *Ueber das Kniephänomen mit specieller Berücksichtigung des normalen und pathologischen Verhaltens desselben im Kindersalter*, 1887, 15 ff.; H. Netter, *Zur Geschichte der Lehre vom Kniephänomen bei Geisteskranken nebst Beobachtungen über diesen Gegenstand aus Freiburger Psychiatrischer Klinik*, 1897; T. Rosenheim, Experimentelle Untersuchung der unter dem Namen "Sehnenphänomene" bekannte Erscheinungen unter möglichster Berücksichtigung von Versuchen an Menschen und pathologischen Beobachtungen, *Arch. f. Psychiat. u. Nervenkr.*, 151, 1884, 184-201.

[17] S. Exner, Experimentelle Untersuchung der einfachsten psychischen Processe, *Pflüger's Arch.*, 7, 1873, 601–660; 8, 1874, 526–537.

[18] A. Waller, On the physiological mechanisms of the phenomenon termed tendon reflex, *J. Physiol.*, 11, 1890, 387 f.

Waller believed, then, that the tendon phenomenon was not a true reflex, but that the integrity of the reflex arc was a *sine qua non* condition. Data obtained from experimentation on rabbits confirmed these results. Waller found that the times for the tendon phenomenon in the rabbit were about 8 sigma, while 'true' reflex contractions, obtained by stimulating the skin of the same animal, were found to take place in about 33 sigma.

Accurate timing of reflex phenomena was difficult with the techniques used by these investigators. After pointing out the need of more accurate timing methods, Sternberg indicated that positive experimental proof of the nature of the tendon phenomenon was not available, since too much weight was not to be attached to the short latencies.

> Es ist daher dem Argumente der zu kurzen Reflexzeit nur ein geringes Gewicht beizumessen. Man muss sich sagen, dass wir uns eben mit der Thatsache befreunden müssen, dass so einfache Reflexe, wie die Sehnenreflexe, bei denen keine Coordination von Muskeln in bestimmter Reihenfolge nöthig ist, wie das etwa beim Zurückziehen eines Beines der Fall ist, in ganz kurzer Zeit verlaufen können. Auch ist eine so kurze Reflexzeit für den wahrscheinlichen Zweck der Sehnenreflexe erforderlich.
>
> Wiewohl also ein exacter experimenteller Beweis nicht gegeben ist und höchstwahrscheinlich überhaupt nicht gegeben werden kann, spricht die weitaus grössere Wahrscheinlichkeit dafür dass das Muskelphänomen beim sogenannten Sehnenreflexe in gleicher Weise wie das Knochenphänomen als ein Reflexvorgang aufzufassen ist.[19]

With the use of the galvanometric technique with photographic record of the thread deflections by Jolly[20] and Snyder[21] in 1910 and 1911, the fact was demonstrated that the time between stimulus and response in the case of the knee-jerk is theoretically long enough to allow for conduction to and from the spinal cord. Dodge had already demonstrated that the time of the responses of the muscle to direct stimulation is shorter than the times obtained for the knee-jerk by the earlier investigators. He was able to obtain times in the neighborhood of 15 sigma in the case of the extirpated frog's muscle. Jolly in 1911, using the galvanometric technique, obtained times in the neighborhood of 6 sigma for the knee-jerk in the spinal cat. In the case of man the same observer found the average of 14 observations on 3 subjects to be 20.5 sigma, 22.7 sigma, and 20.7 sigma respectively. Snyder found an average time of 11.3 sigma in man. Jolly pointed out that "it is, therefore, necessary to reconsider the question as to whether the latent period of the knee-jerk is long enough to include the various times occupied in reflex action."

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[19] Sternberg, *op. cit.*, 56.

[20] W. A. Jolly, The time relations of the knee-jerk and simple reflexes, *J. Exper. Physiol.*, 4, 1911, 67-88; Reflex times in the South African clawed frog, *Proc. Roy. Soc.*, 92B, 1921, 31-51.

[21] C. D. Snyder, The latency of knee-jerk response in man as measured by the thread galvanometer, *Amer. J. Physiol.*, 26, 1910, 475-482.

These 'times' are (1) latency of the afferent endings; (2) time of conduction from the vastus internus and part of the crureus divisions of the quadriceps to the cord by the anterior crural nerve, and back by the same nerve-trunk to the muscle; (3) latent period of the electrical change in the muscle; and (4) time occupied at the synapse.

Analyzed on this basis, the total time of the response in the spinal cat yields the following, according to Jolly.

Total latency of jerk		6.6
Latency of afferent endings	.97	
Nerve conduction	1.45	
Latency of motor endings	2.38	
	4.80	4.8
Synapse time		1.8

It is notable that the times found for man by Jolly were very similar to those offered by Waller as proof that the tendon-phenomenon was not a true reflex. It must be pointed out that the distribution of times presented above represents the situation only under a certain set of experimental conditions.

In 1921, Jolly studied the times for homonymous and heteronymous reflexes in the frog, with the view of finding the differences between the two types and thus "to obtain a measure of the delay in transmission of the reflex impulses in the spinal cord, or 'synapse time.'" Spinal and decerebrate animals were used and the time measurements were obtained by means of the Einthoven string galvanometer. The average time for 41 cases of the heteronymous reflex was 15.7 sigma, while the average for 40 cases of the homonymous reflex was 14.3 sigma. Regarding the difference Jolly says:

> I do not think that the above experiments taken by themselves would justify us in assuming that in the spinal frog at the temperatures employed, and with the frogs in good condition, the intraspinal path of the heteronymous reflex involves a larger number of synapses than that of the homonymous reflex. . . . . It would rather appear that we are dealing with mechanisms involving the same number of neurones on the two sides. . . . The fact that the homonymous time is usually slightly the shorter may possibly be due to the motor neurones responding rather more readily to stimuli reaching them through afferent nerves from the same side of the body.[22]

Jolly analyzed the total time as follows:

> If we select a reflex time recorded at a room temperature of 23 degrees C. from a frog in good condition, first response, thirty-five days after operation, we find an interval of 11.4 sigma for the total reflex time. The length of nerve from toes to cord and from cord to thigh was found post mortem in this frog to be 16 cm. The rate of conduction in the nerve of Zenopus at 23 degrees C. was determined in the usual manner by electrical stimulation at two points, with the electrical change in the muscle as in-

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[22] Jolly, *op. cit.*, 35.

dicator, and was found to be 37 m. a second. The latency of the muscular response to stimulation close to the muscle was found to be 2.4 sigma. If we allow one sigma for the latency of sensory nerve endings we may suggest the following analysis:[23]

Total reflex time		11.4
Nerve conduction	4·3	
Muscle latency	2.4	
Latency of sensory endings	1.0	
	7·7	7·7
Synapse time		3·7

The same type of temporal analysis is exemplified in the work of Hoffman[24] who has studied the tendon-reflex (which he terms "Eigenreflexe") and in his monograph of 1922, he characterized these temporal elements as follows:

L = the conduction time in the nerve (*Leitungszeit*)
I = the length of the nerve
N = the rate of conduction (*Leitungsgeschwindigkeit*)
M = the latency of the muscle
S = the latency of the end-organ
R = the 'true' reflex time

The total reflex time ('Z'), then, is equal to R + M + L + S. Hoffman emphasized the importance of determining the value of N (the rate of conduction). This value varies, *i.e.* it is dependent upon the *length of nerve.* The relationship is one in which the rate decreases as the length of the nerve increases. Hoffmann arrived at this conclusion by dividing the length of nerve expressed in cm. by the total reflex time (I/Z).

> Je grösser der Nervenweg ist, der beim Reflex durchlaufen werden muss, um so geringer ist die Brutto-geschwindigkeit des Prozesses I/Z. Mit anderen Worten, wenn man von Muskellatenz, Latenz der sensiblen Endorgane, und reiner Reflexzeit ganz absieht, so ist die Leitungsgeschwindigkeit um so langsamer, je grösser die Nervenstrecke ist, die durchlaufen werden muss. Dies gilt für alle Möglichkeiten, den Eigenreflex zu messen, mit Ausnahme des Masseterreflexes. Hier ist die Nervenleitung so kurz, dass die Muskellatenz und die reine Reflexzeit schon entscheidend ins Gewicht fallen.[25]

In comparing the gross velocities (I/Z) in 5 cases with a short reflex arc (170 cm.) with those having a long reflex arc (190 cm.), he found the velocity in the first to be 56.7 m. per sec., and in the second 49.3 m. per sec.

The total time, according to Hoffmann, for any reflex remains relatively constant in the normal individual; in the diseased individual this time is lengthened. The rate of conduction in the case of disease is slower than in the case of health. The following table indicates the values of I/Z for the Achilles tendon reflex in certain diseased conditions.

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[23] *Op. cit.*, 37.

[24] P. Hoffmann, *Untersuchungen über die Eigenreflexe (Sehnenreflexe) menschlicher Muskeln*, 1922. Hoffmann points out that the tendon plays no essential part in connection with the release of the reflex; the end-organs lie within the muscle itself. Every muscle has an *Eigenreflex* which, according to him, is purposeless and isolated from other types of reflex activity.

[25] *Op. cit.*, 47.

In internal diseases (inneren Krankheiten)	50.1 m. per sec.
Organic disease of the central nervous sytem	51.5 ,, ,, ,,
Other neuroses	49.6 ,, ,, ,,
Functional nervous diseases	59.9 ,, ,, ,,

Hoffmann regards the tendon-reflex as occupying a peculiar position in that there is only one synapse involved in the spinal cord. The time occupied in the cord is about one sigma.

The analyses of Hoffmann, Jolly, Snyder, and other contemporary investigators of the time relationships of the knee-jerk, tell a dramatic tale of progress in experimental technique. Johannes Müller in the middle of the 19th century had prophesied that the velocity of the nervous impulse would never be measured, but lived to see his student, Helmholtz, measure it in 1850. The measurements in 1879 and 1880 of the time of the knee-jerk made possible only comparatively rough approximations of the duration of the various part processes composing the reflex.

Waller[26] in 1890 summarized under seven heads the evidence bearing on the reflex nature of the tendon phenomenon. These are (1) lost time of the tendon-phenomenon as compared with the lost time of direct and of reflex muscular contractions; (2) lost time of tendon-phenomenon, in muscles at different distances from the cord; (3) clinical observations; (4) experimental section of nerves and of nerve roots; (5) destruction of the spinal cord; (6) muscular tonus; and (7) crossed tendon-reflexes. The evidence from all of these fields of investigation except (1) and (2) indicate that a reflex arc is essential for the knee-jerk. In the opinion of Waller the evidence from the measurement of the time relations tends to indicate that the time is too brief for a reflex to take place, and hence the phenomena are not reflex, but are dependent on the reflexly maintained muscular tonus. The data which have been presented by Dodge, Jolly, Snyder, Hoffmann, and others, seem to indicate that this objection of Waller's has yielded before improved methods of temporal measurement in physiology and psychology.

Dodge in 1910 presented evidence which tended to establish further the reflex nature of the knee-jerk. This evidence may be summarized: (1) the course of the wave of muscular contraction was found to proceed from the central end of the muscle rather then from the distal end, as might be expected if the contraction were the result of direct mechanical stimulation;

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[26] Waller, *op. cit.*

(2) the length of the reflex arc involved increases the total time occupied by the reflex, *e.g.* the latent time of the knee-jerk is from 11 to 18 sigma; shorter than that of the Achilles tendon response; (3) the knee-jerk may take place only as a result of coördination of flexor and extensor muscles; a nerve center is essential to such coördination, and hence a reflex arc is implied.[27]

VARIABILITY IN TENDON REFLEXES

The enormous variability of the tendon-reflexes has been a subject of comment by practically every investigator of these phenomena. It was usual in 1875—as it still is in many quarters—to regard the reflex as a stable and relatively unmodifiable reaction. The possibility that the course of the reflex is modified by simultaneously acting neuro-muscular mechanisms suggested itself as an explanation of the observed variability. The phenomena of facilitation and inhibition point to the existence of such functionally coöperative systems. *Bahnung* and *Hemmung* as objects of scientific investigation had antedated, of course, the discovery of the knee-jerk.[28]

The study of the *cortical influences*[29] as factors in the variability of the knee-jerk is perhaps the most significant advance from the point of view of the development of reflex theory.

Jendrassik[30] in 1885 noted the differences in the percentages of failure to elicit the knee-jerk as reported by different investigators. Berger had reported 1.56% failure among his subjects, Eulenberg 4.8%, Block .72%, Pelizaeus .04%. Jendrassik suggested that these differences might be due to the techniques used by the investigators, and suggested that if some other muscle groups were thrown into contraction *simultaneously with the knee-jerk*, the latter would be exaggerated.

> Zu diesem Zweck benutzte ich meine Erfahrung, nach welcher die Sehnenreflexe, besonders aber das Kniephänomen sich bedeutend steigern, wenn wir mit den übrigen Muskeln unseres Körpers grosse Kraft erzeugen. So, z. B., fällt das Kniephänomen viel stärker aus, wenn wir die Muskeln der Arme stark anspannen. Ich setze das betreffende Individuum, welches mit der gewöhnlichen Methode kein Kniephänomen hatte, auf den Rand eines Tisches mit möglichst erschlafften Beinen, and während ich auf seine Patellarsehne klopfe, fordere ich es auf, die gebeugten Finger der rechten und linken Hände in einander auszuhängen und sie bei nach vorne ausgestreckten Armen so stark als möglich auseinander zu ziehen.[31]

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[27] Cf. Sherrington, *op. cit.*

[28] Cf. W. H. Howell (Inhibition, *Physiol. Rev.*, 5, 1925, 161-181) for a recent review of the theories of inhibition. The monograph by J. F. Fulton (*Muscular Contraction and the Reflex Control of Movement*) contains an excellent critical review of the recent work on reflex inhibition (see especially 272 ff., 315 ff.).

[29] Cf. F. Fearing (A critique of the experimental studies of cortical inhibition with special reference to the knee-jerk, *J. Comp. Psychol.*, 7, 1927, 285-296) for a review of these factors.

[30] E. Jendrassik, Zur Untersuchungsmethode des Kniephänomens, *Neurol. Centralbl.*, 1885, 412.

[31] *Op. cit.*, 413.

The "Jendrassik Handgriff" became a standard clinical device. In 1886 Mitchell and Lewis,[32] making a study of the circumstances which increase or decrease the knee-jerk, noted a reënforcement of the jerk with simultaneous voluntary action, painful stimulation of the skin, and visual stimulation.

Lombard in 1888 studied the variability of the normal knee-jerk, with reference to the effects of fatigue, hunger, music and voluntary movement. He summarized his conclusions as follows:

> The causes of these variations of the knee-jerk are not only alterations in the muscles and nerves involved in the process, but, to a still greater degree, changes in the activity of the central nervous system, either as a whole or in part. Thus fatigue, hunger, enervating weather and sleep, conditions which decrease the activity of the whole central nervous system, decrease the average knee-jerk, while rest, nourishment, invigorating weather, and wakefulness, influences which increase the activity of the central nervous system, increase the average knee-jerk. . . . Thus voluntary movements and strong emotions, when synchronous with the blow, are found to increase the movement; and this is noticed even during sleep when the dreams are vivid. Similarly, sensory irritations, even when not strong enough to produce visible reflex actions, markedly reinforce the knee-jerk, but whether on account of their effect upon the brain, or upon the spinal cord, must be proved by future experiments.[33]

INVESTIGATION BY BOWDITCH AND WARREN

The experiments of Bowditch and Warren[34] published in 1890, mark the most ambitious attack on the problem of facilitation and inhibition of the knee-jerk undertaken up to that time. The present importance of this investigation is evidenced by the fact that the conclusions are quoted as definitive by many modern workers. They appear in such physiological texts as those of Howell, Luciani, Ladd and Woodworth, and Schäfer. The wide-spread acceptance of the results of Bowditch and Warren as well as their theoretical importance warrants a reëxamination of the data and conclusions.

Mitchell and Lewis had noted that "the muscular action . . . . must precede the tap [on the knee], in order to reenforce it, by a period which is, as yet, undetermined." This suggested to Bowditch and Warren the importance of making a study of the effect of varying the interval between the reenforcing act and the stimulation of the patellar tendon. The reënforcing act consisted not only of a voluntary muscular contraction, but also of sensory stimulation—visual, auditory, and cutaneous. The muscular reënforcing act was a voluntary clenching of the hand in response to a bell stimulus; in the case of vision it was a flash of light caused by the sudden opening and closing of a stop-cock in a gas pipe supplying a burner in a lantern; the auditory stimulus was produced by dropping a large paper torpedo "near the head of the individual experimented upon," and the reënforcing cutaneous stimuli consisted of blasts of air directed to the conjunctiva, the mucous membrane of the nose and the skin of the neck.

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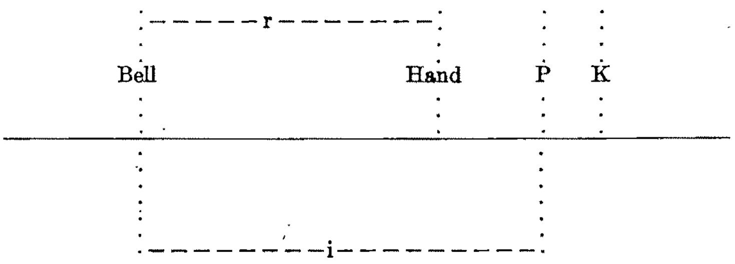
[32] S. W. Mitchell and M. J. Lewis, Physiological studies of the knee-jerk and of the reactions of muscles under mechanical and other excitants, *Phil. Med. News*, Feb. 13, Feb. 20, 1886.

[33] Lombard, *op. cit.*, 68.

[34] Bowditch and Warren, *op. cit.*

The subject was placed on his side and the patellar hammer attached to the leg. The movement of thc leg during the kick was recorded on a smoked surface by means of a system of levers attached to the foot. The apparatus was so arranged that the experimenter was able to control electrically the time between the reënforcing or auxiliary stimulus and the tap on the tendon. The reënforcing stimulus *always* preceded the tap.

The time sequence in the experiment may be represented schematically by the following figure.

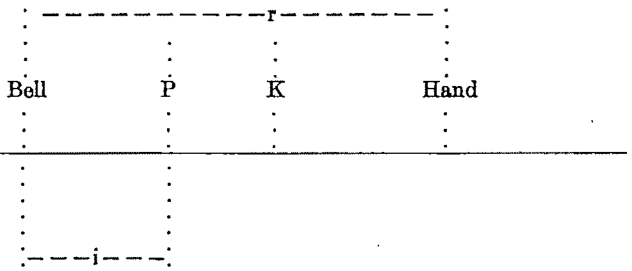


In the series in which a voluntary muscular contraction was the reënforcing stimulus, the subject was instructed to clench the right hand on a piece of wood in response to a tap on a bell. This is a voluntary reaction the time of which is designated as 'r.' The experimenter by means of the apparatus controls the time elapsing between the bell and the tap on the knee, 'P;' this time is designated as 'i.' The object of the experiment, then, is to determine the effect on the extent (amplitude) of the knee-jerk by varying the interval 'i.' Each series of experiments lasted about an hour during which varying intervals of 'i' were used interspersed by experiments in which there was no reënforcing act. These were called "normal" and the differences between the extent of the knee-jerks in the "normal" and reënforced series gave a measure of the reënforcing effect.

Tabulation of these differences according to the value of the interval 'i' showed "clearly that if the blow [on the knee] follows the signal [bell] at an interval not greater than 0.4 sec. the reënforcing act increases the extent of the knee-jerk. If the interval exceeds this amount a diminution of the knee-jerk results." If the interval 'i' was prolonged to 1.7 sec. the reënforcing act was without effect. From this the authors conconcluded:

> When by a brief act of volition the muscles of the forearm are innervated, the spinal cord is thrown into such a condition, that that portion of it which is concerned in the production of the knee-jerk is for a short time in a state of exalted activity, which is succeeded by a period of depression and then by a slow return to the normal state.[35]

The authors note that the value of 'r' is "always subject to considerable variation" and proceed to study the data when they are tabulated on the basis of 'i'–'r,' *i.e.* the interval of the time between the *beginning* of the reënforcing act and the blow on the knee. The authors note that the value of 'i'–'r' may be a minus quantity "in certain cases," *i.e.* the blow on the knee may have fallen *before* the subject has reacted to the bell stimulus. "In certain cases" seems a scarcely adequate characterization, since there are grouped in this category nearly half the measurements. The time sequence in these cases may be represented schematically as follows:



Inspection of the published data indicate that the tap on the knee, 'P,' may take place, in the cases where "the individual failed to reaction promptly to the signal," as much as 160 sigma before the clenching of the hand; since the time of the knee-jerk, 'P–K,' as measured by this technique cannot have been more than 70 sigma, it is clear that the knee-jerk was frequently completed *before the act which the experimenters had intended to to reënforce it.* In these cases the greatest amount of facilitation, as indicated by the extent of the knee-jerk, occurs.

Since the experiments of Bowditch and Warren are in a field which may be expected to yield much that is of major importance to both physiology and psychology, and since their conclusions are widely quoted in contemporary texts, a critical revaluation of their technique and results is in order. While such a critique is not the purpose of the present paper,[36] it may be pointed out in passing, however, that there is a possibility of the existence of factors operating to modify the amplitude of the knee-jerks other than the auxiliary muscular act itself.[37] The process of attention suggests itself in this connection.[38] This process, whether defined in terms of behavioristic psychology as a form of sensory adjustment which increases the effectiveness of stimulating conditions, or in terms of structural psychology as an increase in sensory clearness, might be expected to bring about nervous and muscular changes which would modify a coincident reflex, *e.g.* the knee-jerk. At any rate, the events in the Bowditch and Warren experiment, which occurred between the bell signal and the voluntary response thereto, would seem to be of primary importance in experiments of this type. Stated briefly, the question is: what was the attentional state of the subject just before and during the tap on the knee?

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[35] *Ibid.*, 38.

[36] Cf. Fearing, *op. cit.*

Sternberg is of the opinion that simultaneous voluntary muscular action exaggerates the knee-jerk, since it serves to direct the attention of the subject away from the knee; when the attention is directed towards the knee there is a tendency towards a voluntary contraction of the same muscle which is involved in the reflex. This, according to Sternberg, inhibits the reflex contraction.[39]

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[37] That is in the intact nervous system. In this connection the following quotation from a recent paper by Dodge (*Psychol. Rev.*, 33, 1926, 3) is pertinent: "Restricted in the method of direct experimentation to techniques that may be applied to intact and complex nervous systems, the experimental psychologist is peculiarly dependent on the results of physiological experiments with isolated tissues and simple systems both for his working hypotheses and his elementary facts. But he must take scientific responsibility for testing the extension of these hypotheses and facts to the complex conditions of human behavior, experience, and personality . . . . While it is often regarded as unfortunate that nerve fibers, nerve muscle preparations, and reflexes cannot be functionally isolated in the intact human on demand, it may not be forgotten that it is exactly in the mutual interrelationships of the various neural strata in human behavior and the integration which is called consciousness that there lies a group of problems of the utmost practical importance."

[38] E. B. Twitmyer, *A Study of the Knee-jerk*, 1902, suggests attention as an explanation of the observed variability in the knee-jerk. He says: "When the patella tendons are struck at exactly the same place with blows of constant force and at regular intervals, no two of the resulting knee jerks are of the same extent. This variation is usually referred to some accidental stimulus acting upon the subjects, e.g., a loud or distracting sound or to an idea to which the subject direct his attention, an emotional state of greater or less intensity any one of which conditions may produce a variation in the extent of the jerk, or in a general way to a change in the activity of the central nervous system. From the very nature of the case such sources of variation are not wholly avoidable. However secure the subject may be kept from accidental sensory stimuli, the stream of consciousness is never altogether within control of the experimenter and the organic processes of digestion, circulation, etc., are constantly producing some slight or more profound modification in the equilibrium of the nervous system" (p. 8 f.) Dodge in 1911 noted that attention "to some arbitrarily selected object in the environment diminished the jerk and rendered it relatively constant" (*op. cit.*, 25).

[39] Hoffmann, in his monograph on the "Eigenreflexe," *op. cit.*, objects to the conclusion of Sternberg that voluntary contraction of a muscle precludes its simultaneous reflex contraction, *i.e.* inhibits the reflex. Using the galvanometric technique and registering the action current in the muscle, Hoffmann shows that a simultaneous voluntary contraction of the extensors (*Agonisten*) facilitates the reflex, while a simultaneous contraction of the flexors (*Antagonisten*) inhibits the reflex.

The study by Twitmyer (1902) is one of the earliest attempts to study the variability of the "unaugmented or normal knee-jerk" in human subjects. Twitmyer found a wide range of individual differences in the amplitude of the kick (0–165 mm.). He noted that subsequent to the kick there are swings of the leg which are not to be ascribed to the pendular swings of the leg coming to rest. These after-kicks frequently exceeded the knee-jerk itself in amplitude and tended to increase in extent as the knee-jerks are repeated. Twitmyer suggested the following hypothesis which is significant as a contribution to the theories of knee-jerk variability.

> Let us assume that the repeated transmission of reflex excitation along a reflex arc, including the spinal segment, an afferent and efferent conduction path or from a given segment in the cord to a segment somewhat removed, develops a pathway offering increasingly less resistance. . . . A blow of constant force on the tendons will then result in a more and more intense stimulation of both the quadriceps and its antagonist. This increase will be relatively constant, consequently the extent of the initial kick is still checked by the simultaneous stimulation of the hamstring muscles. The actual extent of the kick, however, tends to increase somewhat with the continuation of experimentation. With the increased intensity of the reflex influence, the flexors are thrown into more violent contraction, which contraction now becomes an adequate stimulus to the sensory fibers terminating in the flexors and as a result there is a vigorous excitation to the cord and ultimately to the quadriceps. This excitation therefore contributes its quota to the second outward excursion of the leg and frequently is sufficiently vigorous to produce a kick larger than the initial kick. This view is fully corroborated by Sherrington's experiment in which he succeeded in greatly augmenting the extent of the knee-jerk by merely compressing the hamstring muscle when it is completely dissected away from its attachments, while the nerves were still intact.[40]

Twitmyer's study is especially interesting since it contains one of the first recorded observations on the phenomenon later included under the term "conditioning." He found that there appeared a decided kick of both legs following "a tap of the signal bell occurring without the usual blow of the hammer on the tendons."[41]

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[40] Twitmyer, *op. cit.*, 24.

[41] Because of their historical significance, Twitmyer's conclusions in this connection are worth quoting in full: "Can these movements, then, be looked upon as the true knee-jerk phenomenon in which the appropriate stimulus, *i.e.* a blow on the tendons has been replaced by a new and unusual stimulus? These movements are not the result of voluntary effort. Each subject gave unqualified testimony on this point. This testimony is further corroborated by the fact that the subjects were kept in absolute ignorance as to the nature of the experiments about to be performed and consequently did not know when to expect the blow and when not to expect it. . . . To the observer these movements resembled the knee jerk in every detail. The characteristic jerky or explosive appearance was very evident. . . . The occurrence of the phenomenon, therefore, depends upon the preliminary simultaneous occurrence of the sound of the bell with the kick produced in the usual way, *i.e.* a blow on the tendon. After a certain number of trials, the number varying for different subjects, the association of the sound of the bell and the kick becomes so fixed that the bell itself is capable of serving as a stimulus to the movement. Physiologically the repeated association of the functioning of the motor cells in the lumbar segment of the cord, upon which the kick immediately depends, with the excitation of centers in the nuclei of the medulla connected with the auditory conduction path, has resulted in developing a fixed relationship between them. . . . The results of the experiments herein reported, however, would seem to indicate that it is only after a habit of interaction between the two involved centers has been developed by repetition, *i.e.* when the connecting pathway of discharge has become well worn, that the sound of the bell alone is an adequate stimulus to the movement."

RECENT EXPERIMENTS ON THE PROBLEM OF VARIABILITY IN THE KNEE-JERK

The investigation by Bowditch and Warren was the first of the attempts to investigate the problem of *Bahnung* and *Hemmung* in the knee-jerk, in man. Among the contemporary investigators should be mentioned Tuttle, who, in a paper in 1924,[42] presented the results of a study of the effects of mental activity on the knee-jerk. Tuttle was especially concerned with the effects of attention:

> In view of the paucity of quantitative data on this subject it was decided to measure the knee-jerk under active and passive conditions of highly socialized forms of peripheral stimulation. We did not feel free to hazard a definition of what psychologists have generally agreed to include under the term "attention" or "mental activity" but preferred to assign to the subject a problem which we felt reasonably sure would release the type of response which most psychologists would agree required "attention" or was performed under "attentive attitudes."[43]

The general method of attack was to obtain records of tendon responses to uniform blows administered at a regular rate while the subject was in a passive condition as regards attention, alternated with periods of mental work. The mental work consisted of mental arithmetic, conversation, mental tests, and, in the case of the one subject, attention to a problem in electrical wiring. The conclusion of the investigator may be stated in his own words.

> The records from all subjects studied show that mental activity as defined in this paper increases muscle tonus as exhibited by the knee-jerk. . . . If mental activity of the problem type as used in this experiment involves the psychological factors of attention, then the conclusion seems justified that muscle tonus as measured by the knee-jerk is either (a) one of the factors in the attentive process, (b) a function of the attentive process, or (c) the process of attention itself.[44]

It would have seemed advisable to attack the problem at a simpler level; such mental activities as adding, solving complex problems—and even carrying on a conversation—involve a complex of factors which are difficult to isolate.

Tuttle and Burtt,[45] using Tuttle's technique for eliciting and recording the knee-jerk, studied the effect of emotional conditions on this reflex. Pleasant, unpleasant, and affectively indifferent words were visually presented to the subject who was required to react by giving an associated word. The object of the study was to determine the effect on the amplitude of the knee-jerk of the various types of words presented. The conclusions of the investigators are stated as follows:

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[42] W. W. Tuttle, The effect of attention or mental activity on the patellar tendon reflex, *J. Exper. Psychol.*, 7, 1924, 401-419.

[43] *Ibid.*, 401.

[44] *Ibid.*, 418.

[45] H. E. Burtt and W. W. Tuttle, The patellar tendon reflex and affective tone, this JOURNAL, 36, 1925, 553-561.

> There is a depression of the reflex on the average for unpleasant stimulus words amounting to 16%. There are slight and less consistent indications of a similar depression for pleasant stimulus words. All the unpleasant categories used were effective in some instances in producing depression. . . . The correlations between the extent of a reflex and the association reaction-time for the stimulus word presented in connection with it are small, but all those that are of possible statistical significance are negative. This result suggests that the depression of the reflex and the slowing of the association time involve the same aspect of the stimuli, presumably the affective tone.[46]

In these experiments the subject reacted in every case *before* the knee was stimulated. The assumption was, presumably, that the subject's attention during the interval between the vocal response and the tap on the knee, was occupied with the word or with his response. In any event this period was critical so far as the knee-jerk is concerned. In spite of these objections these experiments present significant results. The conclusion cannot be escaped that mental processes or the neuro-muscular accompaniment of such processes included under the term attention significantly modify the knee-jerk.

A consideration of these problems leads, of course, to the larger problem of inhibition as it involves all neuro-muscular performances.[47] The field of inhibitional theory has become highly controversial, and as yet no one theory may be said to have behind it the weight of majority opinion. The 'drainage' hypothesis of McDougall has received attention from the psychologists. This hypothesis was presented by McDougall in 1903,[48] and assumes that "inhibition of the excitation of one neural system is always the result of the excitement of some other system, that inhibition appears always as the negative or complementary result of a process of increased excitation in some other part."

Dodge used the knee-jerk in testing the 'drainage' hypothesis of McDougall. He stated the question to which the experiment should give a specific answer as follows:

> Is it or is it not a fact that decrement of a reflex regularly occurs as a correlate of increased spread of neural excitation? Is it or is it not a fact that spread of excitation so as to involve the cortex or the initiation of a cortical reaction actually inhibits a related reflex? . . . If the in-

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[46] *Ibid.*, 561.

[47] The recent papers of E. D. Adrian (*Brain*, 47, 1924, 399-419) and C. S. Sherrington (*Proc. Roy. Soc.*, 97B, 1924-25, 528) contain summaries of the facts and theories in connection with inhibition.

[48] W. McDougall, The nature of the inhibitory processes within the nervous system, *Brain*, 26, 1903, 153.

hibition of a reflex by drainage actually follows the spread of excitation in the cortex, then it ought to be possible to inhibit a reflex by voluntary reaction to the same stimulus that normally excites it.[49]

To test the last statement, Dodge made use of the patellar reflex and the reflex eyelid reaction. He caused his subject in the case of the knee-jerk to respond by a voluntary finger movement to the same stimulus which released the knee-jerk. His assumption is that the knee-jerks which were accompanied by the simultaneous voluntary reaction would be consistently diminished in extent if the 'drainage' theory held. The results, for the few cases presented, were uniformly negative for the 'drainage' theory. Additional experiments are undoubtedly necessary in order to test the 'drainage' hypothesis. Wiersma[50] has recently pointed out that simultaneously functioning systems, *e.g.* the postural reflexes, may be expected to modify the knee-jerk; that is, those conditions which inhibit the postural reflexes facilitate the knee-jerk.

Golla and Hetwer[51] are among the contemporary investigators who have noted the variability of the magnitude of the knee-jerk. They believe that the greatest factor contributing to this variability in the intact human nervous system is cortical activity. This factor, they believe, may be rendered negligible by taking "suitable precautions." If the process of attention may be included under "cortical activities," the experimental control of this process would seem to be essential.[52]

In addition to inhibition, the knee-jerk has been used by recent investigators in testing for the effects of alcohol, fatigue and other modifying agents. The knee-jerk was found by Tuttle[53] to be depressed or absent during sleep. This confirms the results of Rosenbach, Bowditch and Warren, and Lombard in the previous century, and Lee and Kleitman[54] in 1923.

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[49] R. Dodge, Theories of inhibition, *Psychol. Rev.*, 33, 1926, 118 f.

[50] E. D. Wiersma, Die psychologische Auffassung einiger Reflexe, *Zsch. f. d. ges. Neur. u. Psychiat.*, 72, 1921, 254-266.

[51] F. Golla and J. Hetwer, The influence of various conditions on the time relations of tendon reflexes in the human subject, *Proc. Roy. Soc.*, 94B, 1922, 92.

[52] Golla and Hetwer in another paper (*Brain*, 47, 1924, 57-69) have presented the results of a study of the time relations involved in the coördination of muscles in voluntary and reflex responses. The Einthoven galvanometer was used. The following quotation from this paper is pertinent: "The time relations of the coordination of various muscles in certain voluntary movements is the same as when a similar movement is elicited as a spinal reflex response. Electromyograms of the patellar and the Achilles reflexes were obtained, recording the contraction of the antagonistic muscles concerned. It was found that in the patellar reflex the contraction of the flexor muscles of the knee follows that of the quadriceps extensor at an interval of 7 to 10 sigma. If a voluntary kick in imitation of a knee-jerk be made, it will be seen that the flexor contraction follows the extensor in 10 sigma, that is, in about the same time" (p. 66).

[53] W. W. Tuttle, The effect of sleep upon the patellar tendon reflex, *Amer. J. Physiol.*, 68, 1924, 345.

[54] M. A. M. Lee and N. Kleitman, Studies in the physiology of sleep, *Amer. J. Physiol.*, 67, 1923, 141-152.

Alcohol also acts as a depressant according to Dodge and Benedict,[55] Miles,[56] and Tuttle,[57] all of whom have studied the effects of the ingestion of alcohol on the knee-jerk. Tuttle found that alcohol may have either an excitatory or depressant effect; of the 12 cases studied, 9 showed augmentation and 2 depression of the knee-jerk, and one case was unaffected.

Although not concerned with the problems just referred to, the recent papers of Sherrington and Liddell[58] mark an important advance in knee-jerk research. These observers demonstrated that a gradual stretch applied to a muscle (especially the anti-gravity muscles) evokes a contraction. The knee-jerk and the patellar clonus are regarded as "fractional examples of the more comprehensive stretch reflex." In other words, the knee-jerk is produced by a very slight stretch of extremely brief duration. A stretch of less than 1/20 mm., lasting less than 1/20 sec., is sufficient to produce the jerk. The term 'myotatic' is applied to these reflexes which include postural, tendon and tonic reflexes. The work of Sherrington and Liddell bring under a single explanatory principle, phenomena which have appeared to be divergent hitherto.

CONCLUSIONS

What is the result of more than fifty years of experimental work on the knee-jerk? Only an attenuated sample of the huge literature on the subject of the patellar reflex has been presented here; one large section of that literature, which refers to the clinical aspects, has been neglected entirely. Because of the relative ease with which the knee-jerk may be controlled experimentally, it has perhaps received more laboratory attention than any other tendon-reflex. The researches typified by the work of Lombard, Bowditch and Warren, Dodge and others have attacked fundamental problems in the field of neuro-muscular action. The knee-jerk would seem to offer an excellent means of studying some of the 'urgent' problems of physiology and psychology to which Sherrington has referred.[59]

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[55] *Op. cit.*

[56] *Op. cit.*

[57] W. W. Tuttle, The effect of alcohol on the patellar tendon reflex, *J. Pharmacol. and Exper. Therapeut.*, 23, 1924, 163-172.

[58] C. S. Sherrington and E. G. T. Liddell, Reflexes in response to stretch (myotatic reflexes), *Proc. Roy. Soc.*, 96B, 1924, 212-242; Further observations on myotatic reflexes, *ibid.*, 97B, 1925, 267-283, 488-518.

[59] "In other words, the reactions of reflex-arcs are controllable by mechanisms to whose activity consciousness is adjunct. By these higher centres, this or that reflex can be checked, or released, or modified in its reaction with such variety and seeming independence of external stimuli that the existence of a spontaneous internal process expressed as 'will' is the naïve inference drawn. Its spring of action is not now our question; its seat in the nervous system seems to correspond with that of processes of perceptual level. It is urgently necessary for physiology to know *how* this control—volitional control—is *operative* upon reflexes, that is, how it intrudes and makes its influence felt upon the running of the reflex machinery. . . . No exposition of the integrative action of the nervous system is complete, even in outline, if this control is left without consideration." *Integrative Action of the Nervous System*, 1906.

It would seem that, among others, problems involving the relation of a concomitant mental process, or, stated in neural terms, the relation of the function of concomitant cortical systems to the knee-jerk in the intact human nervous system, might approach solution. On the other hand, there are serious technical difficulties, especially in connection with the standardization of the stimulus for the reflex, and the adequate registration of the response. Dodge said in 1910 that as heavy and clumsy a lever as the leg "weighing fifteen pounds or more" would be "laughed out of court in any other physiological experiment." Tuttle, however, has reported that when the excursion of the leg is recorded in knee-jerk experiments, the chief difficulty has been "in the sensitiveness of the method."

It has been pointed out that whatever may be one's definition of psychology, there can be no doubt that its province lies in part, at least, in the examination of the behavior of the animal with an intact nervous system, and that the psychologist must take the scientific responsibility for extending the results from the functionally simple physiological systems to the complex conditions of "human behavior, experience and personality."[60]

Provided the problems of experimental technique are solved, the knee-jerk would seem to one of the responses available for study in connection with the functional relationship between 'higher' and 'lower' physiological systems.

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[60] Dodge, *op. cit.*, 3 ff.

RECALL *VS.* REPETITION IN THE LEARNING OF ROTE AND MEANINGFUL MATERIAL

By WM. CLARK TROW, University of Michigan

This experiment, the object of which was to compare the value of recall and repetition in the learning of rote and meaningful material, was performed with the aid of 10 graduate students in educational psychology who acted as subjects. Three kinds of material were presented to the *S*s under uniform conditions: (1) series of digits; (2) lists of monosyllabic concrete and abstract nouns; and (3) a descriptive paragraph.

The digits were arranged in 19 series, 4 digits in the first series, 5 in the next, etc. They were presented orally by *E* at the rate of one per sec.; the *S*s recorded each series as soon as it was completely presented. The lists of nouns, taken from Whipple's Manual,[1] were given out similarly one list at a time, first 3 words, then 4, etc., up to 9, the last list being composed for the purposes of this experiment. The paragraph used was *The Old Homestead.*[2] This was read at a uniform rate clear through, after which the *S*s wrote down as much as they could remember.

Following the initial presentation just described, all *S*s had 7 practice periods of different sorts. The 9th period took place one day after the last practice, the 10th one week after, and the 11th 15 weeks after. Thus it was possible to discover the permanence value of the different methods used for the different materials.

After the initial presentation, which was the same for all, the practice period, held daily except Sunday for all but Group V, differed for each group of *S*s as follows:

Group I. Control Group. At each practice period, the *S*s wrote as much as they could remember without having the material presented to them again.

Group II. Recall Group. At each practice period, the *S*s first wrote as much as they could recall from the previous presentations, and then listened without writing while the materials were presented to other *S*s. The procedure here followed differs from that of Group I in that progressive improvement was possible since the materials were presented each day. But this presentation did not occur until *after* the *S*s had recalled as much as they could from the previous days. They were thus forced to employ the method of recall in learning.

Group III. Re-presentation Group. At each practice period the *S*s listened and then wrote as in the first presentation. Here, it is observed, there was no necessity for recall from the preceding presentations. Each set of digits, each list of words, etc., was written down at once as soon as it was given out.

Group IV. Recall–Re-presentation Group. A combination of the methods of II and III. At each practice period the *S*s wrote as much as they could recall from the previous presentations like Group II, then listened and wrote like Group III. These *S*s to aid their recall each time had the benefit not only of having *heard* the materials like Group II, but also of having gone through the exercise of writing them, like Group III.

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[1] G. M. Whipple, *Manual of Mental and Physical Tests*, 2, 1915, 164 f.

[2] *Op. cit.*, 210.

Group V. Massed Group. Like Group III, the *S*s listened and then wrote, but all the practice periods were run through at one sitting, each period following immediately upon the conclusion of the preceding. (Unfortunately it was not possible to get the figures for this group for the one-week period.)

The method of scoring was as follows: every digit counted 4 in its correct place in the series; if displaced one place, 3; two places, 2; three places, 1; more than three places, 0. The same method was employed for the words except that every word reproduced correctly however far out of position was given a score of at least one. The paragraph was scored for 'ideas' according to the key in Whipple's *Manual*, and for every noun reproduced scoring one, the context necessarily making the setting practically correct. The *S*s were not asked to underline the words they were certain of, as in Whipple's directions, because this procedure would furnish data irrelevant to the problem.

TABLE I

Digits

G	S	*Immediate*	*1-day*	*1-wk*	*15-wks*
I Control	Le	188	0	0	0
	Co	192	6	6	0
II Recall	Bl	155	27	18	0
	He	178	57	42	3
III Re-presentation	Sa	165	0	0	0
	Br	172	22	8	18
IV Recall–Re-pres.	Fe	158	18	9	0
	Ed	171	54	75	57
V Massed	Ha	157	47	—	0
	Be	168	81	—	6

Table I shows the score for digits remembered by every *S*. The first column of figures gives the total score on the initial presentation when the digits were written down immediately after each series was given out. The average of these scores is 170.4 and the average deviation is 9.8, showing how close together the abilities of the *S*s are in this respect. The other three columns represent the total number of digits remembered after the practice for a period of 1 day, 1 week, and 15 weeks. The plan of the other tables is similar to this, including the arrangement of groups in the order of excellence of retention after 15 weeks, though the zero recollection of one *S* in each group for this difficult rote material makes comparison in this case fruitless.

Since Groups II and IV had a quarter more time than the others (2 min. instead of 1.5), in order to make them comparable with the others their scores in the table are corrected, being in each case ¾ of the raw score.

The superiority of Group II (Recall) and Group IV (Recall–Re-presentation) for the 1-day and the 1-week period seems clear, as does the superiority of the massed method over all others for one day. The performance of the *S*s in Group III, who heard the digits repeated daily, was nearly as meagre as that of those in Group I who heard them but once.

Fig. 1 shows the average performance for each group for the trials given in Table I.

Table II shows the results in the same way for the word lists. The average score of the first trials is here 245.5, and the average deviation

TABLE II
Words

Group	*S*	*Immediate*	*1-day*	*1-wk*	*15-wks*
I Control	Le	232	2	2	1
	Co	296	0	0	0
II Recall	He	245	42	34	30
	Bl	256	150	162	70
III Re-presentation	Sa	246	49	58	20
	Br	262	67	24	7
IV Recall–Re-pres.	Fe	233	80	104	50
	Ed	225	64	72	44
V Massed	Ha	200	109	—	4
	Be	260	225	—	30

TABLE III
Percentage of Total Words Remembered Which Were Abstract

Group	*S*	*Immediate*	*1-day*	*1-wk*	*15-wks*
I Control	Le	49	50	50	0
	Co	47	—	—	—
II Recall	He	47	33	29	20
	Bl	47	29	35	32
III Re-presentation	Sa	45	28	38	15
	Br	49	21	50	71
IV Recall–Re-pres.	Fe	48	41	51	13
	Ed	51	38	39	30
V Massed	Ha	51	38	—	25
	Be	49	46	—	0
Total Score Ab. Wds.	All	1184	343	256	82
Total Score Words	All	2455	958	643	354
% Total	All	48	36	40	32

TABLE IV
Paragraph-Nouns

Group	*S*	*Immediate*	*1-day*	*1-wk*	*15-wks*
I Control	Le	19	21	22	22
	Co	20	15	18	18
II Recall	He	7	39	41	26
	Bl	11	44	46	40
III Re-presentation	Sa	23	43	45	32
	Br	14	43	36	33
IV Recall–Re-pres.	Fe	7	25 (41)	26 (43)	20 (33)
	Ed	13	25 (42)	28 (46)	23 (39)
V Massed	Be	16	43	—	24
	Ha	12	40	—	23

is again small, being 18.5. The superiority of Groups II and IV is shown even when the raw score is reduced ¼ to correct for the added time employed by these *S*s.

The striking advantage of the massed method is to be noted for the one-day period, as before, and its uselessness for the 15-week period. Group III (Re-presentation) again appears as inferior.

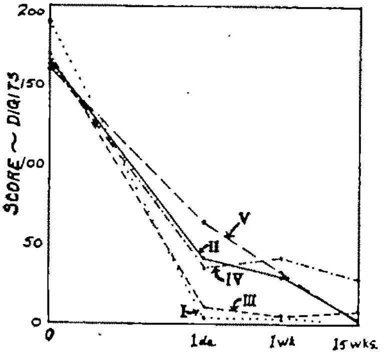

Fig. 1. Digits-Averaged

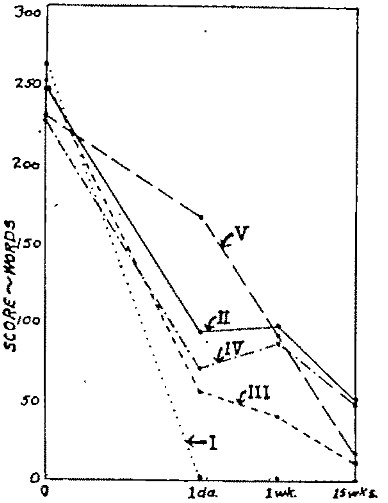

Fig. 2. Words-Averaged

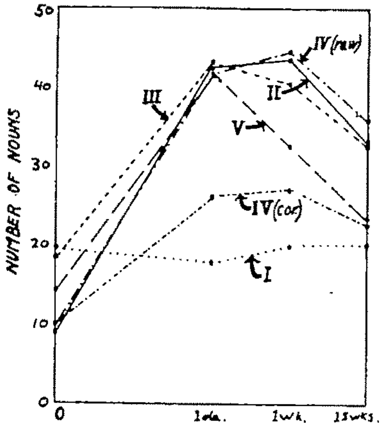

Fig. 3. Paragraph-Words

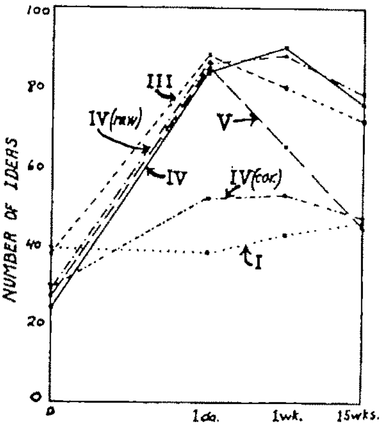

Fig. 4. Paragraph-Ideas

Fig. 2 shows the average performance for each group for the trials given in Table II.

Of the total number of words remembered by each *S*, Table III shows the percentage that were abstract. Fifty percent would mean that an *S* scored as high for abstract words as he did for concrete. There seems to be no marked relationship between the percentages and the methods of learning. It is not easy to see the cause for the increase in the percentage of abstract words remembered as compared with concrete over the 1-week interval. Relatively fewer abstract words are remembered after the 15-week interval.

Table IV shows the number of nouns remembered when they were presented in paragraph form as meaningful material rather than as lists. The average of the first trials is 14.2 and the average deviation is 4.2. The amazing thing here is the negligible superiority of all groups after 15 weeks to the *S*s in the Control Group.

The paragraph was so nearly learned that repetition at the end of the training period, as shown in the 1-day column, was practically perfect, the total number of nouns being 47. There is here little differentiation between groups.

Though Group IV had .4 more time than the others, the performance after the first three or four trials of the practice period was so nearly perfect that the presentation of .6 of the score as a correction in this case is clearly not just. The raw scores are therefore given in parentheses.

TABLE V
Paragraph Ideas

Group	*S*	*Immediate*	*1-day*	*1-wk*	*15-wks*
I Control	Le	40	36	44	48
	Co.	39	40	43	44
II Recall	He	16	77	87	66
	Bl	32	91	93	86
III Re-presentation	Sa	33	89	86	69
	Br	43	88	74	75
IV Recall–Re-pres.	Fe	19	50 (84)	50 (84)	44 (74)
	Ed	40	54 (90)	55 (92)	50 (84)
V Massed	Be	36	86	—	47
	Ha	19	83	—	43

Table V in similar fashion presents the number of 'ideas' remembered by the different *S*s over the different intervals. The average initial performance is 31.7, the average deviation 8.2. The highest possible score is 95. Here even more clearly than elsewhere is shown the comparative success of the massed method for recall one day later, and its utter uselessness fifteen weeks later. There is practically no difference in the results when the nouns are counted as in Table IV and when the 'ideas' are counted as in Table V.

The same fact can be read in Figs. 3 and 4. The groups retain practically the same relative position, too, that they do in the recall of the word lists, the most marked difference being, in the case of the lists, the superiority of the massed method after one day and the consistent inferiority of the re-presentation method.

CONCLUSIONS

(1) The Method of Recall (Groups II and IV) is superior to other methods.

(2) The Method of Re-presentation without Recall (Group III) is consistently ineffective.

(3) The Massed Method is as good as any for meaningful material and better than all other methods for rote material for one day's retention, but quite useless for retention over much longer periods.

(4) Abstract nouns are about equal to concrete in immediate recall, but after one day are forgotten to about the same extent that the concrete nouns are after fifteen weeks.

THE EFFECT OF PRACTICE ON DIFFERENT DEXTRALITY TYPES

By Hazel Bowman, University of Wyoming

The objects of the present experiment were (1) to investigate the effect of practice upon the records made by each hand in a pegging test by individuals of different handedness formulae; and (2) to compare the performances in order to determine whether there are significant differences which may be correlated with the differences in handedness.

Subjects. Five *S*s, all women, were used. They had been used by Downey in her study of dextrality types,[1] and had been classified by her according to the handedness and eyedness formulae as follows.[2]

S	Formula
R	RRR–Right-eyed
D	RRL_s–Left-eyed
C	RRL_1–Either-eyed
B	RLL–Either-eyed
M	LLR–Left-eyed

The formula RRR represents an individual who is right-handed in both unimanual and bimanual activities, writes right-handed, uses a bat right-handed, and puts the right hand down when using a broom.

An RRL individual writes and bats right-handed but puts the left hand down on the broom. There are two subforms of the RRL type; the RRL_1 individual reports that in using the broom the work is done primarily by the right hand; the RRL_s individual does the work primarily with the left hand.

Those who are of the formula RLL write with the right hand, bat left-handed, and put the left hand down on the broom.

A left-handed individual, LLR, uses the left hand in all unimanual activities, bats left-handed, and puts the right hand down on the broom. M has been forced by social pressure to use the right hand in writing; she writes rapidly and in excellent form.

The *S* was seated before a 'primary pegging-board,' 10 in. sq., containing 100 holes in 10 parallel rows and given a supply of wooden pegs. One hand was used to hold the pegs, the other to place them in the holes. When pegging with the right hand the *S* worked from left to right; when using the left, from right to left. The hands were used in the following order: R L L R R L. One minute was allowed for every trial. Every *S* was put through 35 sets of trials (a set being 3 trials with each hand in the above order), they thus had 105 trials with each hand). R and C were given three sets of trials a day on alternate days; a 5-min. rest being given between every set. B, D, and M were given one set a day on succeeding days. The average number of pegs put in by each hand, and the ratio between the two hands, R/L, was calculated for each set of trials.

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*Accepted for publication February 25, 1927.

[1] J. E. Downey, Types of dextrality and their implications, this Journal, 38, 1927, 317-367.

[2] Subject M was tested by Carol Rigby.

The results were summarized for two of the *S*s in Fig. 1, and for all the *S*s in Table I. The practice curves show a fairly steady increase in the number of pegs put in by each hand for all *S*s. The table shows that there was, however, a decrease in the ratio R/L for all right-handed *S*s, though this is partially due to an increase in the absolute values. The right hand, as one might anticipate, appears to reach its approximate physiological limit more quickly than the left. For the left-handed *S* the ratio increases, showing the same shift in the performance of the preferred and non-preferred hand.

TABLE I

Showing for every *S* the average performance with each hand and the ratio between them

S	Formula	First Set			All Trials		
		R	L	Ratio	R	L	Ratio
R	RRR–R E	53.33	32.66	1.632	76.41	58.50	1.319
D	RRL_3–L E	48.33	34.33	1.407	65.24	60.10	1.099
C	RRL_1–E E	37.33	33.33	1.120	60.48	55.17	1.102
B	RLL–E E	70.00	55.66	1.257	80.70	66.76	1.175
M	LLR–L E	38.66	49.33	.783	58.35	71.42	.814

A comparative study of the records is interesting. Table II gives the relative order of the *S*s in the performance of each hand before and after the practice series.

It will be seen from Tables I and II that, of the right-handed *S*s, B (formula RLL) made the fastest record with both hands. M, the left-handed *S* ((formula LLR), surpassed B after practice in the performance of the left hand. R (formula RRR) was consistently high in the performance with the right hand and low with the left. D and C (RRL_3 and RRL_1 respectively) did not have the speed of B with either hand, but they both surpassed R in the performance of the left hand before practice, and D even after practice. M, as was to be expected, was consistently high in the speed of the left hand and low in that of the right.

TABLE II

Showing the relative order of the *S*s in the performance with each hand before and after practice

Right Hand		Left Hand	
Before Practice	After Practice	Before Practice	After Practice
B	B	B	M
R	R	M	B
D	D	D	D
M	C	C	R
C	M	R	C

A study of the practice curves shows another interesting fact. The ratio of the right to the left hand is not the same for all the *S*s. B and R were consistently faster with the right hand, whereas D and C with the left hand equalled or surpassed the record with the right one or more times in the series. The practice curves of R and of D, shown in Fig. 1, are very unlike.

The records show that R (formula RRR) is more unilateral in her performance than are the RRL individuals. She never makes a ratio less than 1.205. The mean ratio is high, 1.319, and at no time is there any evidence of a left-handed advantage. D and C (the RRL *S*s), on the other hand, give a low mean ratio (1.099 and 1.102 respectively) and throughout the practice series the performance of the left hand follows closely that of the right—even surpassing the right hand record, as has been said, at one or more points. The introspections of these *Ss*, to be reported later, also give evidence of frequent left hand advantage.

The records of B are not easy to interpret. Perhaps the fact that the greatest speed is made by this *S* is significant. The pegging test is not 'strictly a unimanual performance and the skill of this subject in 'feeding the pegs by the hand not pegging undoubtedly operated to increase the number of pegs placed.

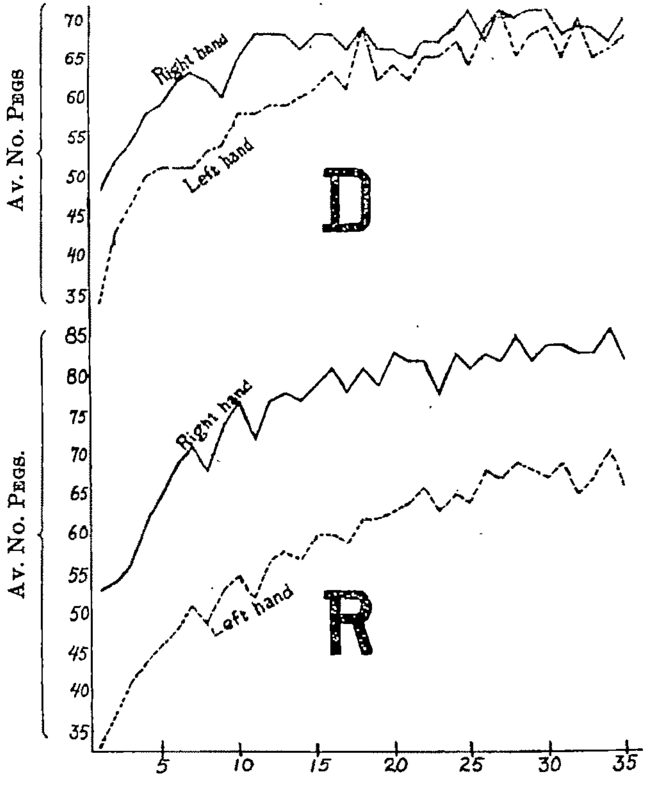

Fig. 1. Practice Curves in Pegging
D's formula is RRL$_{L}$-Left-eyed
R's formula is RRR-Right-eyed

Certain additional data were obtained from the *Ss* which may be significant. B is classified as either-eyed. Apparently in her case, eye-dominance fluctuated from day to day. The question arose as to whether dominance of right or left eye influenced the relative performance of the two hands. A report on eye-dominance was obtained from B and the performances for the right-eyed days and left-eyed days compared. The average performance during the right-eyed days for the right hand was

79.52, for the left hand, 67.22,—a ratio of 1.183. The average during the left-eyed days for the right hand was 79.89, for the left hand, 68.35—a ratio of 1.169. Though the ratio for the left-eyed days is smaller than for the right-eyed days, the difference is slight and the results are of doubtful value. Moreover, it is quite possible that eye-shifts occurred with the shifts of the pegging hand.

D and C showed many left-handed tendencies. They reported in this connection, definite but fluctuating 'feelings' as to handedness, depending upon a feeling of greater kinaesthetic readiness in one or the other of the two hands. They spoke of "right-handed days" and "left-handed days." D reported a difference on different days "in which hand goes out;" that is, which hand is used in the performance of tasks, such as opening a door, etc. To determine whether these 'feelings' represent a variation in the relative motor skill of the two hands, a report was obtained on this point from D. Out of the 35 days, 13 were reported as 'right-handed' days, 7 as 'left-handed' days. For 15 days there was no definite feeling. The average number pegged on the 'right-handed' days with the right hand was 69.92, with the left hand, 60.25—a ratio of 1.111. The average number pegged on the 'left-handed' days with the right hand was 65.04, with the left hand, 60.90—a ratio of 1.068. Here again the difference between the ratios is too small to be significant.

Before drawing any conclusions from the entire investigation it may be well to point out criticisms of the procedure. (1) The number of cases is so small that shifts in relative position mean little. (2) It appears from observation that the pegging test is not a unimanual operation. The skill of the hand not pegging operates to increase or decrease to a great extent the number of pegs put in. It is interesting to note that in most of the right-handed *S*s the left hand was much more skillful in 'feeding,' and often during the practice series it was evident that the great increase in the speed of the left hand actually represented an improved technique on the part of the right in furnishing the left hand with pegs. C was very inefficient in feeding pegs, an inefficiency which cut down her speed to a great extent. R and B were exceptionally skillful in this regard, though the left hand of R performed this operation much more easily than the right. (3) A comparative study of the *S*s is complicated to some extent by the difference in effort put forth. Although the *S*s were told to work as fast as possible it was evident that the instruction was followed more closely by some than by others, dependent upon the motivation. C cared not at all whether her record was fast or slow; B and D put forth the greatest possible effort. A desire to excel with one or the other hand also operated to influence the results.

CONCLUSIONS

A study of the records made in the pegging test by *S*s of different handedness formulae over a practice series of 35 sets of trials suggests the following conclusions:

(1) All the *S*s showed a practice effect—there is an increase in the number of pegs placed in the pegging-board in the 1-min. periods. There was a decrease in the R/L ratio for all right-handed *S*s, and an increase in the ratio for the left-handed *S*.

(2) R, whose handedness formula is RRR, appears to be more unilateral in performance than D and C, RRL *S*s.

(3) The record of B, whose handedness formula is RLL, is difficult to interpret. Her speed in pegging is the greatest of all our *S*s. Since pegging, as it was done by our *S*s, may be a bimanual performance, her speed may be significant of her handedness formula.

BIBLIOGRAPHY OF THE WRITINGS OF EDWARD BRADFORD TITCHENER: 1917–1927

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The following list brings up to date the bibliography of Edward Bradford Titchener which was begun by Foster.[1] For the sake of bibliographical consistency, I am following Foster's arrangement and continuing his numbering.

I. BOOKS

25. *A Primer of Psychology.* (Poczatki psychologji. Polish Translation by Cz. Znamierowski.) 3rd ed. Warsaw, M. Arct. 1921. Pp. 273.
26. *A Text-Book of Psychology.* (*Manuel de psychologie.* French Translation by H. Lesage.) Paris, Librairie Félix Alcan. 1922. Pp. xvi, 571.
27. *A Text-Book of Psychology.* (*Lehrbuch der Psychologie.* German Translation by O. Klemm. 2nd. ed.) Leipzig, Verlag von Johann Ambrosius Barth. 1926. Pp. xii, 470.

II. TRANSLATIONS (None)

III. ARTICLES, NOTES, DISCUSSIONS[2]

165. (With H. P. Weld.) Psychical Research, *New International Year Book* (for 1917), 1918, 560-561.
166. (With H. P. Weld.) Psychology, *New International Year Book* (for 1917), 1918, 561-565.
167. An Anomalous Case of Simple Reaction, *Amer. J. Psychol.*, 28, 1919, 62-65.
168. Applied Psychology, *Science*, 49, 1919, 169-170.
169. John Wallace Baird, *Science*, 49, 1919, 393-394.
170. John Wallace Baird, *Clark Univ. Lib. Publ.*, 6, 1919, 42-45.
171. (With H. P. Weld.) Psychical Research, *New International Year Book* (for 1918), 1919, 520-521.
172. (With H. P. Weld.) Psychology, *New International Year Book* (for 1918), 1919, 521-525.
173. Notes from the Psychological Laboratory of Cornell University, *Amer. J. Psychol.*, 31, 1920, 212-214.
174. Wilhelm Wundt, *Science*, 52, 1920, 500-502.
175. (With E. G. Boring.) Sir Thomas Wrightson's Theory of Hearing, *Amer. J. Psychol.*, 31, 1920, 101-113.
176. (With H. P. Weld.) Psychical Research, *New International Year Book* for 1919), 1920, 545-546.
177. (With H. P. Weld.) Psychology, *New International Year Book* (for 1919), 1920, 546-550.
178. Note on The American Journal of Psychology, *Amer. J. Psychol.*, 32, 1921, 3-4.
179. Brentano and Wundt: Empirical and Experimental Psychology, *Amer. J. Psychol.*, 32, 1921, 108-120.

---

[1] W. S. Foster, A bibliography of the published writings of Edward Bradford Titchener, *Studies in Psychology, Titchener Commemorative Volume*, 1917, 323-337.

[2] This list does not include the book-reviews.

180. Systematic Psychology, *Amer. J. Psychol.*, 32, 1921, 155-156.
181. Wilhelm Wundt, *Amer. J. Psychol.*, 32, 1921, 161-178.
182. The Road to En-Dor, *Amer. J. Psychol.*, 32, 1921, 301.
183. Functional Psychology and the Psychology of Act: I, *Amer. J. Psychol.*, 32, 1921, 519-542.
184. On the Plan of Wundt's *Physiologische Psychologie*, *Amer. J. Psychol.*, 32, 1921, 596-597.
185. Experimental Psychology in Italy, *Amer. J. Psychol.*, 32, 1921, 597-598.
186. The Psychophysiology of the Condemned, *Amer. J. Psychol.*, 32, 1921, 598-599.
187. George Trumbull Ladd, *Amer. J. Psychol.*, 32, 1921, 600-601.
188. (With L. B. Hoisington.) Psychical Research, *New International Year Book* (for 1920), 1921, 559-560.
189. (With L. B. Hoisington.) Psychology, *New International Year Book* (for 1920), 1921, 560-564.
190. Functional Psychology and the Psychology of Act: II, *Amer. J. Psychol.*, 33, 1922, 43-83.
191. *Festschrift* for Carl Stumpf, *Amer. J. Psychol.*, 33, 1922, 157.
192. Mach's "Lectures on Psychophysics," *Amer. J. Psychol.*, 33, 1922, 213-222.
193. The Max Klinger Bust of Wundt, *Amer. J. Psychol.*, 33, 1922, 304.
194. A Note on Wundt's Doctrine of Creative Synthesis, *Amer. J. Psychol.*, 33, 1922, 351-360.
195. Psychical Research, *New International Year Book* (for 1921), 1922, 592.
196. Psychology, *New International Year Book* (for 1921), 1922, 592-601.
197. An Emendation, *Amer. J. Psychol.*, 33, 1922, 599-600.
198. The Term 'Practice,' *Psychol. Rev.*, 29, 1922, 328.
199. (With S. Feldman.) A Bibliography of the Scientific Writings of Wilhelm Wundt, 7th Suppl. List, *Amer. J. Psychol.*, 33, 1922, 260-262.
200. The Expression of Simple Feeling, *Amer. J. Psychol.*, 34, 1923, 149, 616-617.
201. Visual Intensity, *Amer. J. Psychol.*, 34, 1923, 310-311.
202. Wundt's Address at Speyer, 1861, *Amer. J. Psychol.*, 34, 1923, 311.
203. Relearning after Forty Six Years, *Amer. J. Psychol.*, 34, 1923, 468-469.
204. Goerenz' *Vestigia*, *Amer. J. Psychol.*, 34, 1923, 468-469.
205. Psychical Research, *New International Year Book* (for 1922), 1923, 593-595.
206. Psychology, *New International Year Book* (for 1922), 1923, 595-604.
207. (With E. G. Boring.) A Model for the Demonstration of Facial Expression, *Amer. J. Psychol.*, 34, 1923, 471-485.
208. The Term 'Attensity,' *Amer. J. Psychol.*, 35, 1924, 156.
209. The Overlooking of Familiar Objects, *Amer. J. Psychol.*, 35, 1924, 304-305.
210. The New Princeton Laboratory, *Amer. J. Psychol.*, 35, 1924, 465.
211. The Moscow Institute, *Amer. J. Psychol.*, 36, 1925, 156.
212. Edmund Clark Sanford, *Amer. J. Psychol.*, 36, 1925, 157-170. Reprinted in the *Clark Univ. Lib. Publ.*, 8, 1925, no. 1, 22-30.
213. Demonstrations of Affective Method, *Amer. J. Psychol.*, 36, 1925, 157-170.
214. Granville Stanley Hall, *Clark Univ. Lib. Publ.*, 7, 1925, no. 6, 91-92.
215. Experimental Psychology: A Retrospect, *Amer. J. Psychol.*, 36, 1925, 313-323.
216. The Reception of the "Origin of Species," *Science*, 64, 1926, 556.

IV. EDITORIAL

4. Editor. *Studies from the Department of Psychology of Cornell University.* (See Nos. 110-176 below.)
5. Editor. *The American Journal of Psychology.* From January 1921 (vol. 32) to October 1925 (vol. 36). (Associate Editor from 1895 to 1920.)

STUDIES PUBLISHED BY STUDENTS FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY DURING TITCHENER'S DIRECTORSHIP

110. On the Psychological Response to Unknown Proper Names. By E. M. Alspach (with H. P. Weld). *Amer. J. Psychol.*, 28, 1917, 436-443.
111. The Psychological Basis of Appetite. By E. G. Boring and A. Luce. *Amer. J. Psychol.*, 28, 1917, 443-453.
112. The Weber-Fechner Law and Sanford's Weight Experiment. By M. Cowdrick (with E. G. Boring). *Amer. J. Psychol.*, 28, 1917, 585-588.
113. An Example of the Fractionation of Data from the Method of Constant Stimuli for the Two-Point Limen. By L. B. Hoisington (with E. G. Boring). *Amer. J. Psychol.*, 28, 1917, 588-596.
114. The Effect of Absolute Brightness upon Color Contrast. By R. L. Crane (with E. G. Boring). *Amer. J. Psychol.*, 28, 1917, 597-607.
115. A Qualitative and Quantitative Study of Weber's Illusion. By M. E. Goudge (with H. P. Weld). *Amer. J. Psychol.*, 29, 1918, 81-119.
116. A Checking Table for the Method of Constant Stimuli. By G. J. Rich (with E. G. Boring). *Amer. J. Psychol.*, 29, 1918, 120-121.
117. On the Calculation of an Associative Limen. By H. D. Williams (with E. G. Boring). *Amer. J. Psychol.*, 29, 1918, 219-226.
118. An Analysis of the Psychometric Function for the Two-Point Limen with Respect to the Paradoxical Error. By M. Kincaid (with E. G. Boring). *Amer. J. Psychol.*, 29, 1918, 227-232.
119. An Experimental Study of Mixed Feelings. By P. T. Young. *Amer. J. Psychol.*, 29, 1918, 237-271.
120. The Psychological Attitude of Charles Dickens toward Surnames. By E. DeLaski (with H. P. Weld). *Amer. J. Psychol.*, 29, 1918, 337-346.
121. The Discrimination of Cutaneous Patterns below the Two-Point Limen. By C. L. Friedline. *Amer. J. Psychol.*, 29, 1918, 400-419.
122. The Localization of Feeling. By P. T. Young. *Amer. J. Psychol.*, 29, 1918, 420-430.
123. A Preliminary Study of the Psychology of Heat. By F. Cutolo (with E. G. Boring and H. P. Weld). *Amer. J. Psychol.*, 29, 1918, 442-448.
124. An Experimental Study of Feelings of Relation. By J. M. Gleason. *Amer. J. Psychol.*, 30, 1919, 1-26.
125. A Study of Tonal Attributes. By G. J. Rich. *Amer. J. Psychol.*, 30, 1919, 121-164.
126. Tunable Bars, and some Demonstrations with a Simple Bar and a Stethoscope. By P. T. Young. *Psychol. Bull.*, 15, 1918, 293-300.
127. The Lapse of Verbal Meaning with Repetition. By M. F. Bassett and C. J. Warne (with H. P. Weld). *Amer. J. Psychol.*, 30, 1919, 415-418.
128. The Adjustment of the Hering Color-Blindness Apparatus. By M. Cowdrick and M. Winfield (with H. P. Weld). *Amer. J. Psychol.*, 30, 1919, 418-422.
129. Adaptation of Superficial Pain. By H. H. Straus and R. F. Uhlman (with H. P. Weld). *Amer. J. Psychol.*, 30, 1919, 422-424.

130. Foveal Adaptation to Color. By H. Sheppard. *Amer. J. Psychol.*, 31, 1920, 34-58.
131. Size *vs.* Intensity as a Determinant of Attention. By A. M. Bowman (with K. M. Dallenbach). *Amer. J. Psychol.*, 31, 1920, 87-90.
132. On the Non-Visual Perception of the Length of Lifted Rods. By L. B. Hoisington. *Amer. J. Psychol.*, 31, 1920, 114-146.
133. An Experimental Study of Visual Form. By M. J. Zigler. *Amer. J. Psychol.*, 31, 1920, 273-300.
134. A Note on the Series of Black, Greys and Whites. By F. L. Dimmick. *Amer. J. Psychol.*, 31, 1920, 301-302.
135. An Experimental Study of Visual Movement and the Phi Phenomenon. By F. L. Dimmick. *Amer. J. Psychol.*, 31, 1920, 317-332.
136. An Experimental Study of Kinaesthetic Imagery. By A. H. Sullivan. *Amer. J. Psychol.*, 32, 1921, 54-80.
137. On the Relevancy of Imagery to the Process of Thought. By C. Comstock. *Amer. J. Psychol.*, 32, 1921, 196-230
138. On the 'After-Sensation' of Pressure. By R. T. Holland. *J. Exper. Psychol.*, 3, 1920, 302-318.
139. An Experimental Investigation of the Positive After-Image in Audition. By H. G. Bishop. *Amer. J. Psychol.*, 32, 1921, 305-325.
140. An Experimental Study of Cutaneous Imagery. By C. [C.] Braddock. *Amer. J. Psychol.*, 32, 1921, 415-420.
141. The Integration of Punctiform Cold and Pressure. By S. Tung (with H. P. Weld). *Amer. J. Psychol.*, 32, 1921, 421-424.
142. The Hering Color-Blindness Apparatus and the Normal Rayleigh Equation. By M. Winfield and C. Strong (with H. P. Weld). *Amer. J. Psychol.*, 32, 1921, 425-428.
143. The After-Effect of Seen Movement when the Whole Visual Field is Filled by a Moving Stimulus. By W. A. Thalman (with K. M. Dallenbach). *Amer. J. Psychol.*, 32, 1921, 429-441.
144. The Illusory Perception of Movement on the Skin. By A. K. Whitchurch (with K. M. Dallenbach). *Amer. J. Psychol.*, 32, 1921, 472-489.
145. The Involuntary Response to Pleasantness. By C. H. Corwin (with H. P. Weld). *Amer. J. Psychol.*, 32, 1921, 563-570.
146. The Integration of Punctiform Warmth and Pressure. By R. S. Malmud (with H. P. Weld). *Amer. J. Psychol.*, 32, 1921, 571-574.
147. Cutaneous Localization and the "Attribute of Order." By H. M. Lufkin (with K. M. Dallenbach). *Amer. J. Psychol.*, 33, 1922, 128-134.
148. On the Non-Visual Perception of the Length of Vertically Whipped Rods. By E. Shults (with L. B. Hoisington). *Amer. J. Psychol.*, 33, 1922, 135-139.
149. On the Non-Visual Perception of the Length of Horizontally Whipped Rods. By A. S. Baker (with L. B. Hoisington). *Amer. J. Psychol.*, 33, 1922, 139-144.
150. An Experimental Investigation of the Experience which Accompanies the Sudden Cessation of an Auditory Stimulus. By E. W. Amen (with H. G. Bishop). *Amer. J. Psychol.*, 33, 1922, 263-267.
151. The After-Effect of Movement in the Sense of Touch. By W. A. Thalman (with K. M. Dallenbach). *Amer. J. Psychol.*, 33, 1922, 268-276.
152. Optical Illusions of Movement. By W. A. Andrews (with K. M. Dallenbach). *Amer. J. Psychol.*, 33, 1922, 277-284.
153. Film, Surface, and Bulky Colors and their Intermediates. By Mabel F. Martin. *Amer. J. Psychol.*, 33, 1922, 451-480.
154. The Areal and Punctiform Integration of Warmth and Pressure. By I. Bershansky (with H. P. Weld). *Amer. J. Psychol.*, 33, 1922, 584-587.

155. The Integration of Warmth and Pain. By L. Knight (with H. P. Weld). *Amer. J. Psychol.*, 33, 1922, 587-590.
156. The Phenomenological Description of Musical Intervals. By E. M. Edmonds and M. E. Smith (with H. P. Weld). *Amer. J. Psychol.*, 34, 1923, 287-291.
157. Thunberg's Illusion. By I. Bershansky (with H. P. Weld). *Amer. J. Psychol.*, 34, 1923, 291-295.
158. An Experimental Study of Memory Color and Related Phenomena. By G. K. Adams. *Amer. J. Psychol.*, 34, 1923, 359-407.
159. The Effect of Varied Instruction on the Perception of Lifted Weights. By A. C. Reid. *Amer. J. Psychol.*, 35, 1924, 53-74.
160. Size *vs.* Intensity as a Determinant of Attention. By D. Dewey and K. M. Dallenbach. *Amer. J. Psychol.*, 35, 1924, 121-125.
161. The Spatial Limen for the Four Principal Film Colors. By M. Elliott, J. West, and L. B. Hoisington. *Amer. J. Psychol.*, 35, 1924, 125-131.
162. An Experimental Study of the Visual Negative After-Image. By C. C. Braddock. *Amer. J. Psychol.*, 35, 1924, 157-166.
163. Position *vs.* Intensity as a Determinant of Attention of Left-Handed Observers. By R. S. Burke and K. M. Dallenbach. *Amer. J. Psychol.*, 35, 1924, 267-269.
164. The RL of Increased Chroma with Film Colors. By D. Ginsberg and L. B. Hoisington. *Amer. J. Psychol.*, 35, 1924, 269-272.
165. A Preliminary Study of the Bourdon Illusion. By B. R. Rubin and H. P. Weld. *Amer. J. Psychol.*, 35, 1924, 272-279.
166. The Effect of Varied Instructions on the Perception of Distances in Terms of Arm-Movement. By W. A. Garrison. *Amer. J. Psychol.*, 35, 1924, 420-435.
167. Lapse of Meaning with Visual Fixation. By V. J. Don and H. P. Weld. *Amer. J. Psychol.*, 35, 1924, 446-450.
168. Delayed Meaning. By M. V. Wilson and H. P. Weld. *Amer. J. Psychol.*, 35, 1924, 450-453.
169. An Experimental Study of the Affective Qualities. By J. P. Nafe. *Amer. J. Psychol.*, 35, 1924, 507-544.
170. Obliviscence during Sleep and Waking. By J. G. Jenkins and K. M. Dallenbach, *Amer. J. Psychol.*, 35, 1924, 605-612.
171. The "Glassy Sensation." By E. F. Möller. *Amer. J. Psychol.*, 36, 1925, 249-285.
172. The Determination of Memory Span by the Method of Constant Stimuli. By J. P. Guilford and K. M. Dallenbach. *Amer. J. Psychol.*, 36, 1925, 621-628.
173. A Preliminary Study of the Range of attention. By N. F. Gill and K. M. Dallenbach. *Amer. J. Psychol.*, 37, 1926, 247-256.
174. The Effect of Primacy upon Recall. By J. G. Jenkins and K. M. Dallenbach. *Amer. J. Psychol.*, 38, 1927, 285-291.
175. The Experience of Heat. By N. C. Burnett and K. M. Dallenbach. *Amer. J. Psychol.*, 38, 1927, 418-431.
176. 'Fluctuations of Attention' with Weak Visual Stimuli. By J. P. Guilford (with K. M. Dallenbach). *Amer. J. Psychol.*, 38, 1927, 534-583.

APPARATUS

THE YALE PSYCHO-CLINIC PHOTOGRAPHIC OBSERVATORY

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Dr. Gesell has for some time recognized the value of the camera as a tool for the scientific study of children, and to this end has had in mind the utilization of a mechanical device[1] which permits photographing subjects from the most advantageous angle.[2] The method of advancing upon an *S* and photographing from the conventional camera level does not always yield an optimal return, when one considers that one desires a truly representative record for further intensive study. Ideally, one should be in position to photograph instantly from any angle the *S*'s behavior. The two simultaneous photographs of the *S*, shown in Fig. 1, illustrate the point in question. *B* shows the subject from the eye level and *A* shows the overhead view. Our experience with infants is that overhead views are likely to prove more valuable than are horizontal views. An attempt to meet the requirements above mentioned led to the construction of the apparatus herein described.[3]

The photographic observatory is essentially an hemispherical iron-ribbed structure, inside diameter 12 ft. 6 in., mounted so that it rotates on a substantial cylindrical iron base, 32 in. high, inside diameter 12 ft. For brevity we shall refer to this entire structure as the dome. Sixteen T-iron ribs of the superstructure, with the stem of the T exterior, radiate from the top, meridian-like, 20° apart, and are bolted at the other end to a heavy horizontal 2-in. angle-iron, which forms the circumferential support of this upper portion. An iron band, 1 in. wide, encircles the structure midway of the top and base and acts as a brace for the T-ribs. The cylindrical base upon which the superstructure rests consists of a heavy 2-in. angle iron bent in the form of a circle, which sets on the floor, upon which at 9-in. intervals are erected vertical ½-in. square iron posts which support the heavy U-iron of the same form and diameter as the floor iron. The entire piece is riveted firmly together into a substantial base.

The superstructure overlaps the cylindrical base exteriorly by 2 in., and rests upon 7 strong 2-in. fibre pulleys which are bolted to the base near its top at equally spaced distances. The center of the dome is therefore exactly 30 in. from the floor, giving the entire structure a total height of 9 ft. 6 in. Seven other pulleys, fastened horizontally to the base above the first pulleys, protrude ⅜ in. beyond the outside of the base and serve as guides to prevent the upper portion of the dome, which can be rotated through an arc of 360°, from scraping against the base. The dome, except for the camera tracks and the windows which contain the lamps (described below), is covered by a 16-mesh iron screen, wired securely to the iron frame-work.

Two of the T-ribs of the dome, 90° apart, give place to camera tracks, the left[4] one of which extends to the top of the dome through an arc of 90°, the other not quite to the top. Each track consists of two U-irons bent to fit the form of the dome with the hollows of the U facing each other the length of the track. The camera carriage, an oak board 6 x 10 in. mounted on six wheels, four on the bottom and two on the top on springs, travels within this grooved track. The top wheels press elastically against the rear flanges of the track and thus automatically force the bottom wheels against the front flanges so as to hold the carriage at right angles to the center of the dome. The camera is screwed firmly to an oak shelf 7 x 9 in., hinged exteriorly to the upper end of the carriage, and is adjustable to any vertical angle with respect to the inside of the dome by means of a slot-and-screw device. The carriage is operated by a cord and pulley, while a special spring-brake locks it firmly to the track at any point.

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[1] Arnold Gesell, *Mental Growth of the Pre-School Child*, 1926, 160.

[2] A complete description of the procedure and the apparatus used in the clinical examination will be found in Gesell's forthcoming book.

[3] The writer acknowledges with thanks the helpful suggestions by Professor Raymond Dodge.

[4] All directions are exterior.

A battery of 8 Cooper-Hewitt A. C. lamps, supplemented by twenty-four 50-watt incandescent lamps, mounted in three asbestos-lined boxes which fit the curvature of the dome and rest on the T-ribs, supplies the light. Two of the boxes, each containing three Cooper-Hewitt and nine incandescent lamps, lie adjacent midway of the two camera tracks at an angle of 40° with the horizontal. The third box containing the remaining lamps lies 25° to the left of the left camera track at an angle of 50° with the horizontal. Photographically, the two large boxes serve as the light source, while the smaller box is the reflecting screen. Special flexible wire cables, supplying the energy for the lights, pass through a ring hanging from a ball-and-socket joint in the ceiling immediately above the dome and thence down to the boxes containing the lamps. This arrangement permits the conductors to turn about freely as the dome rotates.

White tissue-paper screens, following the form of the dome, shield the *S* from the direct glare of the lights and remove distraction from this source. The incandescent lights, although they have little actinic value, serve to add the red rays absent in the Cooper-Hewitt lights and thus restore to objects their normal appearance. Two light grey window-shades on rollers which are suspended in the dome, one at right angles to each camera, serve as background screens.

The two still cameras are operated electrically by a six-volt storage battery which energizes an electro-magnet on the camera shelf above which projects an iron extension of the camera trigger which acts as an armature. The photographer may operate one, or the other, or both cameras simultaneously, regardless of their positions, by a simple manipulation of the control switches and contact key. The error in simultaneity of action of the cameras, as measured by a 100 d.v. electric fork, is less than 0.01 sec.

The entrance, 6 ft. 9 in. high and of the width of two segments of the dome, is opposite the right camera track. To enter one pulls outward two swinging doors and the hinged angle iron support of the superstructure and a gate of the cylindrical base.

The nature of the dome permits standardization and experimental control to a considerable extent. The fine mesh screen covering the dome, with its coat of flat white paint, serves a three-fold purpose in that it provides excellent ventilation, salvages most of the light, and makes possible observation from the outside without detection by the *S* under examination. The camera tracks and the base on which the superstructure rotates are marked off in degrees so that one may quickly place the camera at any desired angle. The right angular distance between the two cameras checks the third dimension. A cinema camera mounted on a special carriage which may be fitted to either track at any point or projected through the door or apertures in the wire screen gives a continuous record of the *S*'s behavior. The arrangement of the still and motion cameras permits an elastic program in photography. The use of artificial lamps assures dependable light for photographing at high speed. The constant relation between the light source and cameras yields comparable photographic records. The practically noiseless operation of the lamps and dome is an asset to any form of experimentation.

The dome has been in operation since March, 1927, and its usefulness amply demonstrated. To date our products include many hundreds of still photographs and several thousand feet of motion film of the 16 mm. variety. Besides the relatively large number of children whom we photograph for the purpose of securing norms of behavior, we make special intensive studies of a few *S*s, each of which is examined at intervals of four weeks. A single examination yields from 30 to 60 still photographs and from 100 to 200 feet of motion film.

A DEVICE FOR THE DEMONSTRATION OF CERTAIN ILLUSIONS OF MOTION

By Leonard Carmichael, Brown University, and Harold Schlosberg, Princeton University

The device here described is of value in the demonstration and in the analytic study of one type of visual illusion. The present apparatus was developed independently by the authors at Princeton University, but we later discovered it to be somewhat similar to one described by Bowditch and Hall in 1880.[1] Our device, however, seems to present certain advantages over the previous apparatus.

The present machine makes it possible to present under controlled and varied conditions, all of those visual illusions that depend upon what has been termed by Thompson, from its similarity to the movement used in cleaning a dish, a *rinsing* motion.[2] A rinsing movement may be described as a movement in which the stimulus-object is revolved in a circle, at a given distance from the center, but maintains a fixed orientation so that it does not rotate about an axis within itself.

Among the factors that it is possible to vary when the device here described is used, beside the actual form and color of the stimulus patterns, are: (*1*) the speed of the rinsing movement; (*2*) the extent of the excursion, *i.e.* the distance of the circle inscribed by the center of the stimulus design from the true center of rotation; and (*3*) the distance of the *O* from the object.

Essentially, the device consists of two sprockets of an equal number of teeth, coupled by means of an endless chain, so that when one sprocket is driven, the other also turns at an equal rate of speed. Thus all corresponding radii of the two sprockets always remain parallel and when a connecting yoke or strip is attached by a pivot to a pair of these corresponding radii, at points equidistant from their respective centers, this yoke will be moved with a circular motion, but not rotated; *i.e.* the motion imparted to the yoke will be the *rinsing* motion previously described.

In the following description the numbers in parentheses following the names of the parts refer to the indicated points in the accompanying cut. The sprockets (1) and chain (2) are placed behind the upright support (3). The yoke (4) is attached to arms (5) mounted on the same shafts (6) as the sprockets, but on the opposite side of the upright support (3). A board (7), made of soft wood, is mounted on the yoke, and to this base the stimulus patterns (8) may be attached by means of thumb tacks.

The dimensions of the parts are as follows. The sprockets (1) are 11 cm. in diameter and they have 36 teeth, and they are mounted with set screws upon a steel shaft 2 cm. in diameter and 7 cm. long. Each shaft passes through a brass bearing (9) mounted in the upright support (3). The

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[1] H. P. Bowditch and G. S. Hall, Optical illusions of motion, *J. Physiol.*, 3, 1880-1882, 297-307.

[2] S. P. Thompson, Optical illusions of motion, *Brain*, 3, 1880, 292.

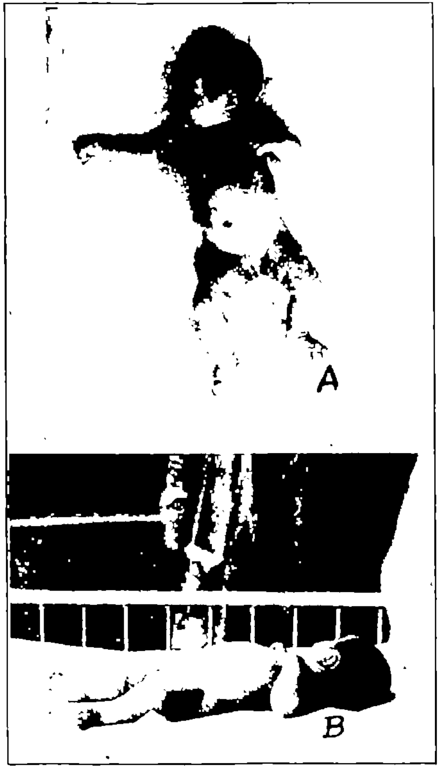
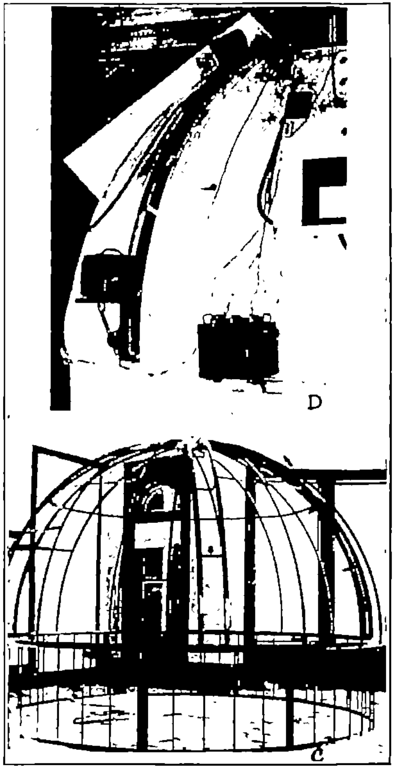
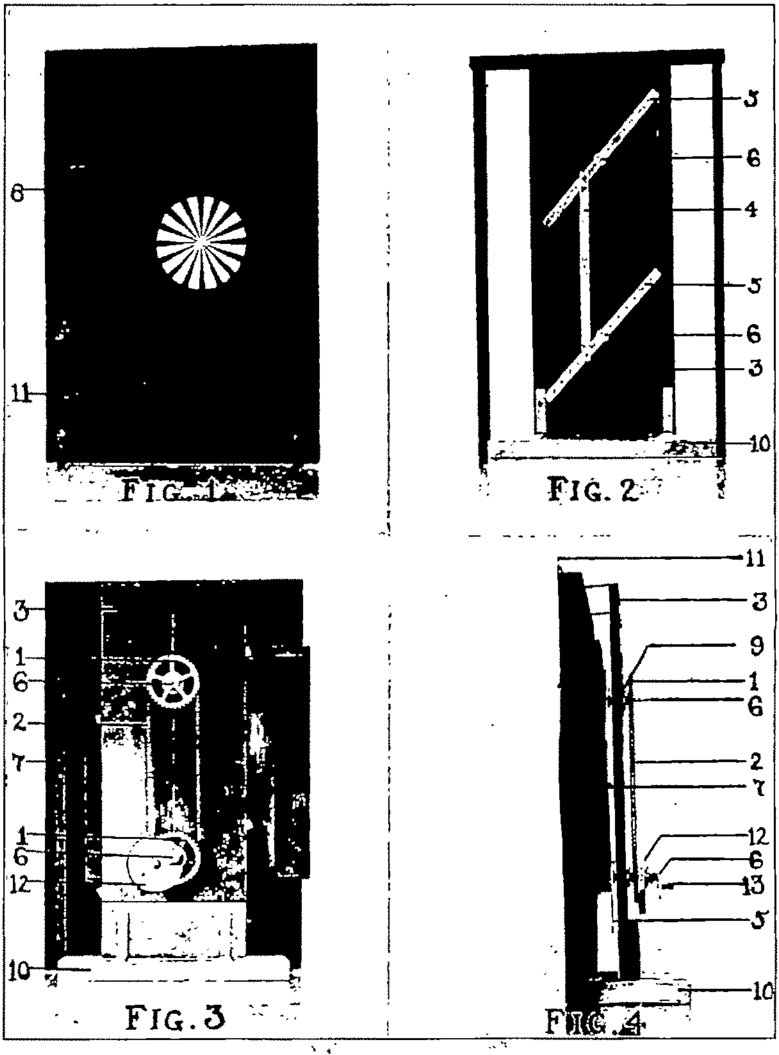


FIG. 1. YALE PSYCHO-CLINIC PHOTOGRAPHIC OBSERVATORY

A and *B* are, respectively, overhead and horizontal simultaneous photographs of an *S.* *C* shows the iron framework of the dome; and *D* is a view of the completed dome showing the left camera track; the still camera mounted on its carriage; the motion camera above on the same track; two boxes containing the lamps; the storage battery; and some of the electrical connections





FOUR VIEWS OF A DEVICE FOR THE DEMONSTRATION OF CERTAIN ILLUSIONS OF MOTION

FIG. 1. Front view, as seen by the class or observer.

FIG. 2. Front view, with the shield and the board with mounted stimulus pattern removed.

FIG. 3. Rear view.

FIG. 4. Side view.

bearings are both in the mid-line of the upright support, and the center of the lower one is 20 cm. from the baseboard (10) and that of the upper is 35 cm. above the lower.

The arms (5) are 34 cm. long and are made of strip brass, 2 cm. wide by 3 mm. thick. They are fastened to the shafts at their middles. Holes are drilled every 2 cm. and tapped. The yoke (4) is made of the same strip brass, and is 39 cm. long. Holes are drilled 2 cm. from each end, and the yoke may be attached to any pair of corresponding holes in the arms.

The upright support (3) is 75 cm. high, 30 cm. wide, and 2 cm. thick. It is mounted on a baseboard (10), 46 by 25 by 3 cm. in size. Angle irons are used to make this support more firm. The board (7), on which the stimulus pattern (8) is fastened, is mounted on the yoke (4). The board itself is 45 cm. square. It is covered with a piece of dull black cardboard of the same size. To cover the moving parts of the device a screen of black cardboard (11) is mounted 5 cm. in front of this board. The screen is 75 cm. high and 50 cm. wide. The aperture in the center of this frame-like screen is 35 cm. square. The complete device as seen by the class or by the *O* is seen in Fig. 1.

To counterbalance the eccentrically moving parts, a lead counterweight (12) is mounted on the lower shaft. If this counterweight is properly adjusted, and if the moving parts are fitted carefully, the apparatus will operate smoothly and quietly. No moving parts are exposed to the observer, except the stimulus field.

The device may be operated by hand by means of a crank (13) or by a small electric motor and reducing gears. A range of 50-400 R.P.M. will be great enough for the demonstration of most of the illusions requiring this type of motion.

No wholly satisfactory analysis has been presented of the various factors important in the production of the illusions dependent upon this rinsing motion. The early theories proposed to account for these phenomena have been well summarized by Bowditch and Hall.[3] Sanford has also considered these illusions and has reprinted certain of the more striking stimulus patterns.[4] We have begun a more complete analysis of the phenomena included in this group of illusions, in which the influence of various colors upon the stimulus patterns will be particularly studied. The device seems to be as satisfactory for this analytic work as it does for the lecture-table demonstration, for which it was primarily developed.

TIMING THE FILMO AUTOMATIC CAMERA

By H. M. Halverson, Yale University

The Filmo camera operates by means of a spring motor which, when released, synchronizes the rotation of the shutter with the movement of the film through the film channel. The camera is standardized to run at two speeds, 16 exposures (normal speed) and 8 exposures per sec., by the adjustment of a large slotted screw which controls the motor governor. One should expect to obtain a speed of 12 exposures by adjusting the screw to a point intermediate to those at which these two speeds are obtained.

We have carefully marked this intermediate point, and timed our camera for all three speeds. For each timing the motor was fully wound and permitted to run until it came to a stop. A stock camera procured from a local dealer was timed in like manner.

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[3] *Op. cit.*

[4] E. C. Sanford, *Course in Experimental Psychology*, 1898, 313-318.

The charted results indicate that the cameras perform at a speed approximating that for which they are designed. The decrease in the speed of the motor, if it is permitted to run no longer than 60 to 70 sec., is slow and constant. To secure a speed of 12 exposures the control screw should be set at a point slightly nearer the 8 than the 16 point on the speed dial. The two Filmo motors did not perform exactly alike for any given speed adjustment.

Speed Indicator at	Laboratory Camera					Stock Camera				
	Ave Speed	m.v.	Initial Speed	Final Speed	Duration of Run	Ave Speed	m.v.	Initial Speed	Final Speed	Duration of Run
16	15.2	0.4	16.3	14.3	69 sec.	15.7	0.4	16.6	14.7	70 sec.
8	8.7	0.5	9.7	7.7	120 sec.	6.4	1.2	8.0	3.4	167 sec.
	8.9	0.4	9.7	8.0	100 sec.	7.4	0.4	8.0	6.4	100 sec.
12	13.5	0.6	14.7	12.5	77 sec.					

A NOTE ON APPARATUS USED FOR SECURING INSPIRATION-EXPIRATION RATIOS

By Carney Landis, Wesleyan University

Several years ago Nixon described a simple device for recording inspiration-expiration time.[1] As part of an experiment concerning the detection of deception on the basis of the inspiration-expiration ratio I had the device described by Nixon constructed by an expert mechanician. When I endeavored to use it, I found it to be unsatisfactory and ill-adapted for exact experimental work. Slight changes in the position of the subject's body, in his rate or amplitude of respiration were sufficient to cause it to cease functioning. Even under the best of conditions the electrical make and break tongue could not be made to function at anywhere near a constant point in the respiratory wave.

In previous experiments I have made use of the apparatus described by Burtt.[2] This apparatus when properly adjusted followed accurately the respiratory wave, showing a clean make and break at the points of inspiration and expiration, but in practice I found it difficult to keep Burtt's apparatus in proper adjustment. When the adjustment was faulty, however, the fact was clearly indicated by the record—a virtue that Nixon's device does not possess.

In view of these facts it would seem wisest, if one desires the accuracy necessary to make diagnostic use of the inspiration-expiration ratios for the detection of deception, to record respiration directly by means of a pneumograph connected to a recording tambour.

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[1] H. K. Nixon, A simple device for recording inspiration-expiration time, *J. Exper. Psychol.*, 6, 1923, 383-386.

[2] H. E. Burtt, A pneumograph for inspiration-expiration ratios, *Psychol. Bull.*, 15, 1918 325-328.

BOOK REVIEWS

Aphasia and Kindred Disorders of Speech. By HENRY HEAD. Cambridge, The University Press; New York, The Macmillan Co., 1926. Vol. I, pp. xvi, 549; Vol. II, pp. xxxiii, 430.

In the history of every science, changing fundamental concepts or philosophical points of view dominate. These concepts affect the development of the science, they retard as well as help. They stimulate studies towards one end, they stifle others which do not conform to the general pattern. But, as development in a science takes place, the current conception becomes like the tomb of the Arab, covered with contributions from the devout, so that the original grave form disappears.

Although aphasia as a scientifically studied field is relatively new there has been time for the development of a number of points of view. These have been partly offshoots of conceptions of cerebral-mental relations, and partly description of theoretical clinical entities. For one who wants to know something also of these varied interpretations, the early chapters of Head's work are indispensable even though they do not pretend to completeness. In this part Head first deals with the early period during the renaissance of medicine when the idea of vital spirits was dominant. That period he relates to the fixed localization doctrine of Gall and his immediate successors. In the succeeding sixty years the accumulation of data culminated in the pronouncement of Broca, which was criticized by Hughlings Jackson, but without general approval. Subsequent "diagram makers," such as Bastian, Kussmaul, Wernicke, and their followers, introduced the concepts of possible clinical relations, which then dominated. Later work, described by Head as "chaos," included the strictly anatomical localizations of Henschen, the opposing views of Von Monakow as illustrated in his hypothesis of diaschesis, the apraxia studies of Liepmann, the language-analysis school, the concept of intellectual disturbance as illustrated in the work of Marie, and the contributions of Pick. Marie, "the iconoclast," stands out almost as prominently as Hughlings Jackson as a prophet or apostle of a new generation. It is to the iconoclast school that Head belongs, but perhaps more to that represented by the English neurologist than that by the Salpetrière professor.

It is with Head's work and views that we are now chiefly concerned. Much of the earlier discussion of aphasia was based on the study of elderly patients, who because of arteriosclerotic changes had brain destructions due to cerebral hemorrhages (apoplexies). In addition to the impossibility of fitting into current schemes the facts noticed in patients of this age, Head's special interest and point of view developed because of his work with more youthful soldiers whose brains had been injured in war accidents. Instead of adhering to the historical methods of examination Head devised and used methods whereby the ability to 'think' could be tested, sometimes without the intervention of obvious verbalization.

The results of the work indicated the necessity of looking at the disturbances in the field of aphasias from a new point of view, which may briefly be described as that of symbolic thinking. This concept of aphasias bears a relation to the views of Hughlings Jackson on imperception, those of Liepmann on apraxia, those of Marie on intellectual defect, and to those of Pick. Head points out (Vol. I, p. 83), what the reviewer has also previously emphasized, that the fluctuations in speech functions are important phenomena, and that the aphasic is not static. One of the cases cited (No. 20) in which recovery was relatively rapid after the removal of a tumor pressing upon the brain substance, "yielded invaluable records illustrating the relation to one another of the various forms of verbal formulation and expression and of the order in which they were restored." In a number of places Head also indicates the importance of the *Gestalt*. He points out (I, p. 420) that anxiety and worry affect the speech function, and that reactions "vary, not only with the mode of giving the command, but with the situation at the moment" (I, p. 121). "The older views . . . . have given place to the idea of a single unitary reaction in response to multiple factors" (I, p. 133). In other places behavior psychology is prominent, when for example, he says that it is "impossible to find any single term to include all these disorders of behaviour, which extend on the one hand for mechanical aptitudes to exercises in formal logic on the other" (I, p. 217), and which characterize the aphasic states. Speaking, reading and writing are the normal, and "aphasia, amnesia, alexia, agraphia, agnosia, and other terms of a like nature . . . . are convenient shorthand descriptions of varieties of . . . . abnormal behaviour" (I, p. 300). "The various disorders of speech . . . . manifest the ways in which the organism masters a situation . . . . with a defective mechanism" (I, p. 301).

Speech disturbances, grouped under the name aphasia, are, however, described under the formulation "disorders of symbolic thinking and expression." It should be understood, however, that (I, p. 399) "symbolic formulation and expression is not more than an empirical term." Language "consists of a vast number of ways in which an individual meets his imperious desire for expression" (I, p. 301). Even in those aphasic states known as 'jargon' the patient tries to express himself, but we fail to catch his meaning because we cannot follow his associative processes, principally on account of the verbalizations he uses. Frequently his rapid replies are correct, but there are difficulties which are evidenced only by a retardation in responses (I, p. 149). The reaction of the aphasic is frequently only an easy way out of a recognized difficulty. So-called images, especially the visual, may not be disturbed, and memory and intelligence may not be affected (p. 418). The patient who is restricted in his speech expression is thrown upon himself, because he no longer meets the social demands of a dinner party, the newspaper, and the like. A patient may know "what he wants to say, but he cannot discover suitable terms in which to express his thought to himself or to others" (I, p. 402). "Speech is a mental process which we may or may not exteriorize; an aphasic is not only deprived of power to speak aloud, but he has also lost the capacity to formulate his thoughts to himself in certain symbolic terms" (I, p. 400).

The clinical varieties of aphasia which Head believes can be distinguished are (1) verbal, (2) syntactical, (3) nominal, and (4) semantic defects.

The verbal defects are those in which the patient may be limited to a 'yes' or 'no' response, or in which the articulation is imperfect or halting. Comprehension may be slightly lowered, but frequently becomes restored, although silent reading may be difficult because the patient is "compelled to look back at the beginning of a long sentence in order to obtain its full meaning" (I, p. 413). His comprehension in other directions may be good, although he may give an incorrect verbal response. For example, in scoring at card games, he may say the wrong number, but his general behavior is consistent with an understanding of his correct score. The main defect is that of "verbal structure and words as integral parts of a phrase" (I, p. 413).

Jargon is more characteristic of the syntactical defects. Words may be incorrectly enunciated, but there is also a lack of grammatical coherence. Word meanings may be apprehended and reacted to in ways other than verbalization. The aphasic of this type appears to take in the meanings of his silent readings. "The patient has plenty of words, but their production is ataxic, and they are strung together without the usual connecting links. This leads to jargon and renders difficult even internal formulation of words and their meanings" (I, p. 414).

The nominal defects are primarily the inability to use words and to understand their values. The patient may, nevertheless, be able to give many words relating to the matter he wants to describe. Because of his inability to formulate in characteristic words the things he wants to describe or picture, he may appear to be stupid. He may try to draw the plan of a room, but may leave out essential elements, but put in minor details, and in doing this he may exhibit important parts of the ground plan in elevation. There appears to be a confusion of ideas.

The semantic defects are "characterized by want of recognition of the ultimate significance and intention of wards and phrases, apart from their direct meaning" (I, p. 416). Writing tends to be confused, and drawing is poor. "These patients . . . . fail to recognize landmarks," and, therefore, cannot be trusted to go about alone. "The tendency to confusion and want of comprehension . . . . leads these patients to seek solitude."

Regarding the anatomical correlates of the aphasias, Head concludes (I, p. 441) that "the more definitely the injury destroys the lower portion of the pre- and post-central convolutions and the parts which lie beneath them, the more likely are the defects of speech to assume a verbal form. A lesion in the neighborhood of the upper convolutions of the temporal lobe tends to produce 'syntactical' disorders. Destruction round about the region of the supra-marginal gyrus causes defects in the use of language which I have called 'semantic;' whilst a lesion situated somewhat more posteriorly seems to disturb the power to discover and to understand names or other 'nominal expressions.' "

These statements of neurological localizations must, however, be understood in the light of the author's general view of cerebral functions. He accepts the view of deviation of Sherrington, and he quotes with approbation the reviewer's concept of the cerebral cortex as a labile organ. He considers it a fallacy to conclude that the motor cortex is "the seat of strictly limited 'centers' of preordained function," and he believes that the destruction of a 'center' produces only a transitory loss of function, which may be followed by recovery. "The deeper and wider . . . . the injury to the cortex . . . . the graver and more permanent is likely to be the loss of function; but we must never forget that it has disturbed a highly organized act and has not removed a strictly anatomical 'center' " (I, p. 436). "A complete act of speech comes to be a wide-spread response of the organism to each fresh situation . . . . Since the cortex is a more flexible organ with less rigid and preordained reactions, the disturbance of function due to injury of the surface is not so severe and permanent and is less easily determined . . . . All attempts shortly to differentiate a cortical from a sub-cortical aphasia are bound to fail" (I, p. 441). There are "no 'centers' for speaking, reading, writing or other forms of behaviour comprised in the normal use of language" (I, p. 478), but there are areas concerned with symbolic formulation and expression in a single hemisphere. Injury to different parts of the brain may produce somewhat different speech diturbances. On the other hand, we must understand that (I, p. 499) "no function is 'localized' strictly in any part of the cortex and no form of activity, somatic or psychical, is built up into a mosaic of elementary processes which become evident when it is disturbed by a lesion of the brain."

No brief summary such as this can give an adequate view of the many important observations and of the acute analyses of the speech disorders contained in the two volumes. Head has introduced into the aphasia problem a new point of view, and his methods of examination have brought to light conditions which would not have been suspected through the use of the older examination methods. More important than the new methods, and more important than the new classification is the turning away from the static picture of the aphasias and the insistence on a dynamic concept. Speech, whether normal or abnormal, is always dynamic. It is always a changing form of behavior, an adaptation to the ever-changing present situation. Moreover, none of the aphasias is self-contained. The verbal may give place to a syntactical or to a semantic type, and there may be in one patient evidence of two or more forms. Although Head does not insist as much as the reviewer has done upon the importance of the phenomena in reëducation, this has obviously appealed to him. It is, therefore, the whole picture—the defect and the recovery from the defect—which must be looked at if one is to have an adequate view of the aphasias. It is the redevelopment of the speech function, or the reëducation of the aphasic, which the reviewer believes will give more adequately facts respecting the relationships of speech defects and the relations of the brain to the speech function. As of special value to the teacher in relation to student instruction we may accept the general formulation that the precentral cortex is the controlling mechanism for voluntary movements. But that formulation can not explain the phenomena of recovery of voluntary control after destruction of the so-called motor cortex, nor the other facts of disturbances in voluntary movements after destruction of other brain parts. The control of movement is a diffuse neurological control. It depends upon the combined actions of a number of brain parts. So also the speech functions depends upon the combined actions of many cerebral (cortical and sub-cortical) elements.

The conditions upon which the speech functions depend are relatively unknown. Why an aphasic may be able to sing, not only tunes but also accompanying words, but be apparently unable to use the same words in conversation or at command, is a scientific mystery. Why he may be able to describe a "key" as "an object which goes into a lock," but be unable to give the name "lock" is another scientific mystery. Why he may be able to give the name "lock" on one day every time the object or the word may be shown to him, but on the succeeding day be unable to give the word at any time is a third mystery. Other similar mysteries could be cited.

If one has a faith, or an intense belief, they may not be considered to be mysteries. If, however, one accepts and defends a general hypothesis, it frequently happens that the acceptance leads to more and more tenuous guesses. The fabric changes from opacity through translucency, to a transparency, in which latter state fact or science disappears. There is a tendency to look through the opacity and 'see' all things revealed. The translucent shows only what is expected or suggested. It is, however, not difficult to get the initiated into the frame of mind that he will "hallucinate" with the transparent. In science we try to get the transparent state, but without the dominating faith.

Head has taken the aphasias out of the opaque screened darkness and has brought them into a position in which we can see them, if we will, in better outline even though it may be vaguely. He has brought them from the limbo of faith, creed, or scientific belief, into the light of experimental investigation. There remains for the future the tedious and the protracted task of lighting them still more.

The reviewer's studies of aphasia do not make him see Head's work and conclusions through blue or through rose-colored glasses. He thinks a more general formulation of the aphasias may be made, but he also sees that Head has made a most notable contribution, as notable as, and perhaps more notable than, his previous studies on the sensations of the internal organs, the sensory functions of the peripheral nerves, the spinal cord, the thalamus, and the cerebral cortex.

The inclusion of cases 23 (II, pp. 348 ff.) and 25 (II, pp. 379 ff.) is unfortunate. These cases are those of so-called congenital alexia and agraphia, or congenital word-blindness. They are disorders of the acquisition of speech, and not disorders of acquired speech. Persons showing these disturbances are cases who do not acquire the abilities to read and to write mainly through vision, as most children and adults do, but by kinesthesis. They have been intensively studied by G. M. Fernald who with special procedures has been able to get these patients to acquire the abilities they could not acquire in the more usual manner. They do not acquire some speech symbols in the ordinary fashion, and because of special social (mainly school) conventions they fail to make certain adjustments. That they are not incapable of making the adjustments is evidenced by the fact that when they are handled in a suitable manner they learn to read and to write readily and sometimes with greater facility than the normal.

The reviewer would be willing to make a generalization broader than that of Head. Now that Head has shown the inadequacy of the "diagram makers" and of other schematists, it would be well to get away from all classification for a time. From the point of view of both neurology and psychology it would be well to settle upon a more general formula, such as "difficulty or defect in the formation of associations," as the basic phenomenon in aphasia. If this fundamental formulation were adopted it would stimulate to many constructive studies of the diversities and the degrees of difficulties. A more specialized formulation is a stimulus to either blind acceptance and support, or to unreasoned opposition. And this has been the history of the aphasia problem.

Notwithstanding these criticisms, the contributions of Head are the most valuable which have appeared in several decades. If they are carefully studied they are bound to rid both neurologists and psychologists of traditional explanations and of superstitious beliefs. It is because of this that the work is truly monumental, partly self-erected, and largely erected for others who travel the aphasia road who may see its device, "Excelsior."

University of California, Los Angeles SHEPHERD IVORY FRANZ

The Trend of History: Origins of 20th Century Problems. By WILLIAM K. WALLACE, New York, The Macmillan Co., 1922. Pp. 372.

The author sets out on a relatively new type of interpretation in regard to history. His key is furnished in the preface where he says, "history is the book of life of mankind." He points out that it ought to be more than a mere record of events and should inculcate the public spirit of the times that brought about gradual changes in the different forms of government. He expresses as the purpose of the volume, "to trace through the tangled maze we call the course of events the logical antecedents and coefficients of, . . . the process of social life."

The volume has considerable interest for the social psychologist since at every epoch the author emphasizes the state of mind which brought about transformations in the underlying theory of the State. For instance, very early in the exposition we find a religious influence contrasted with the political consciousness: "In contrast with the religious instinct common to mankind, we find among civilized people what we may term political consciousness" (p. 3). Later on, he shows that in what he calls the politico-theistic concept of the State the Church was distinct from the State in that "secular laws were not binding on the clergy." In this form of government there was a close analogy between God as a supreme ruler of mankind and the nobility who ruled the state. An inseparable connection appeared between the characteristics which were ascribed to God and those attributed to the nobility.

This was superseded by the politico-juridic concept which claimed that the personality of the individual should be projected into the State and the action of the State was therefore to be made amenable to the will of the individual. When this idea occurred to the social mind of those governed, the notion was inculcated that the State must be moral. This lead to a constitutional form of government in which the rights of the individual were codified, and later developed into the representative type of government when the concept of humanity was instilled into the public mind.

Still later there occurred a rise in economic interests together with a development of nationalism with more emphasis on the welfare of the middle classes. About the middle of the last century, too, communism showed its power in the State. All these influences indicated the tremendous problem that the pursuit of wealth was raising in human affairs. At the same time scientific investigations gave men greater confidence in their own accomplishments. Curiously enough, this was coupled with the consciousness that man was left "isolated in an evergrowing, ever-changing world" (216). All of this came to a head since 1870 in terms of a politico-economic concept of the State. The thesis here was that the "State exists merely as a means for promoting the welfare of the individual" (223). Gradually, through the rise and fall of imperialism and the gradual increase in power of the proletariat, new concepts began to show their influence, including what the author calls "the substitution of quantitative for qualitative standards" (p. 258). This factor is traced through the remaining chapters in international politics as well as in the development of culture throughout the population of a Super-State.

Thus far the impression may have been gained that the book is full of theoretical discussions, but it remains to be indicated that at every turn the author brings to bear his scholarly knowledge of historical and philosophical tendencies. Historical events together with dates dot the pages of the work wherever the author wishes to substantiate the principles which he has laid down. The volume ends with what appears to the reviewer not altogether an optimistic turn, in that the State now emerges as Power (346). We wish that the writer with so much historical background could have functioned, also, as a prophet and could have told us something of the possibilities for the future. He leaves us buried in thought as he closes his volume with the statement "the State had become the shell, it was no longer the kernal of social life."

State University of Iowa CHRISTIAN A. RUCKMICK

Psychology and History. By HARRY ELMER BARNES. New York Century Co., 1925. Pp. 195.

The present volume is a reprint as a separate work of the chapter on "Some Phases of the Significance of Psychology in the Equipment of the Historian" in the author's *New History and the Social Studies*. It was deemed advisable to make this particular portion of the larger work more readily accessible to psychologists and psychiatrists who would have a special interest in the subject and who would want to know of the efforts which have been made by historians and others to apply psychological methods and data to the interpretation of the history of man and human culture.

The book presents an excellent summary of writers on psychology whose views have been of significance to the student of social science—such writers as James, Hall, Baldwin, Thorndike, McDougall, Trotter, Watson, Freud, Cattell, Bagehot, Le Bon, Tarde, Wundt, Boas, Lamprecht, and Robinson. The psychologist, however, will find the summary of little value to him except in so far as it offers suggestions for points of departure into fields with which he is relatively unfamiliar. The review, of necessity, is too brief and selective to give more than a vague apperceptive background for psychology in relation to social science. The psychological points of view which Professor Barnes selects for treatment are too near the frontier to be accepted or rejected upon cursory examination of them in epitomized form. Few, if any, of them have as yet become shaken down into the generally accepted body of psychological doctrine. They are, in other words, hypotheses, and, unfortunately, hypotheses which have for the most part only dialectic rather than factual support. To seek for the springs of human action and the motivation of conduct means that one must enter a domain which is marked by a plethora of speculation and argument and a paucity of observational data. But it is just this problem of motivation which the psychologists are asked to solve by those who are interested in the social sciences or in practical affairs. The students of intellectual history, judging from Professor Barnes' book, are no exception to this rule. To them the book ought to be an invaluable guide to modern trends of psychology in the field of motivation. The author has brought together in concise and clear form a vast array of material. One notes only occasional lacunae: the work of men like Rignano, studies in animal behavior, and some evaluation of the question of hedonism would seem to be pertinent to the general purpose of the book.

The psychologist will be more interested to see in what way the historian tries to make use of certain portions of psychology. A section on "The Psychological Interpretation of History" treats of this problem. Following the lead taken by such men as Lamprecht and J. H. Robinson the present author makes it clear that the study of intellectual history needs some sort of *dynamic* psychology as a guilding principle in its interpretation of cultural movements of the past. The author does not state exactly what he means by dynamic psychology—any more, for that matter, than do the psychologists themselves who use the term. It would seem that a dynamic psychology is a sort of *faute de mieux* either for complete analysis or for an analysis in which the parts separated out descriptively fail to present an adequate picture of some driving, purposeful aspect which seems to attach to the whole. Be that as it may, dynamic psychology in the present context seems to imply the Freudian type of psychology principally. By an adaptation of the Freudian analytic procedures to leading personalities and to institutional and cultural history a wealth of new material will be presented to the historian for study and examination.

The reviewer comes away from the book with certain doubts and misgivings. In the first place the question arises as to the wisdom on the part of historians of taking over for their own purposes a methodological tool which is admittedly imperfect and rough in the restricted tasks for which it was chiefly designed. Motivation, instinct, drive, unconscious, purpose: these are labels which one would have difficulty in sticking on the right object. They are hardly concepts which have yet been clearly and univocally enough worked out to warrant the assumption that psychology has hit upon a marvellous methodological 'open-sesame' even within its own field of research. There are passages in which Professor Barnes appears aware of this difficulty (*e.g.* p. 151 ff.), but on the whole his thesis is an enthusiastic and ardent plea to his colleagues to revolutionize the study of history by the application of dynamic psychology. It is another case of a premature application of a 'new' psychology in the hope that 'scientific' miracles may be worked in social studies and the world of affairs. This premature technological bent is further apparent in the author's general attitude towards science. For Professor Barnes science seems to be a sort of magic tool by which man is to pattern the world after heart's desire, rather than a refinement of observational and experimental technique by which he may hope to discover uniform relations which exist between things *just as they are*. Passages such as the following are numerous: "Without bravely facing these realities which demonstrate the 'sickness of the modern acquistive society,' however, we cannot hope to improve our social order. . . . Until something like the same degree of objectivity can be produced in the social sciences that exist today in natural science, we are likely to continue with our common-sense and rule-of-thumb methods in economics, politics, and modes of social conduct, a procedure which is becoming yearly more dangerous and menacing with the increasing complexity of modern life. If this is an impossible aspiration, then the 'jig is up' with the human race. . . ."

Finally, it strikes the reviewer that the author has at times tried to force into one universe of discourse items and data which are not intimately related. Let us assume for the moment that dynamic psychology offers the historian a means of analysing the motives and characters of leaders of history. Does it follow that, because one knows the motives and subtle circumstances and influences which led some political leader to adopt or work out a certain type of social philosophy, one is thereby in a better position to understand the philosophy itself or its relation to subsequent events? It has been said that the only sure way to understand philosophy is to discover the factors which led a given thinker to cherish certain views and scorn others; that the only way to understand Schopenhauer's philosophy, *e.g.* is to know the conditions in Schopenhauer's life which made a certain *Weltanschauung* pleasing and acceptable to him. But is not Schopenhauer's philosophy and its relation to previous and subsequent systems a subject-matter to be analysed in its own right without relation to the personal motivations of the philosopher himself? Does it aid one in 'interpreting' intellectual history to know, as the author states, that Augustine's theology was a psychic compensation for a wild youth and varied and extensive sexual experiences, or that Alexander Hamilton's love for authority centralized in the government was due to the absence of parental control and authority in his early home life?

One is dealing here with two fields of inquiry which are not very closely related. No one will deny that there is a connection between them, but to follow this connection through its ramifications would lead one into a bewildering regress of causes which would throw very little light, even if they could be accurately determined, on questions of personal biography or matters of history proper. It would seem particularly advisable in the social sciences to mark off the various fields of investigation fairly sharply in order to bring restricted sets of phenomena under the application of suitable methods. History has its own method; biography has had another method, and is perhaps the sort of subject which could profit considerably by the use of recent methods to study the motivation of behavior. But it does not necessarily follow that, because a certain individual has played an important part in some phase of the world's history, one will have a larger knowledge of that particular portion of history by studying the physiological and psychological traits of the individual. For purposes of historical study the ideas and actions of an individual form one set of phenomena to be examined in a restricted context. The same phenomena in another context constitute the starting point for psychological and physiological investigations. The relation of the motivation of an intellectual process within a given individual's psychophysiological constitution to the subsequent historical fate of that same process when presented to the world in systematic form is one which is more likely to be 'interpreted' than observed.

Harvard University C. C. PRATT

Tierpsychologie vom Standpunkte des Biologen. By FRIEDRICH HEMPELMANN. Leipzig, Akademische Verlagsgesellschaft, 1926. Pp. viii, 676.

This work by the professor of zoölogy and comparative anatomy at Leipzig is divided into a *spezieller Teil* of 454 pages, an *allgemeiner Teil* of 182 pages, and an extensive bibliography of 26 pages of small type. The first division treats the behavior of the various phyla with the following allotment of pages: Protozoa, 74 pages; Porifera, 2; Coelenterata, 29; Echinodermata, 23; Vermes, 42; Mollusca, 23; Arthropoda, 120, including 64 on Insecta; Vertebrata, 119. The second division has chapters on Special Capacities (vision, orientation and homing, time perception, counting-ability, childhood, play), Form a Stimulus (reaction to form-stimuli, perception or relation and *Strukturfunktion*, the *Gestalt*-principle, ideas), Feeling Life of Animals (feelings and emotions, speech, ethical and aesthetic feelings), Analogies to Special Psychic States in Humans (sleep, dreams, hypnosis, mental diseases), The Brain as Organ of the Psyche, The Psyche, and History and Methods of Animal Psychology.

The author's purpose is to furnish a comprehensive summary of the established content of animal psychology, avoiding so far as possible too great emphasis either on physiological processes "or on the ethology or ecology of animals." More specifically, the plan of the work seems to be to give full accounts of the behavior-repertories of the various phyla in the first part, and to supplement these accounts with a few chapters of general import in the second part. The book is therefore composed primarily of facts, most of which were experimentally obtained, rather than of theories; and no attempt is made to discuss the anatomy and physiology of receptors and nervous systems except in a few places where the treatment is concise, general, and yet valuable.

The standpoint, indicated in the title, is that of the biologist rather than that of the psychologist; for this reason psychologists will fail to find any particular emphasis laid on a number of problems which have occupied them, especially in America, in recent years. For example, scant attention is paid to maturation *vs.* learning in 'instinct' (no mention is made of Breed and Shepard's work on chicks, or of Yerkes and Bloomfield's on kittens), to the delayed reaction (there is no mention of Hunter's work), or to the order of elimination of errors in maze learning, etc. There is no account of the recent discussions as to the rôle of insight in animal learning, such as would be presented if Koffka's criticisms of Thorndike were summarized; the author goes no further than to give Köhler's statement, with rather extensive quotation, that the learning of apes is 'insightful.' The more purely biological and older problem of the tropism is, however, adequately treated; various criticisms of the Loeb doctrine by Von Buddenbrock and others are quoted.

The account of Protozoa is quite full and includes the theories of amoeboid movement and a discussion of the artificial amoebae of Büschli and others.

The section on insects consists of individual rather than social psychology; only three pages are devoted to social insects. This omission is not to be deplored, as the field is well covered by such books as Wheeler's. The main topics treated are: the rôle of the senses in insect behavior, reflexes, and instincts. The author insists throughout that insect behavior is not wholly rigid but is modifiable, and he cites experimental proofs. He follows Ziegler in his definition of instinct.

The section on mammals is subdivided into two parts on the various groups, which begin with marsupials and end with apes. This section is somewhat marred by the uneven quality of the material presented; for example, after Pavlov's work is described, the "famous poodle of Lubbock (1889)" is mentioned, and there is a good deal of material, especially on the horse, which is non-experimental. The positive results of Kalischer on *absolutes Gehör* in dogs are given on page 423, while Johnson's negative results in the same experiment are not mentioned until page 432, and the relation of the two experiments is not pointed out.

In the *allgemeiner Teil*, the section on Special Capacities is comprehensive and valuable. In subsequent sections we find a statement of the *Gestalt*-principle, but no criticisms of it are quoted. Under "ethics" and "aesthetics" are discussed sex life, treatment of other animals and of young, "marriage," dances, etc.; under "mental diseases," peculiarities of behavior caused by lesions. The Brain as Organ of the Psyche (including discussion of spinal and decerebrate animals) is too short to be of great value, and the same may be said of the History, which is a mere sketch of five pages. The author follows Ziegler in his history and appears to think that the latter has furnished such an adequate one that further efforts at this time are not called for. The Psyche is a chapter summarizing opinions on problems such as the criteria of consciousness, the necessity of postulating an entelechy, and the question whether mind appears suddenly in the evolutionary series or is to be assigned to all living organisms.

The author has succeeded remarkably well in his task of assembling an enormous mass of data from many scattered sources. In a work of this magnitude some omissions and neglects are inevitable, and in the present case some of these faults detract from the value of the book. There is, for example, no mention of Lashley's extensive work on cortical function. That this omission is not due to an intended restriction of subject-matter is shown by citation of work on the very same problem as Lashley's on page 405 (Jellinek and Koppanyi's work on rats, 1923). Watson's *Behavior* is cited neither in the bibliography nor elsewhere, but Watson's monographs are adequately treated in the text. Conradi's work on the social influence in the song of birds is not mentioned, although several pages are devoted to song. Some American studies have not been utilized in discussion of form as stimulus (Lashley, Bingham, Casteel).

The book is beautiful in binding, paper, and typography. There are 134 illustrations and a plate of eight chimpanzee expressions. The only serious typographical error is the changing of "Yoakum" into "Choakum." An amusing error occurs when the author speaks of the experiments on Paramecium made by "den beiden Damen Day und Bentley."

Harvard University FRANK A. PATTIE

The Measurement of Intelligence. By E. L. THORNDIKE, E. O. BREGMAN, M. V. COBB, ELLA WOODYARD and the Staff of the Division of Psychology of the Institute of Educational Research of Teachers College, Columbia University. New York, Teachers College, Columbia University, 1926. Pp. xxvi, 616.

This book represents the results of three years of research by Professor Thorndike and his associates at Teachers College on the nature, meaning, and measurement of intelligence. Much of interest and of value to psychologists will be found in it on all three of these problems; but without doubt the most important contribution of the authors lies in the field of measurement, and more specifically in the construction and validation of a scale for measuring a given "segment" of "intellect," known briefly as *CAVD*.

Intellect *CAVD* is strictly defined by an inventory of tasks involving the completion of sentences (*C*), arithmetic problems (*A*), vocabulary tests (*V*), and directions (*D*), the last named often requiring comprehension of sentences and paragraphs. Forty composite tasks—10 each of *C*, *A*, *V*, and *D*—are placed at levels of difficulty ranging progressively from A to Q; or, differently expressed, from a composite (A) with which an adult of approximately 3 years mental age can succeed (get 50% correct) to one (Q) with which only about one-fifth of college graduates succeed. Each of these difficulty levels has a numerical value which represents its distance from an approximately absolute zero of intellect. This zero point was located by utilizing a consensus of judgments, with respect to certain tasks, made by a group of psychologists who were specialists in animal and child psychology. Every judge ranked 56 tasks in an order of merit in accordance with the degree of intellect (in his opinion) required to perform it. From the tasks thus chosen to be of minimal intellectual difficulty, that is to say, at the zero point of intellect, the distance of level A was measured in terms of the P.E. of 'expert opinion.' Illustrations of tasks of zero intellectual difficulty are, "Will not try to pull off his own fingers or toes," "Having an object of bitter nasty taste in his mouth will spit it out more often than hold it."

Three measures of intellect are obtainable from the *CAVD* examination: altitude (or level), width, and area. The altitude or level score locates an individual on a scale of intellectual difficulty graduated from zero as the point of reference. The width score (defined as the percentage of successes at one or more levels) gives the scope of intellect *CAVD* at given difficulty levels. The score for area (defined as the total number of tasks performed) measures the range of intellect *CAVD* throughout the scale. The curve for age with altitude *CAVD* is parabolic and rises from 0 to about 20 years, becoming approximately level at that point. But after 20 years, in Thorndike's view, intellectual improvement may still take place in specialized fields of endeavor (p. 468).

Many findings of first-rate importance in mental measurement and theory must be passed over lightly. Several which should be at least mentioned, however, are (1) a method whereby scores on stock intelligence examinations can be transformed into a scale with equal units (Chapter VII); (2) evidence to indicate that intelligence is almost certainly normally distributed in children and is probably slightly skewed (positively) among adults due to differential gains after 14 years (Chapter VIII and Appendix III); (3) the proposal of a 'quantity theory' of intelligence, namely, that good and poor intellects differ not qualitatively but quantitatively, which is to say in respect of the number of connections which they possess. This view grew out of the experimental finding that correlations between 'higher' abilities, *e.g.* difficult completions, arithmetic problems, analogies, and 'lower' abilities, *e.g.* vocabulary, information, simple arithmetic, are as high as the intercorrelations within either group. This result would seem to suggest that there is a general intellectual level at which an individual habitually functions, and lends force to the recent friendly criticism by Professor Spearman (*Brit. J. Psychol.* 17, 1926, 365) that Thorndike has really stated his theory of the 'g' factor without, it may be added, subscribing to it.

Several criticims of a somewhat different sort will suggest themselves as one reads the book carefully. The style is often obscure and sometimes involved, and the frequent omission of proofs and illustrations does not serve to make, the argument any easier to follow. One will be struck, too, by the frequent need for judgment for deciding what value to take or what procedure to follow, and will wonder whether the *CAVD* scale would remain fundamentally unchanged if the steps in its construction were repeated by another investigator. Again, the almost blanket use of the Spearman formula for correcting for 'attenuation' will seem to many to require justification. Probably no one knows better than Thorndike, however, the very definite assumptions upon which this formula rests; and at least one must admire his scientific honesty in printing all of his corrected r's even when they reach the astonishing height of 1.40.

To be sure, objections such as these weigh lightly against the body of solid achievement in this extensive research. The importance of the *CAVD* scale lies not so much in its extension of the scale for measuring intellect accurately far beyond that of any other scale, nor in the fact that it permits for the first time the "times as much" statement of intellect from an absolute zero. Its real value lies in the method here laid down whereby other scales for measuring traits and abilities, as well as intelligence, may be constructed. Future research may, and probably will, change the values of certain levels and shift the zero point up or down. This is of relatively small importance. The fact remains that Professor Thorndike has enormously clarified and extended our knowledge of mental measurement; and for this achievement, he is deserving of the thanks of all students of quantitative psychology.

Columbia University H. E. GARRETT

Social Psychology. By F. H. ALLPORT. Boston, Houghton Mifflin Co., 1924. Pp. xiv, 453.

This book represents an able and interesting attempt to write a social psychology in mechanistic and behavioristic terms. Social psychology is defined as the science of social behavior (*i.e.* behavior which is socially stimulating or stimulated) and the social consciousness of the individual. In view of the behavioristic tone of the whole, the inclusion of consciousness in the subject-matter may prove a trifle surprising; and, in view of the author's belief that consciousness may accompany the mechanistic sequence of stimulation, neural transmission and response (but is never a causal link), his emphasis on it may appear gratuitous. But it is difficult to see how the facts of social psychology can be described in even moderately adequate fashion without reference to consciousness and introspective evidence, and, if, in reading the chapters on response to social stimuli and social attitude, we sometimes forget the epiphenomenal character ascribed to consciousness in the earlier chapters, that is perhaps the fault of our bias. The term "social consciousness" is not used in the sense traditional in this field, but here denotes individual consciousness accompanying the social attitudes.

The various group or collective-mind theories are, rightly enough, discarded at the outset. The only behavior and consciousness to be studied are that of the individual. The author carries out his study of social phenomena without feeling any lack because of discarding these theories. It is perhaps true that the last chapter, "Social Behavior in Relation to Society," which deals with that group of phenomena in explanation of which the hypothesis of the group mind is usually invoked, is weaker than the rest of the book; but may we be so bold as to say that this failing is not due to the lack of such an hypothesis. The author is content in that chapter to deal with his subject rather in the fashion of a sociologist who dabbles in psychology and economics than as a psychologist who applies his principles with discernment to the larger and more interesting group phenomena.

Instincts are also discarded, but less successfully than the group mind. Instead of instincts we find prepotent reflexes, which are also drives and demands. The chief advantages of prepotent reflexes over instincts are that they are fewer, simpler (since they involve only the simplest stimulation and crudest movements in response), and, save for the sex reactions, are all manifested shortly after birth and therefore do not involve the difficulties of a maturation theory. The prepotent reflexes posited by the present author are six: starting and withdrawing, rejecting, struggling, hunger reactions, sensitive zone reactions and sex reactions. From these as raw material the whole story of man's activity is derived by the principles of conditioning and trial-and-error learning.

It is evident that much that is considered instinctive and fundamental by other writers must on this plan be derived from these six prepotent reflexes. Thus, we are told that curiosity, hunting, hoarding and constructiveness are all derived from the tendency to manipulate and that in turn from the mouthing reflex (hunger). This is obviously a rather difficult development to trace and the author dismisses the matter in a few sentences.

When we come to what other writers speak of as the social instinct we find here only social habits. Reactions to social stimuli are differentiated from those to non-social by learning. The "universal tendency to produce reactions in others," that is, social behavior which is sought as an end in itself, is derived from the habit, developed in early childhood, of controlling parents to secure satisfaction of bodily needs; the mechanism is the "hurt cry." The force of habit and the desire to control others, then, form the basis of the whole social side of life. Difficult as it may be for some of us to accept this in regard to the facts of human life, it becomes nearly impossible to credit it when we turn to the animals. How does it happen that—given in each case a set of prepotent reflexes which contain no fundamental social element—the dog develops a "tendency to produce reactions in others," yes, even in his human master, and the cat lives all her life in sovereign indifference to such low diversions? Defining social behavior, as the author does, in such a way as to include the stalking of a mouse by a cat (linear social behavior) obscures a distinction that is absolutely vital in the study of social psychology. We know that some animals combine with others for various purposes—such as hunting, self-defense, migration, and even for purely social dancing, singing and games—while others, save for a brief mating season, lead highly individualized and solitary lives. Social psychology must provide some principle for the explanation of this difference before it can adequately cope with the facts of *social* behavior. It is highly doubtful whether it is possible to derive a satisfactory description of social behavior from any elements—be they instincts, prepotent reflexes or drives—that do not contain some natively social factor.

This difficulty is brought out in the discussion of McDougall's principle of primitive passive sympathy. The facts covered by this principle are partly explained by the circular responses, which involve one's having experienced an emotion while observing the expression of it in others in order that the emotion be awakened in us by its expression at a later date; but the author notes also the operation of what he calls social facilitation, which he cannot explain. Nor can this be explained on any theory that does not involve some social factor—call it instinct or what you will.

The limits of a review prevent the discussion of all the questions of interest raised in the book. Mention should be made, however, of the chapters on personality, which show the influence of the recent work in that field, and of the very interesting chapter on the development of language and gesture.

Harvard University E. C. Gilbert

Die Lehre vom Raumsinn des Auges. By F. B. Hofmann. Berlin, Julius Springer. Vol. I, 1920, pp. 213; Vol. II, 1925, pp. 214-667.

Physiologists and psychologists the world over will regret the untimely death last spring of the author of this and other works of interest to them both, primarily in vision. Professor Hofmann had achieved an enviable reputation for scholarly work and careful observation in his contributions to Tigerstedt's *Handbuch der physiologischen Methodik.* Few writers possessed to such an eminent degree his ability to select and coordinate facts from the literature at large, presenting a coherent picture of the present state of knowledge about the subject in hand. With this power went the capacity for painstaking observations which illumine his texts. It is these little experiments that enliven and entice the reader when the text would otherwise be tedious, if not dreary, for Professor Hofmann collated and wrote in true German fashion. The two volumes we have at hand are separate editions of the first and second parts of the 13th chapter of the *Handbuch der Augenheilkunde.*

Since a detailed review of a work like this is impossible, we shall have to confine ourselves to a general discussion of the scope of the work, the style of the author, and his contributions to fact and theory. Certainly the title does not belie the contents of the book: every conceivable factor that should be treated under the perception of space has either a separate chapter or paragraph devoted to it or is given due mention, In fact, the word *Lehre* in the title is slightly misleading if we translate it as 'theory' because the book is full of facts and contains less reference to theory than we might expect. It embodies the known facts concerning spatial perception and if *Lehre* is translated the "teachings" we have an accurate designation for the work.

We have said that the important facts from the whole literature of space perception have been brought together and discussed. It is done not only by the printed word but also by giving numerical results, curves embodying experimental work, and numerous plates and drawings. Opposing views and facts pointing in different directions are quoted in their proper places so that the reader will get an idea of the 'ins and outs' of the problem under discussion. Often when no apparent solution emerges from what would otherwise be a welter of fact, the author points the way by suggesting some theory from the literature, or some fact from his own work, or the type of investigation needed to bring order out of chaos. What Professor Hofmann's industry has accomplished may be gleaned from the fact that his bibliographies contain over 2000 references to books, monographs, and articles. So complete is this work that those who wish to deal with any of the many problems comprehended under the perception, *e.g.* the illusions, visual acuity, the perception of form and *Gestalten*, localization, filling in of the blind spot, correspondence of retinal fields, can neglect this work only if they have mastered the sources from which this author has drawn so plentifully.

In his treatment of the subject the author has assumed that the reader possess some knowledge of the basic sciences, including of course, mathematics. This is a sign of the healthy condition of physiology and points the way to the better psychology. Since the treatment is so detailed one wishes that the author had assumed a litle less on the part of the average graduate student and given him facts (from physics and physiology) which bother him so often. For example, in discussing irradiation and its physical, retinal, and psychophysical causes, the student might well assume that even though the retinal and perceptual field do not have sharp boundaries, the physical source (say a narrow slit) may have, whereas, as everyone knows who has worked with the tiniest light source in a dark room, and in accordance with Huygens' principle, we cannot take for granted the physical boundaries of a visual stimulus, sharp as they may appear against the surrounding field. A word of caution here and there from the author on just such points would have been of great help to those most likely to use the book. In such a work as this, too, the need of theory is felt on every hand: to coördinate facts, to bring the one out of the many, and to establish principles that are the beacons of science. It is here that the reviewer senses the greatest shortcomings of the book.

Remembering that these volumes form part of a larger work, *Physiologische Optik*, the psychologist will naturally be interested in the contribution which Professor Hofmann as a physiologist and which physiology as a science have to make to the psychological problems involved. It is at this point that the discriminating reader will be at least a little disappointed. For where we might expect some enlightening physiological hypothesis or fact to clear up a situation, we find the usual references to attention and past experiences as the maids of all work. Although we have good reason to expect the physiologist above all others to be weary of such easy psychological explanations, we too often find him resorting to these and supposedly psychological mechanisms to fill the gaps in his physiological knowledge. Thus Hofmann accepts Hering's explanation of the great distance between the 50 and 100% points in the perception of difference in position as due to attention; since attention is not devoted to small differences in the retinal images, they pass unnoticed (vol. I, p. 61). This explanation is also supposed to hold for the fact that the perception of small differences is lowered with practice. If one began to ask questions of the facts supposed to be explained by the mysterious activity of attention, which can do most anything, and Übung, he would find the most interesting set of problems of any proposed by the author. The psychologist will therefore find no startling hypotheses (which, perhaps, should not be expected in a *Handbuch*) and certainly he will find many of the hackneyed explanations of the psychological texts when the author trenches upon psychological material.

Die Lehre vom Raumsinn is recommended to those who, wishing some orientation through the intricate maze of factual material of the subject, have mastered their German sufficiently well to read a rather difficult and condensed style and one abounding in the technical terminology in which the subject is enmeshed.

University of Kansas HARRY HELSON

Vom Fühlen, Wollen und Denken. By THEODOR LIPPS. Third revised edition, Leipzig, J. A. Barth, 1926. Pp. vii, 275.

A revision of the 1902 edition, but so different that the author regards it as a new book, the chief departure being indicated by the sub-title, "*Versuch einer Theorie des Willens*." This turns out, in fact, to be an extension of his theory of feeling to cover the data of volition. He begins, as usual, with his analysis of feeling.

Common speech often refers to feeling as objective, while psychology, on the contrary, treats warmth and cold, etc. as *empfunden*, while pleasure and displeasure are *gefühlt* as subjective experiences. What is the ground of this distinction between the subjectivity of feeling, and the objectivity of sensation? It is not that feelings are different from sensations in their *quale*, because the sensations are also qualitatively different from each other. Lipps thinks that the difference lies in the consideration that the feelings are *Ich-qualitäten*, that is, that in feeling we literally 'feel ourselves,' just as in sense experience, we sense a not-self. The objectivity of sensations and the subjectivity of feeling are not inferred—they are *lived*. The *ich* is not given us, we live it, *i.e.* we live the "qualitäten des ich." This view comes to the formula, "I feel, therefore I am," for since a feeling undoubtedly exists when we have it, so must the self exist also. The 'I' is the entity that remains when we strip away all else. The objective world has meaning only because the subjective self exists which is the only thing it could be an 'object' to.

So far, the 1907 edition seems little modified, if at all. Lipps maintains his old objection to treating *Lust* and *Unlust* as properly feelings—they are only colors which feelings take on. The pleasure of the comic is not like the pleasure of friendship, etc. What is unique in each, *i.e.* what is genuine feeling, is rather the element which other psychologists call emotions. The fundamental thing about a feeling is that it is an *Ich-erlebnis*. There are other important things about it, however. Every feeling is *Tätigkeitsgefühl*—a feeling of personal reality, which means at once the reality of *what* we feel, such as anger, fear, or whatever other feeling we 'are.' Another great characteristic of feeling is the inner excitement which we know as the impulse to attain a goal. He employs the word *streben* in a general sense to designate conation, wish, desire, longing. The universal condition of conation (*streben*) is that it is a process having direction and a goal. It is when inhibition arises (*Hemmung*) that conation becomes apparent. He analyzes *Apperceptionstreben* and association and finds in them the above elements of goal-seeking and inhibition.

Lipps distinguishes as passive conation the cases in which an impulse in us, which the whole personality more or less disowns, strives for its goal. Such experiences may appear as a striving for the unpleasant. While enduring it, we feel constrained to seek a certain end which we sanction with reluctance. Opposed to this we have active conation which is the activity of the unopposed personality, which is a striving for the pleasurable, and is the normal condition. Passive conation is really a striving of a fractional part of the self, it implies dissociation, and is not the normal condition. Negativism is to be understood as an instance of passive striving, in which the pleasure-aim of the subject is replaced by its opposite, always potentially present, which now becomes kinetic because of the fact of its dissociation from the bulk of the personality.

From now on Lipps develops and defends the general theme that all experience is fundamentally the realization of selfhood. Whatever end we strive for, we strive to enjoy it. This means, we strive for something which is *realized* in its enjoyment. But, according to his earlier judgment, this enjoyment is an immediate state of the self, hence the attainment of enjoyment is a pure instance of self-realization. For instance, in seeking any sensory experience, we are seeking, not that an object have this sense property, but that we experience this property. To be sure, we may wish to have others to have this experience, but only because this enjoyment by others is something in which we have an interest—and realized interest means self-realization. Association and apperception are processes which realize knowledge for us. But, like a miser who wants gold in a casket so that he may have the feeling of the pleasure of power, so the attainment of knowledge is primarily a pleasure-end. Perhaps these are struggles for phantoms, but the point is that the attainment gives a realization of the self, and this is no phantom. The activity of association and apperception may be play, but they are the play of the soul—it has no more to ask.

The ultimate factor which unites all ends and aims of conation is this fact that they share in one personality, and are its realizations. This is the 'ground' of all conation. However, this is, in the author's case, far from crude hedonism. First, because he emphasizes the higher strivings (as for knowledge) as well as the desires for sense experience. More important still, he denies that spiritual realization is an individual fact. The central aim of all drives is to realize the *human*. We must hold to the principle that "nichts Menschliches kann mir Fremd sein." But this is taught us by reason, to which all strivings must be conformable. He sanctions Kent's law for the will, that we should so 'will' that the humanity within us and in others should have the same goal. This is the 'good will,' which alone is good. But Lipps ends, as he started, by emphasizing the fundamental importance of feeling. "Wir sahen, dass und wiefern dieser 'gute Wille' in seiner absoluten Vollendung das absolut kraftvolle Fühlen und das alles umfassende Denken und Erkennen in sich schliesst."

Wells College C. O. Weber

Les Mécanismes Subconscients. By Georges Dwelshauvers. Paris, Félix Alcan, 1925. Pp. 145.

Dwelshauvers attempts an experimental examination of the conscious and unconscious mental image, in the endeavor to trace its characteristics, functions, and relationship.

His experiments involve (1) the exposure of situations, words, and phrases calculated to arouse typical images; (2) an examination of "l'attitude musculaire," in which the subjects were required to draw a series of lines to the beating of a metronome; (3) a study of the relation between the "reflex graphique" imperceptible movements of the fingers called out by attention to a swinging pendulum—and the mental image; (4) a study of the image resulting from exposure of certain complex geometrical figures; (5) an examination of the performance of various successions of hand and foot movements to demonstrate the relation between the image of a motor act and its resultant execution; and (6) a comparison of ability to draw a series of lines with and without the distraction offered by simple arithmetical problems.

Our subconscious life is, Dwelshauvers believes, a mass (polypier) of images. The mental image is subconscious in its formation and characteristics; and, in the subconscious life peculiar to it, participates to an important degree in the physiological activity of the individual. In this activity the image becomes an intermediary between the organisms and the conscious life of the organism, making it possible for conscious life to "gain a clear idea of the organism." Our muscular attitudes translate themselves into images; "the motor attitude imposes itself through unconscious channels as a necessity for action, and causes to surge automatically into consciousness certain images, schematic in nature and already conceptualized, which are utilized to maintain and perfect movements." Such images are the "inscription on consciousness of the regulatory mechanism, ... and serve as intermediary between the organism and consciousness." In day-dreaming and in dreaming proper, imagery is especially plentiful, negating entirely ordinary rational synthesis, and bringing into prominence the organic life of the individual. The functioning of the organic life presents itself most easily to consciousness through mental images of a visual nature; next in the hierarchy of images comes the auditory, which is less stable than the visual; then the tacto-muscular activity, which but seldom translates itself into images; and, finally, the gustatory and olfactory images, which readily prove their direct affinity with the "subconscious sensitivity of the digestive functions." The constitution of the automatic functions, then, is the internal work of that animated and multiform process which appears as a mass of images in the moments when it imposes itself upon consciousness.

The intermediation of the image is only quasi-conscious, however, since images are few, at best, during the activity of attention or violition. Thus, "we believe," the author says, "that we have recognized the existence, in the nervous system, of a mechanism of regulation and coördination which is no other . . . . . than the mental image itself, and of which the seat appears to be in the mass of associative paths found in that system." It is particularly important, however, that in such work the subjects learn to distinguish between the mental image and association of ideas, since, if such procedure is not observed, experimental examination of the mental image is impossible.

University of Michigan T. C. SCHNEIRLA

Introduction to the Psychology of Religion. By F. S. HICKMAN. New York, Abingdon Press, 1926. Pp. 558.

The psychology of religion has reached the text-book stage: Charles C. Josey's *Psychology of Religion* and the book now reviewed are both elementary texts for undergraduates.

Hickman has evidently labored long and painstakingly at his difficult task, and has achieved a fair degree of success. But one may foresee that an early successor will greatly improve upon this *Introduction.* For the book is heavy, it neither presents new views nor a clear and easily grasped picture of those parts of the psychology of religion which may be regarded as established. If nothing is discussed thoroughly—something quite excusable in an elementary book—everything is set forth in brief paragraphs: introspection *versus* objective study; structural, functional, and behavioristic psychology; the orginal nature of man; the modification of instincts; subconsciousness; sublimation; etc. The book has five parts: Background of the Problem, Major Factors in Religious Experience, Genesis and Growth of Religious Experience, the Nature and Function of Worship, and Psychological Aspects of Religious Belief. Everyone of these parts falls into many chapters and every chapter is abundantly subdivided. The first chapter (The Genesis of Religious Experience) of Part III, for instance, has two main subheads. Under "Incentives and Reactions" are found the following subdivisions: The sense of need fundamental, Lower levels of need, The tendency to idealize, Idealization in religion, The feeling for values, The Growth of the feeling for values, and Religious revaluation. Under the second subhead (Principal Phases of Religions Experience) there are the following divisions: Religious control of the will, The feeling phase, Rise of the tender emotion, The faith feeling, The thought phase, and Summary—all this in a chapter of 24 pages!

The outcome of this conscientious effort to provide little dabs of information on an endless number of topics will be, I fear, to leave the student in an unenlightened and confused state: he will not have seen the forest for the trees.

Two excellent features are the addition to each chapter of a list of "Questions for study and discussion" and a selected list of references. I note the omission from those references of my books, *The Belief in God and Immortality* and *The Psychology of Religious Mysticism*, although the problems of these books occupy a large part of Hickman's volume.

It is unfortunate that his understanding of religion is such as to include magic. Thus he continues the confusion for which Durkheim and his school are mainly responsible. One regrets also that although the author has defined religion as a relationship with some power or powers higher than man (55), he seems here and there to pass over to the camp of those sociologists who identify religion with benevolent social mindedness. In a section on "The range of religious habit" (82-83) we are told, for instance that "the habitual attitude of brotherly sympathy toward the unfortunate" is a religious habit. Religion thus becomes whatever is socially good!

Bryn Mawr College JAMES H. LEUBA

Das Problem der Wirklichkeit. By ANTONIN PRANDTL. München, Ernst Reinhardt, 1926. Pp. 128.

Professor Prandtl is not concerned with any absolute reality. The latter is supposed to exist entirely independent of us. But it is evidently impossible, he argues, to think something which at the same time is independent of all thought and rests merely upon itself. Everything which is to be present in our consciousness cannot help bearing the stamp of our nature. Idealists and materialists have both been mislead. Their reality, whether it is an idea or the tangible (*das Greifbare*) is nothing but an assumption for which no sound reasons can be discovered. When a philosophy judges the value and reality of all things by an abstract concept, it can yield nothing but *Widersinn*. For this reason the author hints more than once—beware of the spooks of metaphysics.

The criterion by which the author wishes to determine the objective reality of all things, is uniformity of behavior. Reality has been divided, by philosophers and psychologists alike, into a physical and a psychical world. Insofar as the words physical and psychical are intended to denote different objects, this distinction is not valid. To define these terms more correctly the author avails himself of the labors of Mach, Avenarius and Ziehen. He does not accept the views of these three scholars in their entirety; he wants to be more thorough-going than any of them. They did not overcome completely the dualism expressed in the words physical and psychical. The author denies without any reservations the existence of psychical objects different as from physical objects. There are not two kinds of objects: one physical, the other psychical. Reality is one; it is not part physical and part psychical, but either physical or psychical. It is physical when it is viewed as determined by laws independent of us. It is psychical in view of the fact that in order to be experienced, it is dependent upon brain processes. Since the essential characteristic of the real in the physical world is uniformity of behavior, the problem of reality resolves itself for Professor Prandtl into the question: Can the principle of uniformity be extended to the psychical? The psychical consists of neural processes and mental states. Uniformity of mental states cannot be established; there remains uniformity of neural processes. The author is fully aware that the uniformity of neural processes can not be absolute.

Pure necessary uniformity can exist only between concepts. In order to arrive at an absolute uniformity we must follow the procedure of mathematics and form ideal concepts. But to be true, the ideal concepts must be workable, they must fit in with the physical world.

Although it is impossible to prove the uniformity of mental facts Professor Prandtl can point to the uniformity of neural processes. But what he wishes to prove is not that neural processes have uniformity, and therefore they are real, but that the psychical possesses uniformity, therefore it is real. It is not a question merely of adopting the ideal concept of necessary uniformity for the neural processes. Every mental state, the author assumes, has its specific neural correlate, for if he wishes to deduce the uniformity of mental states from the uniformity of brain processes, a complete correlation between brain events and mind events must be presupposed. It is a well-established fact that certain mental states have neural correlates. But "that there is a one to one correlation between events in a mind and events in the brain and nervous system of the body which it animates. . . . is a perfectly enormous assumption unless there be some general metaphysical ground for it."

University of Iowa B. Tapper

The Tired Child. By Max Seham and Grete Seham, with a foreword by I. A. Abt. Philadelphia, J. B. Lippincott Co., 1926. Pp. xvi, 342.

Most psychologists will welcome this volume because of the innumerable requests which they receive for the recommendation of books on child health suitable for parents and teachers of little training. It is not fundamentally a psychological work although it is not without the inclusion of much material of psychological interest. It is fundamentally a simple handbook on child hygiene designed for the use of parents especially, but also for the teacher and physician who may not be well trained in the subject matter. The style is clear and there are many appealing stories told of tired children and the causes therefore. Numerous photographs help to maintain interest and also to emphasize the precepts given.

The book is divided into three parts. Part one consists of four chapters which deal with the fundamental principles that underlie work and fatigue. These chapters include a sketch of the normal course of physical development with the usual tables of growth figures. Part two comprises many short chapters on the nature of fatigue, its recognition, its causes, and the stories of some typical cases of tired children. The third and last part is devoted to the prevention of fatigue. There is presented some good advice on the management of the tired child, the rôles of the school and the teacher and the parent, the diet, sleep habits, play, home study and the study of music, and concludes with a general chapter on the mental hygiene of the child.

Most of the material of this book is outside the field of the psychologist. It is prepared by authors who have had much experience with the medical problems they present. As the present reviewer is not a physican he is keenly aware of his inability to examine critically much that is contained herein. In fact he does not care to do so. He prefers to think that it is all scientifically sound because it is so happily presented. It would be a pity if it were otherwise. But as a psychologist he could not avoid noting as he turned the pages, some surprising and some disappointing features. In the presentation of the nature of physical growth, for instance, the authors present the old saltatory notion of growth without mention of the fact that the latest studies seem to indicate the contrary. Baldwin's conclusions on this point are not mentioned although his work is listed in the bibliography. Most of the chapters are followed by bibliographies, some of which are very long, but in them one finds references given indiscriminately in all languages and to anything no matter how obscure its place of publication. The type of reader for whom the volume is designed could not, in most instances, read these references if he lived where they were obtainable. The scientific reader who attempts to use these bibliographies will soon become exasperated by the many defects in presentation. There are frequent omissions of important items, anything in fact from parts of the author's name to the page of the reference may be missing. One wonders also what principle guided the authors in the selection of titles for these bibliographies. The one on fatigue, for example, omits Öffner's valuable summary entirely, includes Weichardt's dubious work, and omits Lee's studies which so effectively controvert the Weichardt myth. The rôle of the teacher's health is presented without reference to Terman's book on that subject. The effects of the loss of sleep are mentioned with an imperfect reference to the study of Gilbert and Patrick but without mention of the later studies by Robinson. Howell's paper on sleep of 1897 is mentioned but Shepard's elaborate monograph of 1914 is omitted. These are not of course serious defects from the point of view of the purpose for which the book was designed; and for that purpose, the instruction of the layman, it should be of great service.

University of Oregon EDMUND S. CONKLIN

Introduction to the History of Science: Vol. I, From Homer to Omar Khayyam. By GEORGE SARTON. Baltimore, Williams and Wilkins Co., for the Carnegie Institution of Washington, 1927. Pp. xi, 839.

Psychologists will wish to have their attention directed to an unusually important publication in the general history of science. Dr. Sarton, the editor of *Isis*, has now issued the first volume of his encyclopedic "Introduction," which is an introduction to the history of science, not in the sense that it is brief or elementary, but because it introduces persons of thoroughly scholarly interests to the primary and secondary sources for a particular period or individual.

This volume covers the period from the ancient Greeks to the end of the eleventh century. The author omits ancient Egyptian and Chinese science, because so little is positively known about either; but the student, who is accustomed to think of the "dark ages" as a time when there was no science, will be amazed at what he finds in this book. To most scientists the chapter headings of the last five hundred pages will be entirely unknown names of important men representing the successive periods: Diophantos, Iamblichos, Oribasios, Fa-hsien, and so on, to Abu-l-Wafa and al-Biruni. There is so much to say of these twenty centuries that Aristotle is reduced to nine pages and Archimedes to three.

In a way the volume is a "Who's Who" and a bibliography. There are, for example, two condensed pages about Aristotle; the other seven are a scholar's bibliography with the informative comment about sources. The condensation is so great that complete sentences are avoided where possible. The book is not for continuous reading; it is, in the author's phrase, a "tool"—a 'machine tool,' one might say, because of its bulk and complication.

Only the work itself will give an adequate impression of its thoroughness and scholarship, of the care exercised for accuracy, of the extent of the informative evaluation, of the meticulous transliteration of Arabic, Chinese, Greek, Hebrew, Japanese, Persian, Sanskrit, and Syriac. (Every Chinese word is expressed in roman letters with the tones indicated by superscripts and a number added which indicates the exact Chinese character among the odd ten thousand.)

Such tools of scholarly workmanship are rare. We may thank the author profoundly, and also be grateful to the Carnegie Institution which made its publication possible.

E. G. B.

The Case For and Against Psychical Belief. By OLIVER LODGE, A. C. DOYLE, F. B. BOND, L. R. G. CRANDON, MARY AUSTIN, MARGARET DELAND, WILLIAM McDOUGALL, HANS DRIESCH, W. F. PRINCE, F. C. S. SCHILLER, J. E. COOVER, GARDNER MURPHY, JOSEPH JASTROW, and HARRY HOUDINI. Edited by Carl Murchison. Worcester, Mass., Clark University, 1927. Pp. 365, 10 pl.

The papers brought together in this volume were written for the symposium on psychical belief held at Clark University Nov. 29–Dec. 11, 1926. The book is divided, according to the views of the different authors, into 4 parts. Part I contains the papers of the first six authors named above, who are "convinced of the multiplicity of psychical phenomena." In Part II appear the papers of the next four authors, who 'believe,' but who are "convinced of the rarity of genuine psychical phenomena." Coover's and Murphy's papers compose Part III, which gives the views of those who are presumably open-minded but are "unconvinced as yet" regarding the genuineness of the alleged phenomena. Jastrow's paper and excerpts from Houdini's *A Magician Among the Spirits* are given in Part IV as representative of those who are "antagonistic to the claims that such phenomena occur."

Ten writers who are convinced of the authenticity of psychical phenomena are set over against four who are not! This hardly bears out the statement made in the preface, "we guarantee fair play in the conduct of this symposium." Such a preponderance of affirmative witness is misleading; the uninitiated, for whom the volume is intended, will wrongly assume that this represents the distribution of competent opinion upon the subject; whereas, in point of fact, as Jastrow's questionary returns show, just the opposite is true.

The symposium is biased in another important respect. The question of psychical belief is not disposed of by a discussion pro and con of the spiritistic hypothesis. Of equal importance to the spiritistic, is the hypothesis that explains the alleged phenomena in terms of the medium's hypersensitivity and super-normal ability. Of the 10 proponents of psychical belief who participated in this symposium, 8 are professedly spiritistic, the other 2 are non-committal, but they appear to the reviewer to have spiritistic leanings.

One view of psychical belief has many witnesses, a second has but few, and the third has none! The "rôle of the parliamentarian" was badly cast. The volume is too one-sided to fulfill the hope expressed in the preface that it "should remain for many years the authoritative source for expert opinion concerning the case for and against psychical belief."

K. M. D.

Psychological Principles Applied to Teaching. By W. H. PYLE. Baltimore, Warwick and York, 1924. Pp. vi, 197.

Pyle's book consists of a list of 115 'principles' of psychology, every one of which is followed by suggestions for application to schoolroom problems. Alternate pages of the text proper are left blank for the teacher's notes. The author says "the book is intended to serve as a manual for the daily use of teachers in grade and high schools." He presupposes "previous study of psychology" by the teacher and suggests that "the result of the [first] reading should be a definite organization of the facts of educational psychology."

It is to be regretted that the book itself is so markedly lacking in organization. There is no grouping of related topics nor such as to indicate relative importance of items. The 'principles' range from the statement that causes "of individual differences are (1) hereditary and (2) environmental," to the pedagogical dictum that "the teacher should provide special incentives to effort." Presumably the author's *Psychology of Learning* is to serve as the basis of organization since page references are given to this book. There is nothing on the title page, however, which indicates that the book was designed especially to accompany the earlier book. The lack of organization of the book being reviewed is very likely to convey the false impression that teaching is a matter of applying isolated principles of psychology.

Some of Pyle's statements seem to the reviewer to be rather seriously misleading. For example: "To learn is to become different. If the difference is not retained we have not learned" (70). What degree of permanence must there be reached in order that there be learning? Again: "If a pupil learns to concentrate all his energies on an experiment while in the biology laboratory he will be better able to concentrate on other work" (160). There is no warning against the inference which the reader is likely to make that the pupil will concentrate better in other work.

Among the applications to schoolroom procedure are a number that should provide good points of departure for a class discussion of the principles involved.

Cornell University PAUL J. KRUSE

Psychology for Child Training. By A. D. WEEKS. New York, D. Appleton & Co., 1925. Pp. 312.

It seems indeed unfortunate that the popular misconception of what psychology is should be fostered by naming a book of suggestions to parents on the training of children, "Psychology for Child Training." The statement in the preface that "the entire treatment rests upon stated principles of psychology" is unfortunately not borne out in the text. "Instinctive endowment" is stressed through eleven chapters treating such general and such specific tendencies as curiosity and "the travel interest." That the "hunting instinct" is expressed in children's play is supported by the statement that "at a birthday party a peanut hunt proves the most delightful play."

The author yields to the temptation to treat the structural basis of behavior in such terms as, "the child inherits an intelligence mechanism. The physical structures of intelligence include nerve cells, nerve fibres, the brain, and the organs of sensation" (p. 28). "Of special educational significance is the synapse. This term refers to the fitting together of fibres leading from one cell body and those leading to the adjacent bodies" (p. 29).

A contribution to our knowledge of human behavior is made through the presentation of personal reports, presumably by college students, of their behavior—supposedly illustrative of tendencies to fighting, display, fear, and reasoning. The relation of these reports to the suggestions for child training, however, is in most cases not clear. A good example of the "definite assistance" promised in the preface is the sound advice to parents on the child's use of money (p. 227).

The reviewer takes this opportunity to enter a plea with authors and publishers for more honest titles to books.

Cornell University PAUL J. KRUSE

Der moderne Dandy. By OTTO MANN. Berlin, J. Springer, 1925. Pp. 128.

The 'modern' dandy dealt with by Mann is not the present day 'dandy' but the representative of dandyism as found in England and France during the nineteenth century. Brummell, Wainewright, Wilde, Barbey d'Aurevilly, Baudelaire and others who sought "to find expression" by means of "pen, pencil, and poison," or who considered life as an art which has "its modes of style no less than the arts that seek to express it," should be named here. The author gives an analysis of the time and the society which produced dandyism, and the persons who represented it, and brings home to us the 'ideal' of the dandy, his 'methods,' his 'system' and the 'consequences' of his life: crime, catholicism, perversion or adjustment to 'normal' life.

The book, written in admirable German, represents the first volume of the *Philosophische Forschungen*, edited by Karl Jaspers, Heidelberg. It may be taken as a contribution to the psychology of 'understanding.' A consideration of its method and its results would lead to an inquiry into the type problem. "In the last analysis, the truth of a type is not something which can be directly determined by the rational methods of science" (p. 81). It is, in the last analysis, 'intuition' which creates the unity of a type by connecting apparently disconnected empirical phenomena. All that science can do is to bring out the different relations which have been discovered intuitively and at the same time avoid any undue exaggeration of any of these relations.

It seems to us that the 'truth' arrived at in such a way is strikingly similar to the 'truth' represented in Hebbel's dramas. At the same time Hebbel was, conceivably, more successful in not exaggerating any of these 'symptoms' of an historical period. The question arises whether cultural psychology which seeks to understand can do more than give us the physiognomy of different historical periods. It may even be a fact that the historian relies chiefly on his 'physiognomical tact' in handling 'facts.' Such a result would seem disastrous; but, if a fact, one should not hesitate to draw the necessary conclusions.

Columbia University HEINRICH KLÜVER

The Psychology of Coaching. By COLEMAN R. GRIFFITH. New York, Charles Scribner's Sons, 1926. Pp. xii, 213.

Professor Griffith's book is a study of coaching methods from the point of view of psychology. The athlete offers himself as an excellent laboratory for the investigation of various physiological and psychological problems. The author suggests that "if psychological theory had begun with studies on athletes instead of with studies on reflective philosophers we might have had a more balanced science." The book aims to aid athletic coaches in sharing their knowledge in an effective manner and to present the dignity and social value of the coaching profession. The coach should endeavor to build up personalities as well as to train men for competition in special skills. The profession of the coach should be regarded as centering upon such aspects as morale and self-control, rather than upon technical accomplishments. The coaching profession, so regarded, assumes genuine human value.

The athletic coach has big responsibilities, since he has much influence upon the characters of those with whom he has to deal. Athletic training should look beyond the stadium to the larger game of life. The coach should be an expert in the diagnosis and handling of all sorts of individual differences. Consideration is given in the book to the planning of the practice sessions, with due regard to incentives and the laws and principles of learning. There is discussion on how to present material effectively and on the planning of the season's campaign. A chapter is devoted to the problem of the 'jinx.' Professor Griffith deals with the 'yellow streak;' here is an important group of phenomena and conditions in the field of emotional response. The problem of will power is also discussed.

The book is well worth the reading by every one interested in the training of young men. It recommends itself not only to the athletic coach but also to those who are dealing with men in the class room. For there should come about a more and more sympathetic understanding between the academic and the athletic interests in our colleges and universities.

University of Vermont PAUL C. SQUIRES

Life and Word: An Essay in Psychology. By R. E. Lloyd, London Longmans, Green and Co. 1924. Pp. xvi, 139.

The author's contribution lies in his point of view, that all animals including man have differential vital qualities and certain fundamental common qualities, *e.g.* all animals breathe, the spine is characteristic of all vertebrates, and verbal-thought is characteristic of man. The author lays a great deal of emphasis upon verbal-thought as being the quality, *par excellence*, which sets man off from all the lower animals. He states that "sensation leads to thought and thought to word, but the step from sensation to thought seems much more important than the step from thought to word. The word or sign is merely the expression or fixation of the thought. How far thought is possible and effective without word, in the animal world, is scarcely knowable." Children, primitive races, and adults with certain diseases or injuries, seem to display thought in their behavior, for which they have no words; the same thought becomes known to normal adults and civilized people in the complete verbal form. The author develops this point of view further as a differential vital quality in organic evolution.

He has extended his biological training into the realm of philosophy and psychology, making as his psychological groundwork, the writings of Plato, Locke, Kant, Bergson, James, etc. None of the findings of modern psychology are mentioned. The essay partakes more of a philosophical than of a psychological treatment.

University of Iowa — Roland C. Travis

Educational Psychology: Its Problems and Methods. By Charles Fox. New York, Harcourt, Brace & Co., 1925. Pp. xiii, 380.

The author states in the preface that the "purpose of the present volume is to consider the contribution made by experimental psychology to the study of educational problems." He begins his book with a chapter in which he discusses the theories of mind, mental heredity, and the inheritance of acquired characters. Launching out from this chapter he carries the reader through a consideration of sensory data observation, imagery, feeling, neural basis of habit, meaning, impulses, imitation, memory, mental discipline, suggestion, psycho-analysis, aesthetic education, mental tests, and mental fatigue.

The book is well written. Fox's diction is clear and direct, and his presentation is interesting. One enjoys reading the book. The reviewer's chief criticism is that Fox runs to theory in his discussion, and gives too little space and attention to fact and practice.

University of Iowa — Lee Edward Travis

The Mastery of Fear. By W. S. Walsh. New York, E. P. Dutton & Co., 1925. Pp. xi, 315.

Walsh discusses in a very popular way the importance that normal and hysterical emotional behavior have for the diagnostic purposes of the physician. He thinks that fear is due to superficial causes or to some particular event in the previous experience of the individual, and that the physician, by discovering the root of the fear, can remove it by introducing habits that break it down.

Fear, according to Walsh, is the antithesis of many desirable aspects of instinctive or emotional behavior. On this basis, his classification of 'fear' is an immediate and all-inclusive one, and chapters are devoted to Fear of Old Age, Fear of Illness, Fear of Heredity, Fear of Maternal Impressions, Fear of Public Opinion, Fear of Ridicule, Unconscious Fear, Causes of Abnormal Fear, and The Fear Habit.

University of Michigan T. C. SCHNEIRLA

The Einstein Theory Explained and Analyzed. By S. H. GUGGENHEIMER. New York, The Macmillan Company, 1925. Pp. v, 336.

A highly popularized statement of and explanation of the main contentions of the new school of physical relativists. The new doctrines of time and space have significance for the psychologist, but this book does not bring out this significance in any fundamental way.

Wells College C. O. WEBER

Human Experience. By Viscount Haldane. New York, E. P. Dutton, 1926. Pp. xi, 231.

The chapters discuss basic philosophical problems in an elementary manner suitable to beginning students. There is little in this volume which bears on live psychological problems and nothing that concerns experimental psychology in particular.

Wells College C. O. WEBER

A Laboratory Manual in the Psychology of Learning. By W. H. PYLE. Baltimore, Warwick & York, 1923. Pp. 161.

In the Preface of this manual the author says it "is published in the hope of furthering and facilitating experiments in educational psychology." Pyle feels that laboratory work and class experiments should be included in every course in educational psychology, and his Manual is adapted to that end. He divides learning into various types, *e.g.* motor, associative, ideational, etc., and he gives, in outline form, an experiment or two in illustration of every type.

Cornell University H. D. POWERS

The Psychology of Elementary Education. By A. S. EDWARDS. Riverside Textbooks in Education. New York, Houghton, Mifflin Co., 1925. Pp. xiii, 333.

Elementary school teachers and student-teachers will be interested in this book, for the author's presentation is striking. The habit theory of education runs through the book, the greater part of which is devoted to learning and to the teaching process. In addition to numerous selected references and questions for discussion given at the end of every chapter, an excellent list of class experiments is given in the appendix.

Cornell University H. D. POWERS

NOTES AND DISCUSSIONS

The Plus Sign in Modern Psychology

A recent congress[1] of notable psychologists proved to be instructive and significant. It evidently marked a decided trend in present-day psychology. If we should mention names, they would be recognized at once as the outstanding ones in the world of psychological scholarship in this country and in England, France, and Germany. Thus in this essay we are not dealing with specialists of the second rate or the mediocre order. Many papers were presented either by person or by proxy, and thus many data could be gathered, and a large induction could be drawn from the numerous essays, addresses and discussions.

The writer was present at most of the sessions of the conference, although he was compelled to miss one day's sessions, much to his regret. He desires to give his impressions, and add some reflections on the plus sign that ever stood more or less distinctly before his mind while the erudite discussions were going—a plus sign that grows more distinct as he continues, since the congress, to mull over the great and significant situation. It is best, we think, to mention no names; then no one can feel that the personal element enters as a coloring motive into our observations.

It may be added that we listened with an open mind, with a keen interest, and with an earnest desire to know just what were the views of those eminent savants regarding the nature and causes of mental phenomena.

To begin at once, and to be entirely frank at the start, we missed the note of pure psychology in most of the theses which were presented. If there was a paper read at the conference which announced a clarion and positive belief in the mind as an entity, or that recognized man as a dual being, composed of body and mind, we failed to hear it. A few of the speakers did not use their voices very effectively, and so a few sentences may have been lost; but we do not believe that a distinct note of dualism was struck. There were, it is true, a few voices from the floor which at times seemed to indicate a feeling, or perhaps recognition, of the plus sign and its vital significance; but even those men, when they came forward to present their written theses, did not seem to want to ring out a clear and definite statement regarding the reality of the *psyche* as a thing in itself—*das Ding an sich*, as our German friends would say.

Indeed, at the first session some one had the temerity to rise and interject the question whether one of the German psychologists, whose paper was read by another in his absence, recognized the mind as an entity, interrelated with the body and having some measure of control over it; but, to our surprise, almost dismay, the question was ruled out of order as a "philosophical" question; therefore not relevant in a conference where 'pure science' was to be the sovereign spirit. It was difficult to understand why matters of such common elementary and almost universal psychical experience as the human mind should be taboo as belonging to 'philosophy;' for surely nothing can be plainer than that we have what are known the world over as mental experiences—experiences, too, which certainly cannot be attributed to mere material substances. At all events, it would be passing strange for almost all peoples to believe they have minds if there is nothing in the whole universe but matter. In that case matter would certainly perform a strange exploit in causing the vast majority of people to labor under the delusion that they had minds. What is matter about, anyway, to play such tricks on the majority of the people of the world?

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[1] The Wittenberg College Symposium on Feeling and Emotion, Springfield, Ohio, Oct. 19–22, 1927.

Afterward in the convention some of the essayists tried to prove that mental phenomena of the emotional order could be explained on a purely physical basis. Some one spoke of the mind as the "so-called mind," although he may have meant it jocosely. Another referred to the mind as a philosophical speculation, as a metaphysical abstraction; while another thought such words as "perception," "concept," and "notion" might as well be banished from language and thought. But one could not help wondering whether the attempt to explain mental experiences by mere physiological processes was not 'speculation' based on a 'philosophy'—namely, the materialistic philosophy. For when a man proclaims that there is nothing existent but material substance, he has entered the field of highly speculative philosophy. Besides, it must be a venturesome speculation which runs up directly against the almost universal experience of the human race; for no one has ever *experienced* that there is nothing but material substance in the universe; whereas thousands, yes, millions of people—sane people, too, to all intents and purposes—have had experiences of a psychical and spiritual character.

The conception of a mind as an entity was girded at by a couple of essayists as something "metaphysical." We pondered much over that statement. If by metaphysical was meant something *above* the physical order, according to the etymology of the word, then, to be sure, the idea of a mind *is* a metaphysical conception, and rightly so; but if metaphysical means, as is often conceived, something abstruse and difficult to grasp, and more or less vague even to keen minds, then there were loads on loads of metaphysics in this convention. We heard many people, even educated college and university professors, admit that many papers were so abstruse or abstract, or both, as to be almost unintelligible to them. Of course, many technical terms, mostly physiological, were used; and that made some of the papers rather difficult for people who had not recently reviewed their physiologies.

We have said that materialism is a philosophy. It is recognized as one of the three monistic systems of speculation, the other two being Idealism and Pantheism. Materialism says there is only one existent substance—matter. Idealism declares that the only entity is mind, the material universe being an illusion, a mere form of thought. Pantheism merges mind and matter into one substance, as Spinoza assumed but one substrate with the two attributes of extension and thought. The materialist attempts to interpret and explain the universe in terms of matter; and any attempt to do that is a philosophy.

The definition of psychology, until recently, has been, "The science of the mind." Its etymology implies that definition—ψυχή and λόγος. The term ψυχή means the soul or the mind. Now, if there is no such an entity as the mind, the term psychology is a misnomer, and its use is injurious to the cause of science, because it constantly suggests to people's thought something that has no real existence. Some people want to define psychology merely as the scientific treatment of "experience." But experience carries a very different connotation from that of psychology. The latter deals with something concrete—an entity, the mind; the former is an abstract term; for if there is an experience, there must be a something that has that experience; and, judged by all the laws of rational thought, that something must be conscious of its experience, otherwise an experience would be impossible. A stone or a block could not reasonably be said to have an experience. Hence to try to explain psychical experiences in purely physical terms is to leave so large a plus sign as to give the impression that the major principle comes after and not before that sign. At least, the attempted physical explanation, in the present status of the case, is largely speculative, even more so than the assumption of a self-conscious agent known as the mind.

A confusion of thought occurred in one of the learned papers. The author tried to explain the experience of fear on a purely subjective physical basis. He jibed at the usual explanation that the experience of fear arises from the appearance of a fearsome object, because, said the critic, that would be tantamount to saying that "you were afraid because you were afraid." Note the confusion of thought. The terrifying object is an objective reality, whereas the terror aroused is a subjective feeling. How can any one think so mistily as to fuse together an objective reality and a subjective experience? If a cross dog should suddenly appear before you and cause you sore affright, you surely would never afterward, in thinking over the adventure, say that the canine and your scare were one and the same thing. *Bene docet qui bene distinguit.*

Now, to enter into the heart of the subject, the stupendous and all-important plus sign in much of modern psychology is this: On a purely physical basis, how can we account for the dominant fact and universal experience of self-consciousness? That is the primary and basic question. The second is like unto it: Without self-consciousness how can we have an experience? For without awareness we could have no knowledge of anything that occurs, whether it were an external event or an internal experience. Arcturus is a very great star, but it does not know that it is sizable; it does not even know that it exists. But you and I are conscious of our existence and of myriads of other facts besides. Hence there is a difference, *toto coelo*, between a self-conscious being and an insensate material object. A denial of this proposition is contrary to all experience, and hence does not belong to the sphere of scientific psychology, but to that of pure speculation and conjecture. Nothing can be proved before the bar of reason and science that does not come within the field of consciousness. Even the most radical behaviorist must be conscious of his attempt to convince the world of his theory. His very zeal is evidence that he is something more than a material monad.

Let us note a few of our experiences somewhat in detail to see how the inevitable plus sign ever follows hard upon any attempt to explain them on a purely physico-chemical or behavioristic basis. Take the experience of sense perception. If any psychologists go so far as to deny anything like sense perception, or to suggest casting the very term into the discard (as we recently heard one express himself), our reply is that such denial and suggestion are contrary to all human experience, and therefore do not belong to the field of scientific psychology, but have wandered off into the mistland of speculation.

We would first analyze the perception of a physical object—say a tree—through the sense of vision. We say we see a tree out yonder in the college campus; but what is the process of our experience? Well, the waves of the ether of space, set into vibration, carry the colors and form of the tree to the eye; they enter through the iris, are refracted with proper coördination by the crystaline lens and the humors of the eye-ball in such a way as to form an image of the tree upon the retina, which may be described as the moving-picture curtain in the rear part of the eye; thence, in some real, albeit inexplicable, way, the image is borne by the optic nerve back to the cortex of the brain, where it breaks out into clear consciousness, and we say that we see a tree. Strangely enough, instead of perceiving the inverted image on the retina, we see the tree standing, right end up, out in the campus forty feet away.

Now, we can trace the purely physical process quite clearly up to the point where the sensation or stimulus reaches the cortex (although, of course, there are a thousand mysteries along the pathway); but right there, at the end of the physical process, something entirely different occurs which constitutes our dramatic plus sign—the image of the tree breaks out into our consciousness! We are aware of the existence of the tree. How is the passage from the unconscious physical process to the conscious psychical experience accomplished? What is the bridge that spans the chasm between the unconscious and the conscious? On a purely material basis there is and can be no explanation. The plus sign looms like a phantom before us, and we feel that what lies beyond it is the thing of major importance. Everybody knows that mere material substance, as we know it, has no consciousness and sentiency; yet we are conscious of the presence of the tree out yonder. When we walk out to it, we find it there—a real tree. We may test its reality by feeling its bark and by hearing the soughing of the wind through its foliage. At least three of our senses, therefore, attest its real existence to our awareness.

However, there is still more to follow. By a little attention we are aware of a good deal more than a mere tree. We note that the tree has a symmetrical bole, with comparatively smooth bark; that the branches spring up slantingly from the trunk; that the bark is dark gray; that the foliage, the season being autumn, is quite variegated in color. Now a new psychical experience comes into the field of consciousness: perceiving that the tree is beautiful, we are aware of a feeling of pleasure, and we may exclaim, "How beautiful!" Can mere physical molecular motion or muscular stimuli experience a feeling of esthetic pleasure? Surely there looms here another crucial plus sign.

But still further mental experience may occur in connection with the tree. Why is the foliage so variously tinted. In response to this question, another mental functioning power comes to the fore: we remember that there was a light frost a few nights ago which turned the green of the leaves into browns, reds, pinks, etc. Can mere material substance 'remember?' Must there not be a something after that interrogation-point that has the innate power to recall and to bring past observations and experiences into the area of awareness when the proper stimulus from without reaches it?

But the mind can go much further in its relation to the tree. It may reason and schematize, and bring into play the scientific faculty (from *facio*, to work; therefore a functioning power of the mind, and thus etymologically a good term for psychology). To what order of the natural realm does the tree belong? To the organic world. To what phylum of organic objects? To the vegetable. To what family? The trees. To what species? The maple. Can such a complex psychical process be attributed to mere material substance? Surely not. There must be a rational agent or being or entity (call it what you will), that is able to carry on a process of ratiocination such as has just been depicted.

Suppose we try out another kind of sense stimulus in connection with its accompanying psychic reaction. Sitting at your desk reading or writing, certain sounds divert your attention. They are borne on the air in waves, which strike the tympanum of your ear, pass along through the anvil and hammer, and are caught up by the auditory nerve and carried to the proper brain center; then mysteriously the result of the process breaks out into your mental consciousness, and you say, "I hear the playing of the piano." How will you explain the transition of the physical process into the mental experience? There again is an unbridged gap—unless you posit or assume a real agent, the mind, which has the innate capacity for conscious cognition.

It should be further noted that the sound-waves in connection with the piano gave a very different impression and experience from the light waves in connection with the tree. You do not think of saying, "I *hear* the tree," but, "I *see* the tree;" nor, "I *see* the music," but, "I *hear* the music." Can such psychical differentiations be assigned merely to sense impressions, molecular motion, or conditions and actions of the viscera and glands? Again, in listening to the sounds from the piano you experience much more than mere sound; you are arrested and stirred by the beauty of the music if the instrument is skillfully played; perhaps you are so affected that you cease your work at your desk and give yourself up to the enjoyment you experience in listening to the sweet sounds. Then you wonder who the artist is, and you rush downstairs to meet him, and to thank him for the rare and uplifting delight he has afforded you. All these experiences follow the plus sign which looms up after the mere physical auditory stimulus has come to you. You never think of saying, "My auditory nerve experienced a delightful feeling by becoming conscious." Somehow, explain it as you will, or leave it unexplained if you prefer, you feel that it was your *soul* that was thrilled by the rare music.

Suppose you hear a beautiful hymn or anthem with ennobling sentiment, then you experience a great deal more than the mere concatenation of sweet sounds; there is the important addendum of spiritual emotion and uplift. Could mere physical action and reaction create a spiritual emotion? In other words, can something be gotten out of nothing? Or, to put it still differently, can an effect belong to an entirely different category from its cause? If there is such an experience as a spiritual emotion—and there surely is in thousands upon thousand of cases—must there not be a spiritual agent or entity capable of having such an experience? If man is all material substance, and nothing more, why in the world and how in the world would material substance ever give rise to ethical and spiritual experiences? These are salient questions? They should be answered.

Percepts, concepts, and emotions exhibit *qualitative* differences. Men have high and low conceptions and feelings; also good and bad. Such differences are certainly hard to explain on a purely physico-chemical basis, or as the result of muscular action and reaction, or of glandular secretions, or of visceral operations. Think of trying to explain a high and holy joy or love as the sequence of the normal action of the viscera! Would not that be dragging a noble science down into the dust? If there are qualitative distinctions in men's motives, feelings and actions, there must be an entity capable of cognizing such differences. The best explanation is the admission of a rational mind or soul.

Of course, we recognize the difficulty of the materialistic monist in psychology. He asks, "What is the mind? What do you mean by such a metaphysical something?" Another question gives the best reply: "What is matter?" You believe that matter is the real thing and the only real thing. Then what is matter? No one can tell. If you say, "Matter is composed of atoms and molecules," we ask, "What are *they*?" If they are made up of electrons, what are *they*? If they are the result of vortex rings or eddies in the universal ether, the fundamental question leaps to the fore: What is the universal ether? No one knows, not the wisest chemist or physicist. Pursuing our interrogations to the *ultima thule*, we do not know what matter is in its fundamental essence any more than we know what mind is. Neither do we know what energy is, or electricity, or life. What we know is only phenomena, not noumena (things in themselves). But we know the phenomena of mind as well as the phenomena of matter. Those of mind are self-consciousness, thought, feeling, volition; those of matter are form, color, hardness, softness, solidity, liquidity, inertia. One class is as patent in human experience as the other. Indeed, our mental experiences seem to be the most intimately and immediately known, because all sense impressions must come into the consciousness in a circuitous route through material media.

The normal human mind has also the direct experience of *volition* in numberless instances. Philosophize about it as one may, such psychical facts come clearly within the range of experience. No experience makes a more palpable impact on the center of human consciousness. We choose between alternatives, we determine ourselves, and we know we do—if we know anything at all. The present writer has exhibited this obvious fact of experience in the classroom as follows:

In a few moments, he says, I am going to use my will thus: by lifting my right arm (now hanging by my side) to a horizontal position, then closing all my fingers except my index finger, and then crooking my index finger four times. Wait a moment! Ready! Now I *will* to do it! And the act is performed precisely as I predicted it would be performed. If ever I was conscious of anything in my life, I was conscious that, at a certain self-determined moment, I exerted my will, lifted my arm, and performed the rest of the experiment. Yes, the major thing of which I was conscious was the voluntary exertion of my mind. This simple act of volition and all other acts of the kind are inexplicable on a merely physical basis.

It must not be supposed from the foregoing discussion that the dualist in psychology denies, ignores or slights the physiological concomitants in mental experience. Indeed, the consistent dualist must believe in interaction, because he recognizes the reality of both the mind and the body and their marvellous and intimate integration. It is evident to the dualist that the Power that brought man into existence made the mind and the body for each other—made them to be companion-pieces—at least, during this earthly life-time. Therefore, what affects the one part of man affects the other.

A simple analysis of experience proves that the condition of the body affects the mind. If the body is in perfect health, the mental processes proceed in the normal way. On the other hand, ill health or violent bodily pain will greatly impede and disturb the action of the mind. This is a matter of common experience. But the opposite is just as patently true. To illustrate, if you have received good news just before starting to your place of business, you walk erectly and rapidly, your step is elastic and sprightly, and your whole frame and movement proclaim the exhilaration of your mental condition. If, however, you have received sad and depressing news, your body becomes stooped, and your feet drag along heavily—that is, if you do not make an effort to camouflage your feeling of dejection. For this reason, and others that we cannot take time to name, the study of physiology is very interesting for the psychologist, and furnishes an important aid in his studies and their practical application.

Science must gather enough data in the realm of reality to form an hypothesis, and then must try out that hypothesis on other facts, to see whether it will explain them adequately or not. We think we have shown the monistic hypothesis to be inadequate. The supposed causes are not sufficient to account for the grand effects. The primary fact of consciousness ever baffles the attempts of the behaviorist and the monist. So let us broaden our hypothesis to that of dualism and interaction, and try it out in the light of experience.

In the first place, this hypothesis comports with the almost universal experience of the race and with the *consensus gentium*, for nearly all people seem to feel that they have minds or souls as well as bodies. It would be a queer trick of matter, if it is the only subsistence, to cause the vast majority of people to feel and believe that they have souls when they have none. That would certainly create a fundamental suspicion that the testimony of matter is very unreliable.

In the next place, on the assumption that man has a mind, that it is constituted a self-conscious psychical entity, and that it dwells within the body as its fitting habitat—on this postulate, we hold, all man's psychical and somatic experiences can be explained. Let us try it out. We are gifted with a visualizing power as a part of our imagination. We may thus visualize the mind as a self-conscious agent ready for action in connection with the brain. Now, whenever a sense impression is carried back into the cortex of the brain through the corresponding physiological channel, there is the self-conscious mind ready to recognize it. When a physical object is seen— a tree, an animal, a man—the mind apprehends it as belonging to the sphere of vision, *i.e.* as a visible object. When a sound is carried back to the proper center through the auditory apparatus, the mind is there on the alert to cognize it as the ringing of a bell, or the music of a pipe organ, or the singing of a prima donna. The same is true of all other psychical experiences, whether they come through the senses or through rational or abstract mental processes. Every mental fact is known by the impact made by it upon the focus of the conscious self, and only in that way. This self, ever alive and alert in waking hours, may be likened to the operator in a central telephone station, who, being always on the *qui vive*, responds to every signal on the board before him, and calls, "Number?"

Should the objection be raised that we have *assumed* that the mind is so constituted as to be self-conscious and to be able to respond to various stimuli in a self-conscious way, we reply that the physicist has to assume that the ether of space is so constituted as to carry light and color on its infinitesimal undulations; the optic nerve is so constituted as to carry the image of an external object back to the cortex; the auditory apparatus is so constituted as to convey the waves of sound to the proper brain center; the nerves of taste carry back to the appropriate organ of the brain the various sensations produced by the passing of food and drink over the palate.

So reasoning from analogy and testing all data in the alembic of experience, we must conclude that the mind is so constituted by its Creator as to be conscious of itself, of perceptions, reasoning processes, moral feelings, spiritual exaltation or depression, and all the other varied facts and phenomena that come within the arcanum of its awareness.

Wittenberg College LEANDER S. KEYSER

CERTAIN IMPLICATIONS IN THE CONCEPT OF *GESTALT*

It is well-nigh impossible to present a new idea without arousing a great variety of misunderstandings, for the clothing of a new idea in words is like the putting of new wine into old bottles.[1] Nobody can be blamed for seeing the bottle before its contents. There are, however, certain intrinsic sources of error, two of which are mentioned here, that bear upon most scientific discussions.

First, the originator of an idea is forced to use concrete illustrations and he of course chooses them from fields in which it is easiest to establish his points, *i.e.* from fields in which the greatest advance has been made and in which the chance of meeting unanswerable questions is reduced to a minimum. This procedure—especially if the entire proof of the hypothesis rests upon it— is, however, likely to lead others to the conclusion that the field from which the illustrations are drawn is somehow essentially related tó the idea that is being advanced. For example, some articles by Köhler and Koffka, which were concerned with facts of shape more than any other of the early studies on perception,[2] gave some people the mistaken impression that they considered shape to be of greater importance than qualitative and quantitative properties of the visual perception.[3] Since the *Gestalt* theory considers shape, quality, intensity, and quantity as interdependent and does not apply more specifically to one than to any other, arguments against the primacy of shape (of any one of them) does not affect the central idea of the *Gestalt* theory.[4]

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[1] The writer wishes to acknowledge his indebtedness to Professor Max Wertheimer, Professor Wolfgang Köhler, and Dr. Kurt Lewin of the University of Berlin, who have, during the last five years, introduced him to the theory of *Gestalt*. He is alsó greatly indebted to Miss I. G. Probst, Miss A. Mathiesen, and Professor C. A. Ruckmick for their assistance in the preparation of this note.

[2] W. Köhler, An aspect of *Gestalt* psychology, *Ped. Sem.*, 32, 1925, 691-723; K. Koffka, Perception: An introduction to the *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 531-585.

[3] Harry Helson, The psychology of *Gestalt*, this JOURNAL, 37, 1926, 205, 213 ff.

[4] Köhler points out (*op. cit.*, 692-702) how the perception of things and consequently the perception of shape depends on the qualitative and intensive properties of the visual field.

In the second place, every new hypothesis has to be defended against other existent hypotheses.Thus in Germany *Gestalt* psychologists argued against Wundt's hypothesis of 'creative synthesis' and also against Meinong's and Benussi's 'production theory'—neither of which does justice to the importance of the objective configurational aspects of the stimuli. *Gestalt* psychologists, therefore, emphasized the importance of the objective facts. Now a reader who does not believe in the 'production theory' is easily lead to the opinion that the doctrine of the *Gestalt* implies that subjective determinants are of no importance in perception.[5] Any argument against such a position does not touch in the slightest degree the central idea of the *Gestalt* theory. Similarly, *Gestalt* psychologists argued against the negation of consciousness by behaviorists, with the unexpected result that at least one non-behavioristic psychologist was led to believe that the *Gestalt* psychologists pretended to be the only, or at least the chief, investigators into consciousness.[6]

The list of misunderstandings could be greatly extended. The only purpose of mentioning them here is to explain why for the moment—and fifteen years are only a moment in the development of science—certain misunderstandings are inevitable. I mention the two above simply because the sources of error appear very clearly in them.[7] The only outcome of such misinterpretations is to learn that the original exposition was ambiguous and that under certain unexpected conditions was even positively misleading, and consequently to try another approach to the subject from a new angle. An approach, which has not as yet been tried, consists in taking some of the actual cases in which the term *Gestalt* is used—chiefly cases where it is not used in accordance with its original meaning—and in explaining how the real concept of *Gestalt* would have applied there. An example of this method is offered in the following paragraphs.

Mogensen and English had an original and happy idea of investigating the apparent warmth of colors.[8] If the warmth of colors has something to do with real warmth, they argued, it must have some influence upon the tactual perception of warmth. They exposed pairs of electrical resistances at equal temperature and covered with various brightly colored papers. The observers, without knowing that the resistances were equally warm, touched them while looking at them and then gave a judgment as to which one of a pair felt the warmer. The number of times that every one of the colors used was judged the 'warmer' was as follows: green, 416 times; blue, 405 times; orange, 380 times; yellow, 377 times; red, 371 times; and purple, 301 times. This was an unexpected result. The experimenters, and I, too, had expected exactly the opposite outcome, since green and blue have the least apparent warmth, and red and purple the highest. The experimenters were disappointed. "Although the data are slight," they say, "the conclusion seems warranted that the apparent warmth of colors is *insufficiently intrinsic to enter into a total configuration or* Gestalt in such a way as to modify the judgments of tactual warmth."[9]

Must we really give up so soon? Let us see. We are not told explicitly what kind of influence of color-warmth on tactual-warmth the experimenters expected as an effect of the formation of a *Gestalt*. We can, nevertheless, discover what they expected from what—to their disappointment—did not happen. It did not happen that the red or purple objects that *look* warmer *felt* warmer, that the blue or green objects that *look* colder *felt* colder. It must, therefore, be supposed that they expected visual warmth to enhance tactual warmth, and visual coldness to diminish it.[10] To be more explicit: it was expected that a certain amount of visual warmth (either a positive or a negative warmth) would be added to the given amount of tactual warmth, viz., that the apparent warmth underlying the judgment of the observers would be the total of the two kinds of warmth in question. In other words it was supposed that the attributes of a whole could be found by adding the attributes of its parts.

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[5] Cf. Helson, *op. cit.*, 205 ff., esp. 213; also H. R. de Silva, An experimental investigation of the determinants of apparent visual movement, this JOURNAL, 37, 1926, 469-501; esp. the summary.

[6] Madison Bentley, The major categories of psychology, *Psychol. Rev.*, 33, 1926, 87.

[7] That I take an American instance does not imply that equally flagrant German examples could not have been selected.

[8] M. F. Mogensen and H. B. English, The apparent warmth of colors, this JOURNAL, 37, 1926, 427 f.

[9] *Op. cit.*, 428. Italics are mine.

Is that to be expected of a *Gestalt?* Not at all! Such an expectation is a direct contradiction of Von Ehrenfels' negative definition of *Gestalt*. According to Von Ehrenfels no prediction can be made from a consideration of the attributes of the separate parts regarding the attributes of the whole. In color-contrast, for example, we have the paradoxcial fact that when red is added to red, *i.e.* a red spot is put on a red background, we get not more but less red than before. In this way color-contrast shows most convincingly how *'gestalted'* units behave. Such facts, in the meaning of *Gestalt* theory, are not therefore exceptions for which specially adapted nervous apparatus must be assumed, but rather illustrations of the true nature of psychic processes.

But if *Gestalten* are not the total of the part attributes then what are we to expect? Provisionally it can only be said that, as a symptom of having entered a *Gestalt*, the total of the part-attributes most probably will not be found, for the attributes of the former parts will be altered in some manner. In order to get a positive answer to this question, we have first to see where, if not in the attributes of the parts, we should seek the new attributes of the compound. This is shown by Wertheimer's adequate interpretation of the positive definition in 1924,[11] according to which *Gestalten* are units of such a nature that what occurs in their parts is controlled by intrinsic laws of the structure or character of the actual whole. The meaning of these laws of *Gestalt* obviously implies that only a very small number of such laws can be found which are valid in, and can be applied to, all the various kinds of wholes, for what laws are effective depends wholly upon the special character of the particular whole that is actually given. In perceptual formations it generally depends, for the greater part, on the actual configuration of the external stimuli, from which it arises.[12] According to Wertheimer's definition, as is obvious, this occasioning physical configuration itself is not necessarily a *Gestalt*, as will be made clear in our further discussion.

Our first task, therefore, in the search for the attributes of the compound, is to become thoroughly acquainted with the special physical configuration in question, *i.e.* to analyze it exactly and wholly. But we are told that analysis is strictly prohibited in *Gestalt* psychology, that only unanalyzed wholes are allowed to be considered. We must admit that we never could understand how, in this way, any scientific results could be obtained. In fact analysis is prohibited; or rather, as *Gestalt* psychologists believe (and in many cases have found to be true), analysis does not help to discover the nature of a compound. But this concerns only a certain rather widespread kind of analysis, such as "separating a given whole into its elements, viz., into its constituent parts and considering these parts separately," as in the case which we were considering, separating the perceived warmth into its tactual element and its visual element and hoping to get the apparent warmth by putting these elements together. This kind of analysis led to no result. But there is another kind of analysis which we are not only *allowed* to apply, but which is wholly indispensable and, in the opinion of *Gestalt* psychologists, has never been carried far enough in older psychology. This is the minute and full description of all details of the fact in question, of all the parts, part-processes, and part-qualities of the whole, just as they look or behave at their particular place in the actual whole, and would not look or behave elsewhere, neither at another spot of the same whole nor as a part of a different whole nor as separate independent things.[13] It is not necessary to consider here the wider circumstances under which the facts observed happened, or to mention what external and internal 'factors' may have modified the result, as *e.g.* bad sticky air, or fatigue, or a headache, or an insufficiently concentrated attention of the observer. These also are important facts, but in most cases they are not so important by far as the structure or configuration, in space and time, of the specific external stimuli themselves.

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[10] As I prefer to call it, for both tactual and visual warmth are *apparent* warmths.

[11] Max Wertheimer, *Über Gestalttheorie*, 1925.

[12] Exceptions to this rule will be discussed later.

In our example this structure was as follows. There was a warm object the surface of which was homogeneously covered, as long as the object was only looked at. But at the moment when it was touched its surface was partially covered—that is to say, optically interrupted—by the hand of the observer. At the spot covered by the hand the color and, in consequence, the *visual* warmth of the surface was not perceived, but only tactual warmth; on the other hand, this area in which tactual warmth was perceived was rather distinctly limited by the size of the hand and somehow, at least in two directions, *surrounded* by the still perceivable visual warmth of the uninterruptedly visible remainder of the surface. (If the authors had applied electrical resistances exactly the size of a hand, and thus wholly covered, an analogous circumstance would have resulted in *time*, as in their experiment it resulted in *space*.) These are, so far as I can see, the essential traits of the configuration of stimuli *in each member* of the exposed pairs, the configuration being independent of the colors applied. Perhaps we can find out now what kind of *Gestalt* laws are valid in such a configuration, what modifications the part-qualities will undergo, in how far *the apparent Gestalt will deviate from a mere configuration of sensations* which would exactly correspond to the configuration of physical stimuli.[14]

Since only a relatively small number of special *Gestalt* laws have at this time been definitively formulated, we are not able in every case to predict the laws actually valid from the description of the configuration. For we may possibly encounter a type of configuration not yet explored; in this case our task would be to fathom the function which is valid as between the configuration in question and the results found. This would be a contribution to the discovery of new laws. But in the actual case we are not in such a difficult situation. Generally speaking, we have two kinds of like sensorial quality spread over a surface and clearly separated, so that the less extended of the two is, at least partially, enclosed by the other. These are just the conditions under which in all known cases, *contrast* will occur at its greatest intensity. If we consider once more the results of Mogensen and English, we are not so disappointed as they are, since the rough outcome of their experiment, which the authors themselves are inclined to ascribe to mere chance and therefore to neglect, is just what was to be expected from the configuration in which the stimuli were exposed.

No doubt, the result is very slight and there are a great many insignificant and even contrary cases. But here our task is primarily to get a finer and more extended configurational analysis. We have to consider the relative size of the covered and the visible parts of the paper. We have to consider what colors were compared in each pair and how, perhaps, the positive, negative and insignificant results are correlated with certain kinds of color-configurations within the pairs. We have to consider the consequence of exposure, for it is possible that certain preceding influenced certain following colors in a direction opposite to contrast. We have to consider that the psychical state of the observer may enter the configuration, that opposite results may originate from different inner attitudes which, as is known, are the more effective the less unambiguous the configuration of the stimuli. Perhaps our conditions are not unambiguous enough for clear results, *e.g.* when two colors are very similar to each other there is no unambiguous color-contrast but a fluctuation between contrast and assimilation. The same thing may be possible in our experiment. That is to say, we have to consider the relative rates of the visual and tactual warmths used. Finally, we have to consider the relative sensibility of our various observers for color warmth, which naturally will modify the results. Possibly we have among them a few with a very high threshold or even total insensitive to warmth.

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[13] For such 'analyses' see, *e.g.* K. Koffka on Perception, etc., *Psychol. Bull.*, 19, 1922, 540, 553 ff.; W. Köhler, An aspect of *Gestalt* psychology, *Ped. Sem.*, 32, 1925, 696 ff.

[14] A word on the meaning of the terms 'Configuration' and 'Gestalt' in German psychology. In our example, visual and tactual warmth of the papers would have been in *configuration* in any case (in such a relative situation and proportion of extent in space or time as can be stated in geometrical terms, such as being close together, one surrounded by the other, and the like). But visual and tactual warmth are in a *Gestalt* only if a greater or lesser mutual modification is possible. This mutual modification that results in a deviation from the 'total' is just the decisive criterion between mere configuration and the union into a real psychical *Gestalt* —as Mogensen and English did correctly suppose but did not adequately apply to their experimental conditions.

Starting from our assumptions, and with a refined and extended analysis, our second task is to improve the conditions, to modify them either in such a way that contrast will certainly be strengthened or, on the other hand, just as certainly will be converted into its opposite, the assimilation or mixture. In order to strengthen contrast, according to well-known rules, we have to make the covered spot smaller and more neutral in warmth, the surrounding surface larger and, if possible more brightly colored. In order to obtain assimilation or mixture (the result expected by our authors), we have to avoid interruption of a larger surface of the one kind by a smaller of the other kind; we have even wholly to avoid the possibility that the two kinds of stimulation are so situated that one is beside the other with a common boundary. Perhaps it will be suitable to create a kind of 'atmosphere' in which both kinds of warmth simultaneously contribute their share, *e.g.* to let the observers enter a number of small rooms which have the same temperature but are warmer or colder by the color of their walls or of their illumination. Perhaps it will even be possible to adjust the temperature of the rooms so that they will have the same apparent warmth, in spite of different temperatures.

Only by such an improvement of the experimental conditions will it be possible to prove whether the visual and tactual 'warmths' are merely configured or enter into a real *Gestalt.*

SUMMARY

I have attempted to show that an inadequate use of the concept of *Gestalt* has led to false expectations concerning the outcome of experiment; that an adequate use of the concept would have promoted a contrary expectation; that such a contrary result was obtained but was neglected by the experimenters in consequence of the false expectations; and, finally, that certain modifications of the experiments are suggested by the concept in order to obtain clearer and more effective results.

University of Iowa WOLFGANG METZGER

EMPIRICAL PSYCHOLOGY

Carmichael[1] and Boring[2] have recently pointed out the implications of the term *empirical psychology*. Carmichael has differentiated three meanings and urges the use of the term in the sense of experimental. Boring objects to this synonymous usage of the two terms and prefers to substitute 'empirical' for the more awkward term 'experiential.' It follows that experience and experiment for him are not equivalent terms. With this general position I should agree although I can not literally admit his statement that empirical psychology is necessarily "something broader than 'experimental psychology,' something which includes experimental psychology, for to my mind certain types of empirical psychology would not necessarily be experimental. Thus much that has been written by Freud and his followers might very well be empirical psychology but for the most part not experimental; and certainly much of the intuitive psychology of the present day as we find it in Roffenstein,[3] Jaspers,[4] Pfänder,[5] and Scheler[6] belongs to the same category. In my classificatory scheme I have preferred to take the historical position,[7] opposing rational and empirical as coördinate and subdividing empirical psychology into theoretical and experimental.[8] Of course, as Carmichael has pointed out, this may not be strictly speaking the historical point of view since Wolff made the basis of his distinction between rational and empirical the essential difference between the mental faculties involved in the discussion of these divisions. When their underlying scientific principles were derived from rational faculties they were known as rational; when they came from experience they were designated as empirical.[9]

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[1] L. Carmichael, What is empirical psychology? this JOURNAL, 37, 1926, 521-527.

[2] E. G. Boring, Empirical psychology, this JOURNAL, 38, 1927, 475-477.

While classifications indicate simply shifts of emphases where the stress in one case may be more in the direction of one category and less in another, I think in the main the various psychological approaches can still be put under their classificatory rubrics. A purely rational type of psychology worked out by the *a priori* method of reasoning in which concepts were analyzed into their fundamental meanings has very much disappeared. This was largely the psychology underlying the writings of Descartes, Leibnitz, and Kant. With a shift towards the actual analysis of experience, whether on theoretical grounds or mainly on the basis of theoretical consciousness or whether on grounds of strict experimental procedure, empirical psychology has developed and survives today. Empirical psychology in other words, to my mind, means any form of psychology whether theoretical or experimental, or both, which collects its data from experience, and which draws its conclusions only after these data have been collected and examined for points of similarity and difference *in their own*, not in their *conceptual*, nature.[10]

In experimental psychology the procedure may involve theoretical approaches in general, as when experiments are performed by certain schools of psychological thought, or it may develop out of a tentative hypothesis. In these respects there is, of course, involved a certain amount of deductive reasoning. But from this point on the procedure is characteristically scientific in that logical thinking of this major deductive or speculative sort is suspended until sufficient data have been assembled and the hypothesis is reëxamined in the light of the results of experimentation. The history of psychology has shown that in many cases opinions have been reversed, if not radically at first but certainly in time, as the controlled results from laboratory experimentation have either failed to corroborate the original thesis or have accumulated increasing amounts of data on the opposite side. Anyone who has studied carefully the intricate process of thinking knows, of course, again the fallacy of the arbitrary separation involved in the conceptions of inductive and deductive logic. Pillsbury has admirably shown how impossible it is to follow exclusively one or the other procedure. We are obliged to characterize by the average or predominant process knowing how artificial, in general, classification is.

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[3] G. Roffenstein, *Problem des psychologischen Verstehens*, 1926.

[4] K. Jaspers, *Die Psychologie der Weltanschauungen*, 1920.

[5] A. Pfänder, *Einführung in die Psychologie*, 2nd. ed., 1920.

[6] M. F. Scheler, *Die Transzendentale und Psychologische Methode*, 2nd. ed., 1922.

[7] C. A. Ruckmick, A schematic classification of general psychology, *Psychol. Rev.*, 33, 1926, 398.

[8] F. Thilly, *The History of Philosophy*, 1914, p. 380.

[9] Klemm states that Wolff's rational psychology was built around the power of representation as a fundamental power of the mind whereas the empirical aspect of psychology depended upon the faculty of sense-cognition. O. Klemm, *The History of Psychology*, 1914, 59.

Cf. also W. Wundt, *Grundzüge der physiologischen Psychologie*, 6th ed., I, 1908, 6 ff.; *Grundriss der Psychologie*, 8th ed., 1901, 20 ff. Wundt shows very clearly why it is impossible to separate in practice *rational* or *metaphysical* psychology from empirical. Even the most orthodox "arm-chair" metaphysical psychology will be concerned primarily with "experience!" But the procedure is for the most part different, and, of course, the objective is distinctly different. He gives a schema in the *Grundriss* which distinguishes, under rubrics of the first order, *metaphysical* and *empirical* psychology and then, under *empirical* psychology, the psychology of the "inner sense" or pure self-observation, and psychology as the science of immediate experience of which there are two branches, the psychology of peoples and experimental psychology.

[10] In this historical classification it is a puzzle where to put so-called extreme behavioristic forms of psychology. Physics can be called empirical when it obtains its testimony in the last analysis through the senses, *i.e.* as when voltmeters are read and electron deposits are noted through vision. But behaviorism in this form, of course, at once uses experience to gather in the testimony but denies it as material for science. Experience in this sense is not, therefore, the fundamental datum for this form of psychology!

Now there are so-called psychologists, of course, who elaborate upon a theory of consciousness, of mind-body relationship, or of emotion, who do not resort to experimental methods to substantiate their hypotheses. Even an experimental psychologist may write a theoretical treatise that could be classified under the category of theoretical psychology without quoting experimental data to substantiate his theory. Experimental psychologists furthermore at some time or other must resort to theories or explanations of data obtained in the laboratory. So, also, when any theoretical question arises the proof or disproof of it must be based ultimately for experimental psychology on actual investigation in the laboratory.

I think, therefore, that there is still considerable utility for a clear-cut distinction between the terms empirical and experimental and I should be the last one to agree with Carmichael's conclusion, although I think his analysis of the historical use of the term is admirable in many respects. I suppose the reason for the attempt to make the terms synonymous lies in the fact that today there is less call for a distinction between rational and empirical psychology and, turning the logic about, that most scientific psychology today since it is experimental is also empirical. But to any student of general movements especially in Germany in the present day, one must not overlook the fact that much systematic psychology is empirical in the sense that it is based upon actual human experience even to the point of its phenomenological origin, but that it does not always take pains to corroborate its own position with experiments done in the laboratory. Whether we assimilate *Gestalt* psychology or not we must admit that this new movement, for one, has not only proceeded to prove its claims through experimentation, but has provoked experimental procedure in new directions and has thus stimulated the experimental method throughout the whole field of psychology.

University of Iowa CHRISTIAN A. RUCKMICK

A DEMONSTRATION METHOD OF STUDYING THE LEARNING PROCESS

In order to demonstrate to classes in elementary educational psychology, some of the many objective and subjective phases of learning, I have developed a series of 450 problems involving multiplication of two-place numbers by two-place numbers. In this series of problems all zeros and ones were omitted, and all problems whose answers involved fewer than four digits or which were missed more frequently than the usual number of times, were removed from the series. The remaining problems are approximately equal in difficulty since the records of performance of students show the same relative results when the students begin with any series or with any problem in a series. A tabulation of the problems missed shows that no problem was missed much more frequently than another when the factor of position near the end of the daily task was accounted for. The series of problems was divided into 15 tasks of 30 problems each. A series can be given each morning during the first 6 min. of the class period without disrupting the work of the period to any considerable extent.

The students may work uninterruptedly for the entire 6 min., or the instructor may call out the minutes as they pass and require the students at the end of every minute to draw a line under the last completed answer. This series of problems has proved very satisfactory in demonstrating many of the phases of learning to the students. The introspective observations of the students on their own processes and methods of work prove to be of as great interest to them as do the scores which they make from day to day. The problems were mimeographed on suitable paper, two columns of 30 problems each to a page, double spaced, and with margins on either side of the columns. These pages were then cut and trimmed into sheets 4½ x 11 in., each bearing a daily assignment.

The weaknesses of the procedure were (1) the tendency of students to practice on similar problems outside of the learning period in spite of requests that they refrain from this; and (2) the devising of a satisfactory scheme for equitable scoring in terms of effort and accuracy. In using this series of problems I returned the scored problems of the preceding day to the students in order that they might inform themselves of their scores, their types of errors, and the correlations between their introspections and the results of the work done in connection with them. These sheets, however, were laid aside when the multiplication for the day was begun.

When I first began to use this series of problems, I considered scoring only the entire answer to every problem as right or wrong. This made an index that was too coarse to show well the gains and losses in accomplishment. As a result I scored every digit of every answer—the score for the day being the number of correct digits in the answers obtained. It is obvious, however, that the processes involved in finding the different digits are of very different degrees of difficulty. I am therefore now using a system of weights to equalize the errors that occur within the different digits. I indicate an error, beside the answer in which the error occurs, in the thousand column by 'a,' in the hundreds column by 'b,' in the tens column by 'c,' and in the units column by 'd.' These symbols can be counted quickly and the number of each multiplied by a constant that was empirically determined.

These constants were found by tabulating 6,866 errors according to their places in each of the four columns (units, tens, etc.). These numbers were then reduced to percentages of the entire number of errors, and the reciprocal of each was found and designated as the constant for the errors in that column of the answers. The number of errors by columns was: thousands, 1171; hundreds, 3386; tens, 2125; units, 184. The constant or weight that was used for the errors in the column of thousands was −0.8; of hundreds, −0.5; of tens, −0.7; and of units, −1.0.

The amount of work involved in scoring a number of papers by this method is slightly greater than that involved in the simpler methods of scoring each incorrect digit in the answers as −1, but it gives scores that seem to be more nearly in accord with the amount of effort and attempted accuracy involved. The virtue of the method lies in the discussions that arise from comparisons of scores by the students and the discussions of working methods when they receive scores from the two methods of scoring mentioned above.

Whitman College H. R. LASLETT

THE WITTENBERG SYMPOSIUM ON FEELING AND EMOTION

A symposium on feeling and emotion was held at Wittenberg College of Springfield, Ohio, from October 19–22, in connection with the dedication of a new psychological laboratory. The papers read at this meeting undoubtedly represented the greatest assemblage of psychological authority ever brought together in this country, for there were papers by most of the leading psychologists of both Europe and America.

Since the papers, 15 European and 18 American, are soon to be published in book form it is needless to attempt a detailed summary of every one. Moreover, there seemed to be a fairly general belief that we shall have to await the critical study of the papers to gauge at all accurately their true import. It was a very heavy program presented to packed houses. Anything like a comparative evaluation was rendered still more difficult by the fact that the papers on emotion, especially, were the logical development of *a priori* assumptions and empirical facts. Until the basic assumptions are brought into harmony or recognized as irreconcilable any formulation of a general nature will be futile.

It is always in order, apparently, to bewail the lack of genuine experimental work in this field. Here the physiologists hold a distinct advantage. One wonders, to cite only a few cases, whether any type of report would reveal the "objects, occurrences, and relations" of Dunlap, the teleological "tendencies" of McDougall, or the "energies" of Morton Prince. The fact is that here as elsewhere there is a wide divergence between postulates with no evident principle which will bring them together. Even an external teleological factor, such as Carr proposes, will scarcely add to our understanding of the emotions as such.

This leads to the functional consideration of the emotions as presented by Pillsbury, McDougall, Prince, Dunlap, Claparède, and Howard. This does not imply that the treatment in other papers was not functional in intent. But who can say whether the emotions help or hinder in the economy of life; each view had its advocates. Such disagreement does not augur well for the direction and control of the emotions; it again savors strongly of the reflection of *a priori* principles.

There was one fairly well marked trend that should prove wholesome. Several papers voiced the opinion that the feelings and emotions must not be considered as special discrete moments, *i.e.* specific entities, in our lives but rather as a current in the total stream, which at times, to be sure, makes up the total but which more often runs along with other processes. Viewed from this angle mind in its functional character does not present clear cut cleavages; it is rather a matter of organization, of focus and background, of wholes and part wholes. We find that experience at every moment presents a total or pattern which, often at least, betrays a greater or less resemblance to the affective or emotional aspects. If these aspects become too strong, as Washburn found, the thought processes lapse.

Hence in the future we may expect to find greater emphasis placed upon mind as a unity of experience, externally and internally aroused, with varying degrees of different aspects differently patterned. This appears hopeful. But as stated above, the real worth of the synposium can be realized only through a critical study of the papers in published form.

Cornell University L. B. HOISINGTON

THE WASHBURN *FESTSCHRIFT*

The thirty-ninth volume of the JOURNAL, published as a single number in December, 1927, was dedicated to Margaret Floy Washburn in commemoration of her completion of 33 years of distinguished service to psychology, including 25 years as professor of psychology at Vassar College, and as a member of the editorial board of this JOURNAL.

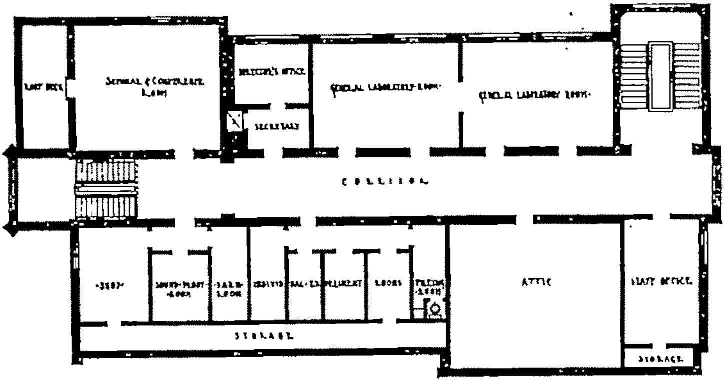
The Volume contains 32 studies in various departments of psychology, which were contributed by Professor Washburn's colleagues and former students.

Orders for the Washburn Commemorative Volume should be sent to D. R. Knight, 43 Morrill Hall, Cornell University, Ithaca, N. Y.

K. M. D.

The New Psychological Laboratory of Wittenberg College

A new psychological laboratory has just been completed at Wittenberg College, Springfield, Ohio. It is located on the third floor of a new building that is shared by the departments of chemistry and psychology. It consists, as is shown in the accompanying floor plan, of a specially constructed sound-proof room, of a dark-room, a shop for the lighter work—a modernly equipped machine shop is located in the basement and is shared with the department of chemistry—of four small rooms for individual research, of two large rooms designed especially for introductory classes in experimental psychology, of offices for the director and for the staff, and of filing rooms for departmental records. All the rooms are modernly equipped and all the floors are covered with heavy battleship linoleum.



Of the rooms on the lower floors of the building, a large auditorium with an accompanying demonstrational laboratory, several class rooms, and a room for a departmental library have been allotted psychology.

Wittenberg College MARTIN L. REYMERT

Editorial Departments

With this number of the JOURNAL a new department, Notes and Discussions, is begun—an expansion of the old department that appeared under the title Notes. In this department the JOURNAL will print all discussions of articles, replies to them, criticism of them, or controversy about them. It will also print here other long informatory notes and discussions, and all material of interest to psychologists, and without regard to length, that does not properly belong elsewhere. Naturally the short communications will follow the longer ones.

Recently the JOURNAL began a separate department under the title Apparatus. The JOURNAL welcomes for this department both long descriptions and short notes that describe new apparatus or improvements upon old apparatus, when there is reason to believe that new construction is of general scientific interest.

The JOURNAL will thus have five separate departments: original Articles; Minor Communications of the nature of the Minor Studies which have long been familiar to its readers; descriptions of Apparatus; Book Reviews; and Notes and Discussions. Original articles will be printed in large type (10-point) or in mixed type sizes (10- and 8-point), according to appropriateness and length. The other four departments will be printed entirely in the smaller type (8-point). The JOURNAL solicits for this policy the coöperation of its contributors, who, although sometimes preferring the larger type for their communications, will readily realize that there is at the present time great pressure for space upon all the psychological journals, and that these rules enable this JOURNAL to serve the interests of psychology very much more extensively than would otherwise be the case.

E. G. B.

THE DATING AND PUBLICATION OF MANUSCRIPTS

Beginning with this number of the JOURNAL, the date of the acceptance of an article for publication will be given as the first footnote. Manuscripts will be dated as they are received, and divided into two classes: long articles, extending from 25–50 pages, and short articles. Within each class publication will follow in the order of receipt, in so far as such procedure is consistent with a balanced number. In every number of the JOURNAL there will appear 2 or 3 long articles and from 5–10 short articles.

Articles accepted for publication in the Apparatus and the Notes and Discussions departments, since they are usually short and timely, will be run at the earliest possible date.

K. M. D.

ERRATUM

While working upon apparent movement in our laboratory, one of my students discovered a discrepancy between his results and my statement of Cermak's fourth law in this JOURNAL, 36, 1925, 512: "the greater the time of exposure, just so much the greater must the pause be between the phases if any kind of movement or any degree of fusion is to be seen." Referring back to the original (*Psychol. Forsch.*, 1, 1921, 106), we find it plainly stated that "je grösser die Expositionszeit, um so kürzer muss die Pause sein" which is in agreement with our laboratory findings and shows that the fourth law should read: "the greater the time of exposure, just so much *smaller* or *shorter* (*kürzer*) must the pause be between the phases if any kind of movement or any degree of fusion is to be seen." Read, therefore, *smaller* for *greater* in line 2 from the top of page 512 in the place cited above. The error in my statement of Cermak's fourth law was simply a translator's error, but important for anyone working on apparent movement—all of which goes to show that the student cannot dispense wholly with a knowledge of foreign languages and must be prepared to consult sources no matter how plentiful translations and commentators may be.

University of Kansas HARRY HELSON

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THE RÔLE OF CONSONANCE AND DISSONANCE IN MUSIC

By Martha Guernsey, University of Michigan

I. Introduction

This study considers the aesthetic enigma of the rapid development of the so-called 'modern' music and its apparent discrepancy with harmonic theory and scientific inquiry. Assuming that music is a compound of three fundamental variables, rhythm, melody, and harmony, it is obvious that each in itself is a changeable factor. The slightest acquaintance with musical history or even with the musical expression of different races is sufficient illustration of this fact. Yet, with the exception of one or two investigators, the conclusions from experimental acoustics, particularly in psychology, assume a more or less rigid entity, and display comparative disdain for the ethnological background of aesthetics in general, and the evolution of musical form in particular. The argument is commonly advanced that such factors are irrelevant to pure research. But is it not legitimate, scientifically as well as aesthetically, to question such conclusions as Seashore's,[1] that musical children and adults have an innate ear-mindedness which recognizes immediately and without training the 'consonance' of a major fifth—when recent research shows entire races in other geographic localities which react with equal spontaneity to the 'consonance' of a diminished third? It may be a well-demonstrated fact that a musical cross-section of a given population, subjected to standardized tests, shows the traditional diminishing consonance preferences of the octave, fifth, sixth, fourth, and so on, but it is equally demonstrable that many Upper Mongolian nomads will have nothing of such lordly intervals, and when they sing in harmonies at all, revert to a monoton-

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*Accepted for publication April 11, 1927.

[1] C. E. Seashore, *The Psychology of Musical Talent*, 1919, 144.

ous chant in a true augmented second.[2] Certain tribes of the Maya Indians in southern Yucatan 'howl' their part songs both in major fifths and major sevenths.[3] It is not the purpose here to digress into the relative aesthetics of primitive and European music, but the example does serve to anticipate the enigma of previous reference, namely, is the composer of modern music a freak of perverted tastes? Is the audience at a Stravinsky program having its psychological, or—to anticipate the Helmholtz theory—its physiological apparatus violated by the amazing sequences of seconds and sevenths perpetrated under the sacred name of Symphony? Or—to isolate the strictly scientific aspect from its practical context—what is the proper relation of consonance and dissonance in music? Is dissonance in and of itself legitimate? Or—further to limit the inqury, what is dissonance, if it is, and what is consonance, if it is? Is the psychological distinction, if there is a psychological distinction, a matter of innate 'ear-mindedness' as implied by Seashore,[4] a matter of physiology as contended by Helmholtz,[5] or is it purely an associational bias actively acquired, as described by Moore?[6]

Any psychology of music or of sound, in order to be at all functionally adequate, must provide at least conditional hypotheses in answer to these ordinary questions. Furthermore, any psychology of music, to be legitimate, must be capable of molding its experimental data with the observable facts of musical development.

Without getting entangled in the devious and dubious channels of controversy regarding the nature of music as an acoustic art or a physical science, it seems at least safe to define it as a form of behavior-response to aural stimuli which are selectively harnessed into a definite form, this form representing at any given time what is aesthetically pleasing. The probable origin of the modern connotation of 'music' is from the Greek word *ἁρμονία* (harmony), and is defined as a name given to the art of arranging sounds for aesthetic impression. The significance of this derivation will occur later. It takes but a glance at musical literature to ascertain that the basis of musical form, as we know it, is consonance. This concept, consonance, in turn, is ambiguous and controversial, whose proper analysis would seem the function of psychology alone. As Max Meyer has phrased it, "neither the physicist nor the physiologist can prove by physical or physiological laws why we must enjoy certain combinations of tones."[7] It may be added that neither the physicist nor the physiologist can explain the historical shift in concord, nor certain characteristic racial prejudices, nor the affective appeal of a Ravel or a Schoenberg composition to some people. It may likewise be added that no psychologist has adequately performed that service either.

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[2] Ida Pfeiffer, *First Voyage*, 1861, 119.

[3] W. H. Prescott, *History of the Conquest of Mexico*, 1878, 92-105.

[4] C. E. Seashore, *op. cit.*, 139.

[5] H. L. von Helmholtz, *On the Sensations of Tone*, Ellis transl., 1912, 179-197.

[6] H. T. Moore, The genetic aspect of consonance and dissonance, *Psychol. Monog.*, 17, 1914, no. 73, 1-68.

The present study is therefore an attempt to isolate certain aspects of the single musical factor of consonance and to correlate them, if possible, with the dissonance tendencies in modern musical composition.

In spite of the fact that harmony texts are founded on 'consonance,' that musical form has been harnessed in it, that psychological, physiological, and physical treatises assume it, there is nowhere available a uniform conception of the term. Thus is occasioned a preliminary obstacle to experimental approach and experimental technique. For if the ideas of an entity itself are contradictory, the conceptions if its attributes, in fact the very criteria which gauge its existence are perforce contradictory and ambiguous. The following summary of definitions, sufficiently augmented by criticism to make their import intelligible, serves well as a measure of this quandary. In general, two contentions may be *a priori* granted; first, no one denies the relationship apparent between the simplicity of numerical ratio and the simplicity of sensation in a two-tone clang; second, with the exception of the early Greeks, pleasantness and unpleasantness as the sole criteria of consonance, have been definitely outlawed. However, it is highly significant to notice the frequency with which the latter idea occurs in modified garb as circumstantial evidence for consonance.

Descartes, Leibnitz, Euler, and Schopenhauer are ordinarily classified together as the chief exponents of the purely Pythagorean mathematical basis of harmony. Leibnitz amplifies the theory with a convenient hypothesis of "unconscious calculation,"[8] and Euler includes the affective element in consonance as a function of the ease of perception.[9] Schopenhauer's analysis is a metaphysical transcription of arithmetic.[10] These theories, as has been frequently pointed out, state a factual phenomenon, but offer absolutely no explanation of it whatever. A most obvious refutation lies in the simple experiment of slightly mistuning these perfect intervals, thereby rendering their mathematical ratios exceedingly complex, but effecting no appreciable change in their consonant value.

The four outstanding historical figures in divergent theory, as well as in experimental mass, are Helmholtz, Lipps, Krueger, and Stumpf.

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[7] Max Meyer, Contributions to a psychological theory of music, *Univ. Mo. Studies*, 1, 1901, 1.

[8] G. W. Leibnitz, Principes de la nature et de la grace, *Leibnitii Epistolae ad Diversos*, Epistola 154.

[9] L. Euler, *Tentamen Novae Theoriae musicae excertissimis Harmoniae Principiis dilucide expositae auctore*, 1739, Cap. II, III.

[10] A. Schopenhauer, The world as will and idea, *The Metaphysics of Music*, iii, 235.

The Helmoltz theory is an evolutionary one of three stages,[11] developing from an early associatory basis into a detailed analysis of overtone and difference tone relationships. Its context is too well known to demand other elaboration than the familiar summary, "consonance is a continuous, dissonance an intermittent sensation of tone,"[12] the latter comparable in sensory effect, according to his opinion, to the visual unpleasantness of flicker. Dissonant intervals always involve the roughness of beats. These beats may accrue from the primaries, as in the minor second, or from their upper partials, as in the case of the fourth partial, e of c′ beating with the fourth partial, f of f′; or from a partial of one tone with the other fundamental, as in the third partial, b of e′ beating with the other primary in the major third, C′-e′. The third aspect of the Helmholtz theory concerns the effects of difference and summation tones, not only those between the fundamentals, but also those resulting from harmonic combination tones. Without digressing too far into the intricacies of his aural theory, the possibility of perceptible beats between the difference tones of the higher partials is somewhat difficult to conjecture. Assuming that the difference tone is generated internally in the first place with its only resonator comprising the appendages of the middle and outer ear, it is pertinent to ask exactly how the difference tones of the high partials are generated, whether they meet the same resonator device, and above all, whether their intensity is actually sufficient for perceptual effect. There is voluminous literature at variance with the beat theory of dissonance, but the only two universal objections are as follows:

(1) That beats may occur without dissonance and vice versa. For instance, a pure tone clang of 600 and 900 vibrations produces 300 beats between its primaries, which are well above the limen of beat perceptibility. Theoretically, therefore, this clang should be consonant, yet it produces the effect of a very marked dissonance.

(2) Monaural perception of dissonant primaries, or the condition of diplacusis will eliminate beat interference but will not effect a consonance metamorphosis.[13] In passing, it is pertinent to add that Peterson[14] regards this criticism of Helmholtz as invalid, in that these beatless discords may be due to habituation.

Regardless of the controversy, the Helmholtz definition is apparent. He attributes to consonance a physiological, *i.e.* a sensorial basis, with smoothness (identity of overtones and lack of beats among partials and combination tones) as its foundation. (Apropos of affective inference, we find this statement repeatedly occurring, "The cause of the *unpleasantness* of dissonance is attributable to this roughness and entanglement.")[15]

Krueger,[16] though apparently disdaining the detailed accounts of his predecessor, really has adopted and modified the third phase of the Helmholtz theory, by shifting the major responsibility for dissonance from the overtones to the difference tones. Thus, even if two primaries are of sufficient distance to prevent direct interference, the difference tones generate beats, with their attendant blurring and roughness. For instance, there are at least three difference tones of 100 vd. each, generated by the interval, 200-300 vd. These three difference tones, in reality, are identified with each other, or with what Krueger calls the "characteristic" difference tone, and the interval is thereby consonant. By slightly mistuning the interval to 200-307 vd. the difference tones change to 107, 93, 14, 79, and so forth. These tones lie in close proximity, and hence generate beats, effect roughness, and consequently abolish the consonant nature of the tuned clang. This theory obviously does not account for the dissonance of such intervals as 11:8, with its difference tones 3, 5, 2, 1, 1. As Stumpf points out, a fifth is in this instance the minimal interval, and hence should produce no beats. The second criticism of this theory is that it, too, fails to explain the beatless dissonance of diplacusis.

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[11] Helmholtz, *op. cit.*, 225.

[12] *Ibid.*, 226.

[13] The author is as present investigating the possibility of bone conduction of beats with monaural primaries of high intensity. It is possible that this wide-spread assumption may need modification.

[14] J. Peterson, A functional view of consonance, *Psychol. Rev.*, 3, 1925, 17-33.

[15] Helmholtz, *op. cit.*, 226.

[16] Felix Krueger, Die Theorie der Konsonanz, *Psychol. Studien*, 1, 1906, 305-387.

Theodore Lipps[17] augments the Euler hypothesis of simple ratios and simple physical rhythms with the metaphysical assumption of an "unconscious rhythm." For example, an interval of 300-200 vd. stimulates two correspondingly separate unconscious experiences, but these latter may combine into one conscious experience. If the ratio be simple, as 2:1, the fused conscious effect will have the 'pleasant' nature of a consonance; or, in neurological terms, if two frequencies have the ratio 2:1, every second nervous excitation coincides, but with a ratio of 8:9, there is non-rhythmic excitation. This rhythm is, per se, unconscious, but Lipps would endow it with the same psychological function as a perfectly objective dance-rhythm. The obvious criticism of such theory is the same one applicable to the Freudian conception of art in general, to wit, the assumption of an "unconscious" process with a consciously perceiving function, regardless of whether such process is called a "micro-psychic" or a "libido."

Next to Helmholtz, Stumpf[18] has contributed more definite experimental data toward the problem then any other single investigator. Taking a new departure, he restricts the meaning of consonance to *fusion*, defining fusion in turn as "that relation of two sensation-contacts in which they form a sum but not a whole."[19] In his opinion the criteria of consonance must not only be conscious entities, but objective tonal attributes as well. Though he himself denies it, the very technique of his experiments synonymises fusion with the unanalyzability of the sensation. Low degrees of fusion mean high degrees of analysis, and vice versa. The objective factors in harmonic experience, commonly known as the Stumpf laws, are an important technical contribution, and will be later referred to in connection with the problems of experimental control. It may be said in passing, however, that as a study in tonal fusion, his experiments suffice admirably; as an explanation of consonance, they fail. They reassert a sharp line of demarcation between consonance and dissonance, and attribute to each an elemental and inviolable simplicity of sensory nature. They can in no way account for the harmonic shifts of the last century, nor for the anomalous aesthetic principles of post-Wagnerian music.

In addition to Wundt, for whom consonance is merely a tonal synthesis effected by the 'apperceptive' faculty of the mind, these four investigators, though widely divergent in view, comprise what we may call, by analogy to contemporary religion, the 'consonance-fundamentalists.' Each has his disciples and dissenters, Tartini[20] and Pryor[21] following chiefly the Helmholtz tenets; Faist,[22] Meinong and Witasek, [23] and Pear[24] differing only in degree from the original Stumpf. Authors of psychological texts and experimental manuals tend similarly to classify their discussion material after one or the other of these principal schools. Titchener[25] uses the terms consonance and fusion more or less synonymously; Sanford[26] accepts smoothness and the absence of beats as the basic criteria. Pillsbury[27] states that consonance depends partly on the degree of fusion, but more upon the training of the individual. Both he and Ogden[28] would add to the purely cognitive and physical combinations the factor of feeling tone. The physicist Barton[29] emphatically substantiates Helmholtz, reserving however, that the degree of dissonance or roughness is dependent not wholly on the interval, but to a marked extent on the clang or quality of each of the component tones.

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[17] Theodore Lipps, Das Wesen der musikalischen Harmonie und Disharmonie, *Psychologische Studien*, Heidelberg, 1885.

[18] Karl Stumpf, Konsonanz und Dissonanz, *Beiträge z. Ak. u. Musikwiss.*, 1, 1898, 91-107.

[19] Stumpf, *Tonpsychologie*, ii, 1883, 127 ff.

[20] Tartini, *Tratto di Musica*, 1754.

[21] Wilhelm Preyer, Zur Theorie der Konsonanz, *Akusische Untersuchungen*, 1899, 44.

[22] A. Faist, Versuche über Tonverschmelzung, *Zsch. f. Psych.*, 15, 1897, 189-205.

[23] A. Meinong and S. Witasek, Zur experimentellen Bestimmung der Tonverschmelzungsgrad, *Zsch. f. Psychol.*, 15, 1897, 189-205.

[24] T. H. Pear, Differences between major and minor chords, *Brit. J. Psychol.*, 4, 1911, 56ff.

[25] E. B. Titchener, *Experimental Psychology: Qualitative, Instructor's Manual*, 1907, 329.

Malmberg,[30] on whose experimental data Seashore directly bases his own conclusions, practically if not theoretically, allies himself with the 'fundamentalist' school. His criteria for consonance are four-fold—blending, smoothness, fusion, and purity; for dissonance, they are disagreement, roughness, disparateness, and richness. His definition is as follows: "When the two tones of a two-clang tend to blend or fuse and produce a relatively smooth and pure resultant, they are said to be consonant. Dissonance is the reciprocal of this."[31] Both Malmberg and Seashore conceive the nature of consonance as an elemental sensory capacity differing innately in individuals and have proceeded on this basis to standardize and commercialize tests for determining its presence and degree. To quote Seashore directly, "The ability to judge the quality of two-clang as in consonance is now the most general test of sensory capacity for musical intellect."[32] And again he refers to it as a "natural sense" independent of the vagaries of feeling, and dependent primarily upon pitch discrimination. For practical purposes, in detecting "ear-mindedness" (*for diatonic intervals!*) this theory may suffice. But in its turn, it is too rigid to explain either the actual historical and aesthetic aspects of consonance, or the chromatic tendencies in the 'new music.' Malmberg emphatically states that the perception of consonance is a "purely sensory and cognitive" function; that affection, being of high variability, is definitely subsidiary to all other criteria in the aesthetic judgment as a whole. Seashore however, comprises with the statement that consonance is *ultimately* feeling and that we must not minimize its affective rôle in the 'enjoyment' of harmony and the 'jarring' of dissonance. It is somewhat difficult to reconcile these varying ideas.

Though evading a definite personal opinion, Külpe's ideas are of interest because he considers consonance in its practical relation to music. For him consonance and dissonance, and their musical counterparts, harmony and disharmony, are perceptual aspects of simple fusion. Fusion, in turn, is a function primarily of the relative *intensity* of the clang primaries in facilitating or in hindering the analysis of the tonal perception. 'It cannot be denied," Külpe says, "that the principle affords a real theory of musical harmonics and a theory which covers a large proportion of the facts. Yet in certain respects it remains inadequate."[33]

As opposed to these pure and hybrid 'fundamentalists' we have on the other hand, a group of pure and hybrid 'associationists.'

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[26] E. C. Sanford, *Experimental Psychology*, 1908, 78 f.

[27] W. B. Pillsbury, *Fundamentals of Psychology*, 1922, 147 f.

[28] R. M. Ogden, A contribution to the theory of tonal consonance, *Psychol. Bull.*, 6, 1909, 297-303.

[29] E. H. Barton, *Textbook of Sound*, 1922, 472-484.

[30] C. F. Malmberg, Perception of consonance and dissonance, *Psychol. Monog.*, 25, 1918, 93-133.

[31] Malmberg, *op. cit.*, 108.

[32] C. E. Seashore and G. H. Mount, Correlation of factors in musical talent and training, *Psychol. Monog.*, 25, 1918, 83.

[33] Oswald Külpe, *Outlines of Psychology*, tr. 1901, 306.

Max Meyer definitely differentiates harmony from consonance.[34] He insists that harmony is as much a function of successive as it is of simultaneous tones, but that consonance is a unique by-product of the latter. He offers explanation no further than to say, "All we know is that the degree of consonance depends in some manner upon the simplicity of numerical relations, similarly as melodic relationship does. In duads, we observe a high degree of consonance where we find a close relationship, a low degree of consonance where we find a remote relationship or no relationship at all. In triads, tetrads, and more complex chords there is no such simple relation between the consonance and the relationship."[35] Meyer would outlaw the term "dissonance" as having no scientific value, and speak merely of lesser degrees of consonance. Later experimental studies, indicating personal habituation as a highly pertinent factor in judgment of consonant intervals, lead him eventually to conclude that it is purely a psychological matter as to why we enjoy certain combinations of tones.[36]

Somewhat in advance of his time, Watt assumes the simultaneous vibration of the entire basilar membrane and develops accordingly a volume-theory of consonance, which he seems to define as fusion. It is physically, if not psychologically demonstrable, according to Watt, that "the volumes of all tones that have the ordinary fusional relationship with each other, are inversely proportional to the corresponding rates of physical vibration."[37] Fusion means the whole mass of sound formed by the two tones. Contemporary research seems to indicate that the entire basilar membrane, does, as a matter of fact, vibrate simultaneously, but it is difficult to see why this fact would legalize a hypothesis of balanced and unbalanced tonal volume. The association element in the Watt theory, while not original with him, is less disputable. It merely reiterates the generally known fact that in tones which we hear, including the sounds of nature and the human voice, those partials which occur loudest and oftenest are in order the octave, the fifth, the second octave, the third, and so on. It is a psychological truism that what we hear often together tends to become, if not definitely pleasing, at least anticipated.

Washburn[38] amplifies this habituation factor in a somewhat anthropomorphic fashion by saying that a two- or three-tone combination which emphasizes other than these habitually audible partials, produces confusion in an ear so naturally and constantly sensitized to the octave and fifth. The mental concomitant is one of unexpectedness, surprise, and novelty, and consequently, unpleasantness. The demand for a return to the accustomed would account for the existence of a musical 'tonic' or 'key-note.' Indisputable as these natural overtone intervals are, and logical as her derivation seems, there is, after all, a great quantity of music, both ultra-modern and aboriginal, which has no tonic to return to.

Emerson, working with reduced intervals, has demonstrated, at least to his own satisfaction, that "The whole apparently natural demand for the tone-combinations which give fusion (or consonance) can be inhibited during the listening to a-musical combinations as soon as a short training in miniature intervals changes the acoustical perspective."[39] The fact that our tone-consciousness has been trained in our musical tone-relations under the guidance of harmonic experiences is responsible for our apparently instinctive aesthetic preferences. This analysis would obviously reduce consonance, or at least its affective constituents, to pure association.

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[34] Max Meyer, *op. cit.*, 60.

[35] *Ibidem.*

[36] *Idem, The Musician's Arithmetic*, 1925, 1-25.

[37] H. J. Watt, *The Foundations of Music*, 1919, 20.

[38] M. F. Washburn, Psychology of aesthetic experience in music, *N. E. A. Pub.*, 1916, 600 f.

[39] L. E. Emerson, The feeling value of unmusical intervals, *Harvard Psychol. Studies*, 1906, 269 f.

Wood[40] makes our present classification of consonant intervals the by-product of diatonic evolution, and the diatonic system in turn is a by-product of the natural differences between the male and female voice, which led to the repetition of ceremonial melodies an octave or a fifth apart. His evidence rests historically on the late appearance and very slow growth of the harmonic system in music. As will be later indicated, Wallaschek, the foremost authority on primitive music, tends to refute the belated appearance of harmony.[41] There is also dispute as to which is cause and which is effect—our diatonic system, or its 'consonant' intervals.

Buch,[42] experimenting with both fusion and smoothness as criteria, decides that "fusion" is an unnecessary term as applied to consonant relationship, and somewhat drastically reduces the entire phenomenon to the ordinary laws of association.

Peterson,[43] one of the more recent experimenters, propounds what he calls a "functional view" of consonance. In reality, it is the Helmholtz-Krueger theory plus the social-genetic theory of Moore. According to Peterson, consonance is too complex to be called an "elemental capacity" in the Seashore sense, but it is rather a matter of synthetic perception comparable to the perception of an overtone clang, thoroughly conditioned in turn by socially transmitted musical habit.

Ogden[44] asserts a somewhat similar theory, except that his genesis of concord is biological, not social. Consonance is for him a perceptual synthesis, which by virtue of centuries of repetition of the 'natural' overtone intervals has made these discrete elements a consonant 'unity.' "The nervous correlate of consonant relationship," he says, "I regard as nothing other than a relatively simple and economic activity on the part of the sense organ and nervous system. Such dispositions, I take it, are in the nature of fundamental capacities resulting from very frequent racial experiences." The chief objection to this theory is the assumed transmission of acquired characteristics—which, with the current change in biological attitude, may be eventually eliminated.

There remains to discuss briefly the comprehensive genetic survey of Moore.[45] Tonal fusion, for him, means simply a highly permanent connection wrought under the influence of repeated sensory coincidences, "the nature of whose ancestry has been too little considered."[46] According to this conception, the first and original dissonance was the octave, prior to which all music was in unison. This dissonant interval, having crossed somehow over the borderline of usage, occasioned perceptual conflict, hence an emotional reaction of pleasure-pain, with its successful incorporation into the musical system finally leading to a dominance of pleasure. As the ear became accustomed to it, its affective value correspondingly suffered diminution, and since there was no struggle involved, it became indifferent. Musical innovators then inaugurated the fifth, which followed a similar career. The fourth, the sixth, and the third came in due turn, though relatively late historically, and now the diminished seventh is over the threshold. He says, to quote him formally: "The inherent feeling value of a particular interval is a function of two factors, success and degree of difficulty. It is in the barely successful synthesis (fusion) in which the individual accomplishes with difficulty the unification of a manifold, that he finds the keenest pleasure. If the synthesis is effected without effort, he has little awareness of his accomplishment, and therefore little pleasure. If, on the other hand, he is baffled by the complexity of the manifold to be unified, he experiences acute displeasure."[47]

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[40] James Wood and C. K. Ogden, *Foundations of Aesthetics*, 1922.

[41] Richard Wallaschek, *Primitive Music*, 1893, 139-144.

[42] Einar Buch, Über die Verschmelzungen von Empfindungen besonders bei Klangeindrucken, *Phil. Stud.*, 15, 1900, 240.

[43] Peterson, *op. cit.*, 20-33.

[44] R. M. Ogden, *op. cit.*, *Psychol. Bull.*, 6, 1909, 297-303.

[45] Moore, *op. cit.*

[46] *Ibid.*, 4.

All perfect consonances, according to this theory, would be indifferent —as they actually are; all imperfect consonances would be pleasant; and all dissonance would be acutely unpleasant. So far, the theory seems to account for the vagaries in aesthetic reaction to tonal combinations, and particularly for their historical shift. But Moore is an extremist, and summarizes thus: "We must expect that our whole system of harmony will gradually come to reflect whatever changes occur in the consonance degrees of its constituent intervals; and this latter type of change is one to which no limit can be set."[48]

If this infinity of limit were correct, we should expect music eventually to become an obsolete art. For, having accustomed ourselves to all its available intervals in all its conceivable scales, we should sometime strike the physical limits of audition, and if there were no more 'dissonances' to incite our pugnacious instinct, and all the other intervals had through habituation approached the stage of indifference which the octave and fifth now presumably hold, there could be no further emotional or aesthetic appeal in harmony. Perhaps this criticism is far-fetched. Certainly Moore provides us with a good description of what happens in musical adaptation to new concords, but as will be pointed out later, he has fundamentally erred in extending his boundaries beyond the natural physiological activities of the aural mechanism.

To deviate momentarily from scientific paths and venture over the boundary of aesthetic theory, we find the same inconsistency of opinion. Gurney[49] withdraws himself from the complicated relation of consonance and music, just as he begins to suggest acceptance of the Helmholtz theory. Parker[50] states that consonance is the basis of musical harmony, though not identical with it, and attributes to dissonance the aesthetic function of contrast. His criterion is smoothness, and his explanation is essentially the Helmholtz principles. In comparison, Croce[51] is not such a good psychologist, and is content to imply that music as an art transcends pure sensory and physical laws.

The conclusion to be derived from the résumé of the literature is that *consonance* is not *accordance*—not at least by definition. Yet, historically and experimentally, the controversialists classify themselves into definite categories:

(1) Those who make sensory fusion a criterion for consonance.

(2) Those who make perceptual 'smoothness' the criterion.

(3) Those who make feeling and affection the criterion.

(4) Those who make habit or familiarity the criterion.

Correspondingly, their explanations classify themselves as follows:

(1) Consonance as due to the numerical relationship of the primary tones.

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[47] *Ibid.*, 38.

[48] *Ibid.*, 68.

[49] Edmund Gurney, *The Power of Sound*, ii, 1880.

[50] DeWitt Parker, *The Principles of Aesthetics*, 1920, 161-165.

[51] Benedetto Croce, *Aesthetic*, Tr., 1922, 104-118.

(2) Consonance as due to the absence of beats among the overtones and difference tones.

(3) Consonance as due to the innate feeling value of certain intervals.

(4) Consonance as due (a) to ears naturally sensitized by frequent stimulation of certain partials; and (b) to the social transmission of an arbitrary harmonic system, of which we are the habit victims.

II. PROBLEM

The specific problems attacked in this study are: (1) The comparison of the judgments of all the musical intervals of the tempered scale according to (a) fusion, (b) smoothness, and (c) affective value, *i.e.* pleasantness or unpleasantness. (2) The comparison of the judgments given by (a) musically untrained subjects, (b) subjects with a moderate amount of musical training, and (c) very advanced musical performers—principally concert or teaching musicians. (3) The construction of apparatuses (a) to provide a stimulus series of pure tones of two different intensities, and (b) to provide a duplicate series of musical clangs with pronounced overtones.

Apparatus and Method. With modern invention and electrical equipment facilitating the generation of pure tone and the control of its intensity, the physical bases of music will undoubtedly enjoy much more accurate analysis than they have in the past. Historically, apparatus for determining consonance and dissonance has been limited to the clangs of various musical instruments, or to the pure tones of the tuning fork. Helmholtz worked principally with violin tones, though he supplemented them by a number of other generators; Stumpf and Buch with pipe organ tones of different stops; Krueger and the more recent investigators with tuning forks and Tonmesser. Malmberg used piano, tuning fork, and pipe organ tones.

Theoretically, and especially if the overtone hypothesis holds true, both the clang type and the specific condition of any musical generator ought to yield results not strictly equatable nor accurately comparable to those of a different medium. Thus, by virtue of its prominent third, fourth, and fifth partials, an interval played on the violin could not produce the same *physical* fusion as the interval played on the clarinet, whose construction permits its seventh, eighth, ninth, and tenth partials to average each from 15 to 18% of the total loudness; and neither ought theoretically to produce the same effect as the interval generated by the simple tones of forks and resonators. Collective experiments do demonstrate a discrepancy in data, but since the net result seems anyway to fall into the conventional harmonic classification, very little attention has been paid to this variable factor.

Method and technique show the same variations as do apparatus and theory, ranging from Euler's purely mathematical calculation and the direct analyses of Stumpf and his disciples to the current method of making paired comparison judgments under each separate criterion. Külpe's suggestion for elaborating the reaction-time experiment is an intriguing one, but with the exception of Max Meyer's work, very little has been done with its possibilities.

For this experiment, three types of generator were selected: mounted Koenig forks to provide low and medium intensities of pure tone; Helmholtz resonators to provide high intensity of pure tone; and organ pipes to provide the overtone-clangs. The method of actuating forks is really a serious problem in technique. Even if electrically driven, so that intensity is controlled, there remains the doubtful period preceding maximum vibration. Since our results from tuning forks serve principally as a check against the Helmholtz resonators, we have contented ourselves with a simple adaptation of pendular striking. Two hard rubber balls, mounted on metal rods and suspended in a wooden frame at an appropriate angle are raised at every trial to a level of 45° and allowed to fall 2 sec. after the warning signal. In this way, at least some effort is made to equalize the energy of stimulation between the sounding primaries.

A familiar variation of this method involves the actuating of several tuning forks with the subsequent damping of all but the fundamentals of the desired interval. A comparison of data from the two methods will show discrepancies and very interesting introspective reactions apropos of auditory imagery.

The second apparatus consists of a set of mounted Helmholtz resonators, equivalent in pitch to the tuning forks.[53] The resonators are individually actuated by a set of valves controlling the admission of compressed air, the force being equalized by means of a subsidiary constant pressure air tank.

In the third part of the experiment, we use a series of open organ pipes, similarly actuated with uniform pressure by attaching them to the rubber conductors in place of the resonators. The pipes are selected from the 'wood-wind' series and reproduce quite audibly the first, third, and fifth overtones in addition to the fundamental.

The intervals common to all series are as follows: unison, major and minor second, major and minor third, perfect fourth, perfect and diminished fifth, major and minor sixth, major and minor seventh, and octave. The tonal range extends from c′ of 256 vd. to c″ of 512 vd.

In accordance with Stumpf, any investigation of consonance must take into consideration the following objective variables: distance on the tonal scale, absolute and relative intensity, spatial separation, the number of fusing tones, the duration of the clang, partial tone exchange, the subjective variables of attention, practice, fatigue, and memory. Külpe would add clang analysis of the connecting tones, combination tones, expectation, and habituation. We should add, as a very important though neglected factor, *sequence*. Obviously any single one of these many factors is sufficient in itself for a problem in research. But it is equally obvious that in isolating any one intensively, the others must be gauged as accurately as possible for correct technique. Accordingly, attempt has been made in this experiment to control intensity, duration of the stimulus, sequence of interval, and the various psychological elements of attention, fatigue, habituation, and the musical background of the individual observers.

Preliminary to his experimental observations, every *S* filled out a questionary, which, for purposes of standardization, follows the one in Seashore's text. In addition, these directions were appended: (1) Name, in order of preference, your favorite musical composers. (2) Describe briefly the elements of musical theory you have studied.

This brief questionary makes no pretense of covering the detail in the individual's musical nature and musical history. It is merely an index to his degree of training, the type or types of music in which he is interested, and the possible harmonic prejudices he may have acquired in his experience.

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[53] This apparatus was first set up for elementary laboratory experiments by Carl R. Brown, instructor in psychology. The principal modification here is the addition of easily regulated valves to govern the air supply.

Subjects. We used 106 *Ss* in the experiments; 56 of the *Ss*, students in experimental psychology, were musically untrained; 34 of them, members of musical sororities, were moderately trained; and 16, members of the staff of the University of Michigan School of Music and other professional musicians, were highly trained.

For experimental work *S* was seated with his back toward the apparatus, with the sound source 8 ft. away approximately in a straight line with the median plane of his head. All stimuli were binaural. Warning signals of 2 sec. preceded the stimulus, and rest intervals of 10 sec. were interpolated between every two series to counteract fatigue. *S* used prepared blanks on which to record his reactions. Whereas Malmberg used the method of knowledge and permitted his *Ss* to discuss their judgments before recording them, we preferred to employ strictly the method of ignorance wherever possible.

Experimental series. The complete experimental series were as follows:

Series Ia. Fusion was taken in Series Ia as the criterion of judgment. Duration of the clang was 3 sec. Time interval between stimuli was 6 sec. The method was that of direct analysis, *i.e.* *S* recorded his judgment regarding the number of tones heard. If a judgment was easily made *S* marked it with a plus sign, if made with difficulty he marked it with a minus sign. For statistical reasons no indifferent judgments were permitted, they all had to be marked as easy or difficult. The clangs were given in haphazard order, but so distributed that every interval in the scale followed at some time during the experiments every other one.

Series Ib. Backward repetition of Series Ia. Duration of the clang was increased to 10 sec., and the time interval between stimuli to 20 sec. Series Ib was in other respects the same as Series Ia.

Series II. Tuning forks: procedure similar to that of Series Ia and Ib.

Series III. Organ pipes; procedure similar to that of Series Ia and Ib.

Series IV. Tuning forks; group actuated, damped selectively; otherwise procedure similar to that of Series Ia and Ib.

Series V, VI, and VII. Repetition of Series I, II, and III with smoothness as the criterion of judgment. No arbitrary *Aufgabe* was imposed, the *Ss* were permitted to interpret 'smoothness' in their own manner, just as they do under ordinary circumstances when they listen to music.

Series VIII, IX, and X. Repetition of Series I, II, and III with the affective value of the stimuli (pleasantness, unpleasantness, indifference) used as the criterion of judgment.

Series XI, XII, and XIII. Tuning forks, resonators, and organ pipes were used respectively to provide the single tone stimuli. Otherwise procedure similar to that of Series Ia.

TABLE I

SUMMARY OF RESULTS

Group of *Ss*	No. *Ss* in Group	No. of Trials per Interval	Total No. Trials per Interval	No. of Intervals	Total No. Trials per Group
I. Untrained	56	10	560	12	6720
II. Moderately Trained	34	10	340	12	4080
III. Highly Trained	16	10	160	12	1920

Results. For the most part the quantitative results of the experiment are translated into curves which are given in Figs. 1-14. Unless otherwise indicated, the ordinates represent either the percentage of error, or the percentage of absolute judgments, and the abscissae represent the musical intervals employed. Since the curves give the data of the tables it was deemed unnecessary to include the latter. With the exception of Table I, which gives the summary of the results, we have therefore omitted the tables.

From analysis of the curves in the accompanying Figures, it will be seen that the results for smoothness and for fusion are approximately the same. The sequence with the resonators for Group I is as follows:

GROUP I (Resonators)

Fusion:	8	5	M6	4	M3	m3	m6	m7	m5	M7	M2	m2
Smoothness:	8	4	5	M3	M6	m3	m7	m6	m5	M7	M2	m2

The differences between the curves concern chiefly the position of the perfect fourth, and the sequence of the thirds and sixths, the first two and the last two intervals of the series being strikingly coincident. Reference to the curves for tuning forks and pipes will show a similar consistency of result, the variations being small at both ends, and occurring principally in the region of the major and minor thirds and sixths. Practically the same relationship holds within the data for Group II and Group III.

GROUP II (Resonators)

Fusion:	8	4	5	M6	m3	m6	M3	m7	m5	M2	M7	m2
Smoothness:	8	5	M3	M6	4	m7	m3	m6	m5	M7	M2	m2

GROUP III (Resonators)

Fusion:	4	5	8	M3	M6	m3	m6	m5	m7	M7	M2	m2
Smoothness:	8	5	4	M6	M3	m6	m3	m7	m5	M2	M7	m2

There is a noticeable variation in amplitude which characterizes the curves for different stimuli among Group III, which at first seems inconsistent. It is perhaps best explained by the following excerpts from introspective reports:

> R. L. (concert pianist): It is hard to regard each separate stimulus as such. I usually recognize each tone of the combination, even with the octave. Then I have to stop and think, 'What is its *smoothness* value?' I find myself invariably comparing it either with the preceding stimulus, or with a mental image of various other musical intervals. Now a major sixth, for instance, from the organ pipes, seems to me to blend into a smoother sound than a major sixth with the tuning forks, or perhaps I am just thinking that the overtones ought to cover up the separate primary tones. In other words, I find myself rationalizing, and comparing and classifying, instead of regarding each chord as a separate sensation experience. I doubt whether a trained musician *could* really do the latter.

S. W. (professional organist): I find it hard to overlook musical theory and harmonic experience in my judgments. That is, it is hard to say whether a major seventh is "smooth" without having an actual context or supplying it with an imaginary one.

This tendency to rationalization is fairly prevalent among the *S*s of Group III. The reactions of the members in Group I are, on the contrary, fairly devoid of such empirical ramifications, and their experiences more closely approach a strictly sensory aspect. They show little inclination to dissect each clang, and are less guilty of confusing the affective with the sensory element. Ignorance works exceedingly well in the case of their judgements of fusion, and they show little hesitancy in deciding which clangs are 'smooth' and which are 'rough,' in comparison with the other group.

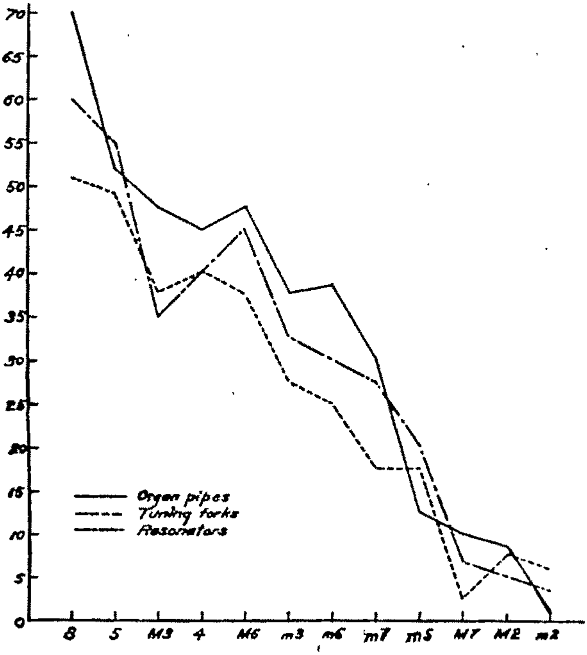

FIG. 1. FUSION DATA FOR GROUP I
The ordinates represent the percentage of error (judgments of 2 tones as one tone). The abscissae represent the musical intervals

It is interesting to note in passing the striking differences between the fusion curves of Malmberg and those of this study. We compare below Malmberg's results with the weighted results from Group I.

Malmberg:	8	m2	M2	M5	m3	M3	4	m5	m6	M6	m7	M7
Group I:	8	5	4	M6	M3	m3	m6	m7	m5	M2	M7	m2

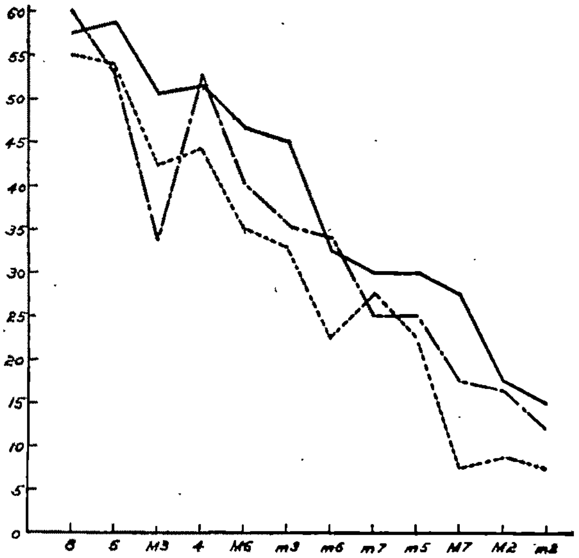

Fig. 2. Fusion Data for Group II
The ordinates represent the percentage of error; the abscissae represent the musical intervals

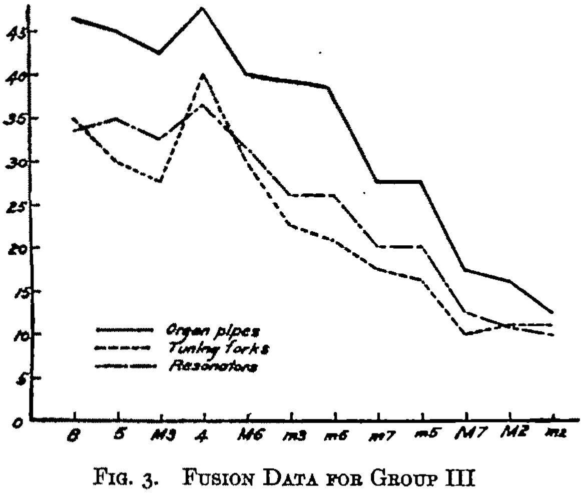

Fig. 3. Fusion Data for Group III
The ordinates represent the percentage of error; the abscissae represent the musical intervals

The most feasible explanation of these discrepancies is the difference in the technique employed. Our *S*s were unaware of the actual number of tones sounded at any time. They were furthermore given the simple direction, "listen carefully to each one of the following series of tonal stimuli which will be given after a warning signal, and record the number of tones heard in each case. If it is difficult to make a judgment, record also a minus sign; is it easy, record a plus sign."

Malmberg,[53] moreover, defined 'fusion' for his *S*s, and his experiment was, as he says, "made 'with knowledge,' and was conducted on the plan of an informal seminar, allowing a discussion of each judgment, but each observer recording his own final judgment."

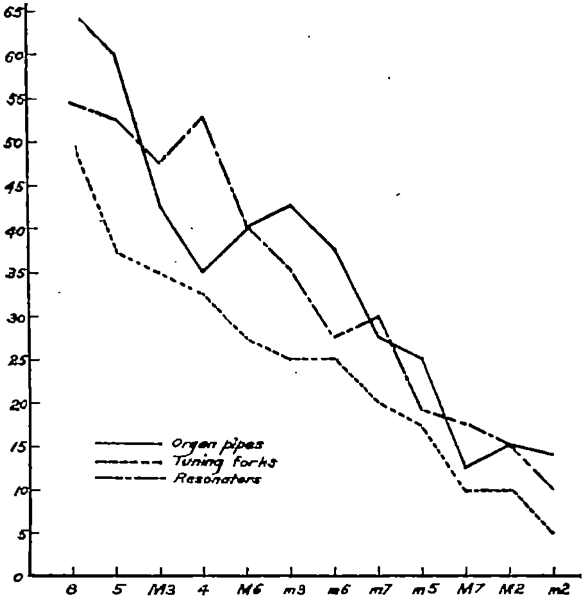

FIG. 4. SMOOTHNESS DATA FOR GROUP I

The ordinates represent the percentage of total trials in which an interval is called "smooth." The abscissae represent the musical intervals

The fusion curves obtained in our study agree, on the other hand, rather closely with the curves obtained by Stumpf, who gives the following sequence:

Stumpf:	8	5	4	M3	M6	M2	M7
				m3	m6	m2	m7

The influence of tone-quality appears in the amplitude rather than in the absolute form of the curves for different generators. The results show with marked consistency that organ tones fuse the most easily, with resonators second, and tuning forks last in effect. This difference cannot be due merely to differences in intensity, since the former two are actuated by the same absolute energy, mechanically controlled. The only plausible explanation seems therefore, to be in terms of overtones. The ear is unaccustomed to pure or simple tones, since they are generated only in unusual situations, but it is thoroughly accustomed to the various natural overtone clangs and compounds in speech, noise, and music. Whether this habitual perception of 'oneness' is merely carried over, or whether the physical rôle of overtones in fusion is the determining factor, the results are nevertheless evident and consistent. Since, however, there is not coincidence in amplitude for the fork and resonator series, both of which produce pure tones, it is desirable as a future study to determine the exact influence of intensity by using generators of minimal and of maximal intensities.

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[53] *Op. cit.*, 109 f.

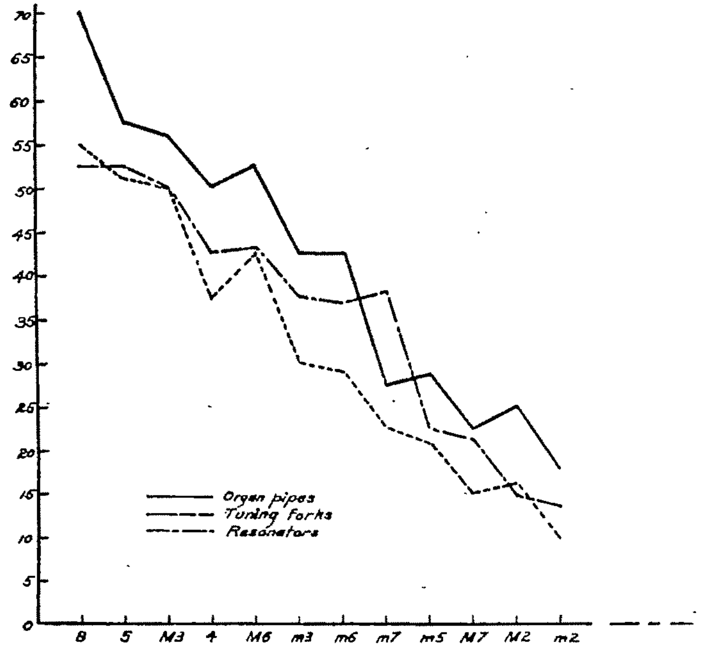

FIG. 5. SMOOTHNESS DATA FOR GROUP II
The ordinates represent the percentage of total trials in which an interval is called "smooth." The abscissae represent the musical intervals

As would be expected, the difficulty or ease of judging the number of tones generated, as indicated by plus or minus, is almost directly proportional to the degree of sensory fusion.

Fig. 10 shows the curves in terms of plus-judgments for all the groups; the minus judgment curve is merely inversely proportional to the plus-judgment curve. The reduced amplitude of the plus curve is due obviously to two types of 'error within error,' which are discounted in the averages. For instance, a trial in which the judgment of an octave is recorded 1+ is omitted in the computation, and only correct combinations, such as 2− for a major fifth, or a 2+ for some other interval of two primaries, are used for statistical reasons. Similarly, a judgment of 4+ or 4− in response to any interval is discounted, since, for the purpose of this particular curve, one would cancel the other. In no instance, as a matter of fact, are more than two tones sounded simultaneously, though there is a plentiful sprinkling of unisons used for checks.

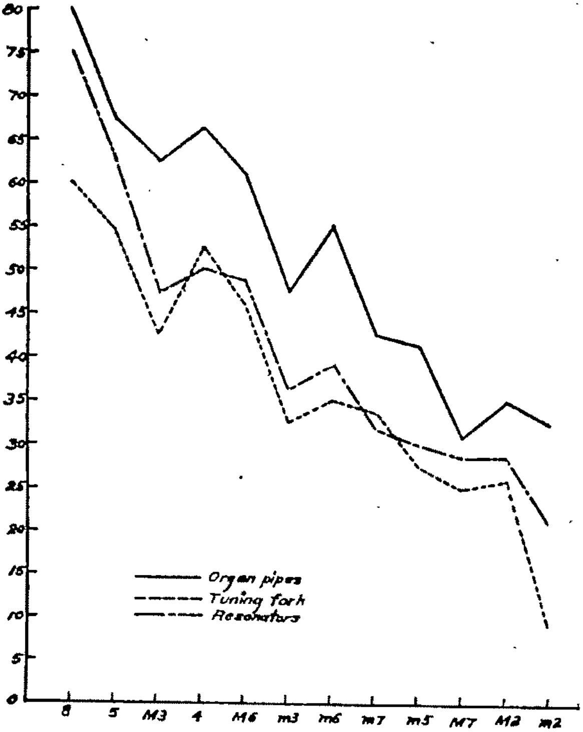

FIG. 6. SMOOTHNESS DATA FOR GROUP III
The ordinates represent the percentage of total trials in which an interval is called "smooth." The abscissae represent the musical intervals

Fig. 11 shows the curve for these 'errors beyond two.' It is readily seen that the tendency to overestimate the number of

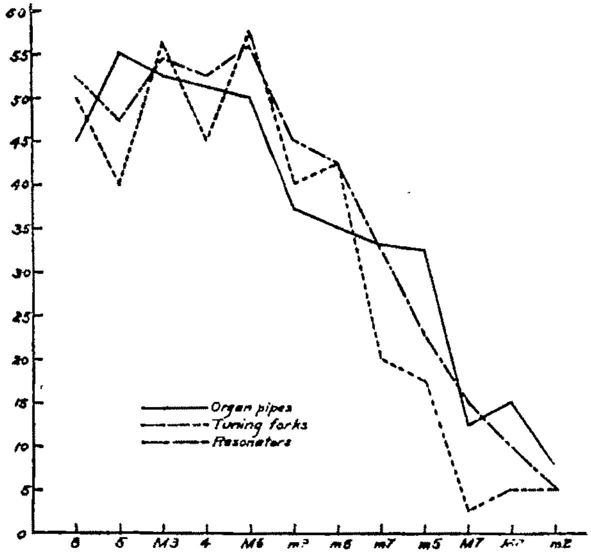

FIG. 7. PLEASANTNESS, GROUP I
The ordinates represent the percentage of total trials in which an interval is called "pleasant." The abscissae represent the musical intervals

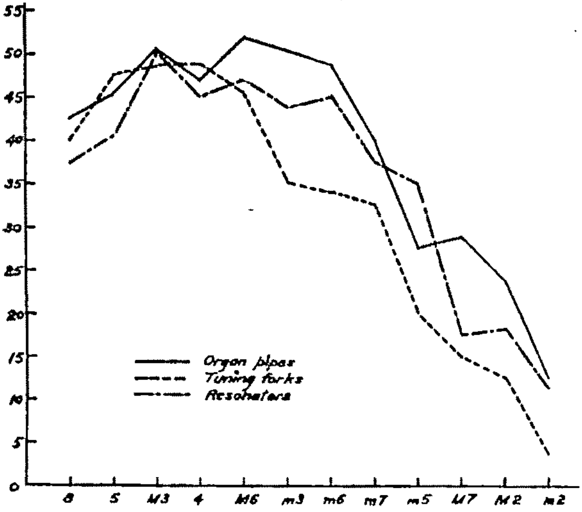

FIG. 8. PLEASANTNESS, GROUP II
The ordinates represent the percentage of total trials in which an interval is called "pleasant." The abscissae represent the musical intervals

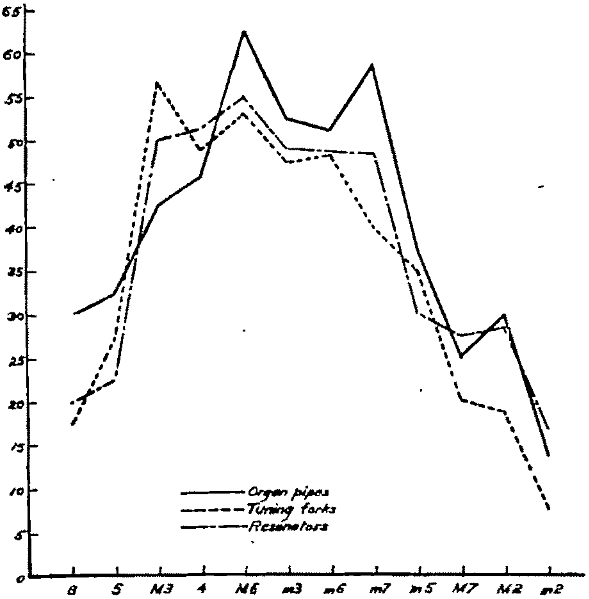

FIG. 9. PLEASANTNESS, GROUP III
The ordinates represent the percentage of total trials in which an interval is called "pleasant." The abscissae represent the musical intervals

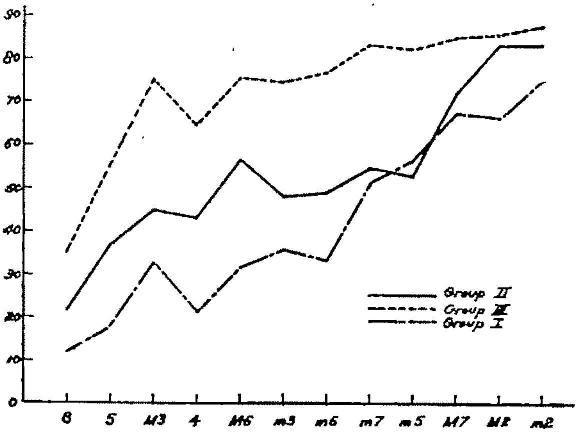

FIG. 10. PLUS JUDGMENTS. RESONATORS
The ordinates represent the percentage of total trials in which the subjects recorded their judgments as being easily made (judgments of one or more tones). The abscissae represent the musical intervals

sounding primaries varies proportionately with decreasing sensory fusion. For instance, the weighted curve for all groups shows the following table:

Interval:	8	5	M3	4	M6	m3	m6	m7	m5	M7	M2	m2
Number 'multiple' errors:	6	14	20	18	18	20	29	33	34	41	45	47

With tuning forks and resonators these multi-tone judgments result primarily from thirds, sixths, sevenths, and seconds. Although the curve for organ pipes follows the same general contour—all its intervals are more frequently confused by many *Ss*. In the first case, difference and summation tones may possibly be held accountable for the results, although this assumption would belie the fact that difference tones from the simple fusions are ordinarily more easily audible than those from dissonant primaries. Certainly in the latter instance, the multiple auditory effect may be the natural consequence of overtones.

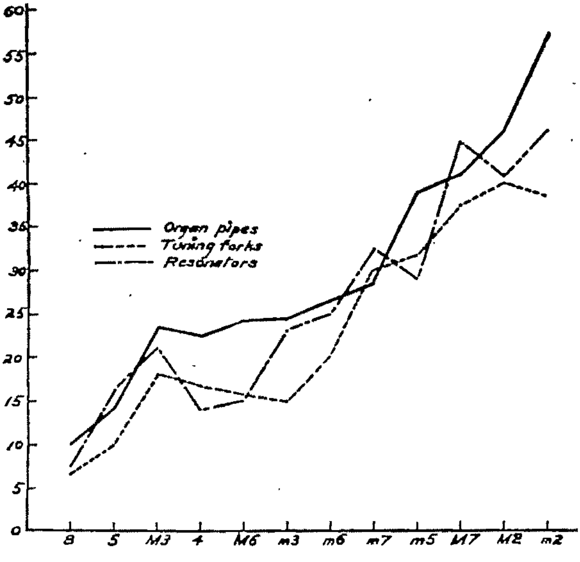

FIG. 11. MULTIPLE TONE JUDGMENTS, GROUP I
The ordinates represent the percentage of total trials in which the subjects recorded judgments of 3 or more tones. The abscissae represent the musical intervals

The technique of successively actuating and selectively damping the tuning fork series leads to confusion of judgments.

*S*s in general report a carrying over of auditory after-images. Most of the *S*s in Group III remember all the tones sounded, and recognize each as it is eliminated. This 'confusion' on the part of the untrained *S*s, and the analysis on the part of highly trained *S*s doubtless accounts for the somewhat erratic variations from those curves established by the more direct and simple method.

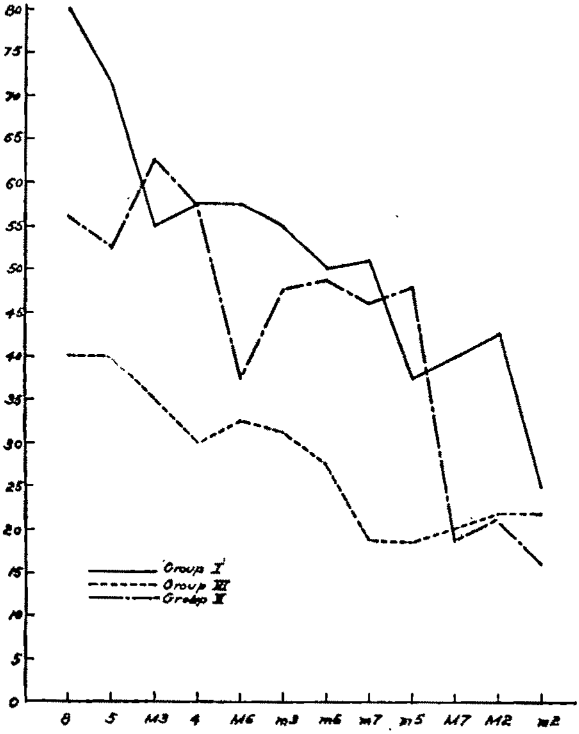

FIG. 12. FUSION: TUNING FORKS
GROUP ACTUATED AND SELECTIVELY DAMPED
The ordinates represent the percentage of error (two tones heard as one). The abscissae represent the musical intervals

The series of single-tone stimuli are introduced chiefly as a check against practice and attitude, and serve excellently as an illustration of the effect of *Aufgabe*. The directions given for these series are identical with those given for the other fusion series. Curves for Group I and Group III are in terms of absolute error, and while their form apparently means very little, the difference in amplitude is again strikingly indicative of variations in acuity between the two groups of *S*s. This compound perception of a 'simple' stimulus must be accounted for either by the presence of audible partials or by the carrying-over of auditory imagery. The first effect would be expected where pipes are used, but is theoretically inexplicable in the case of pure tones, without the assistance of auditory imagery, tonal memory, and habituation reactions.

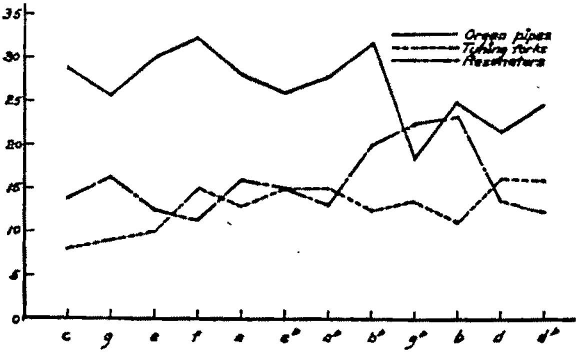

FIG. 13. FUSION: SINGLE TONE STIMULI, GROUP I
The ordinates represent units of absolute error (*i.e.* single tone stimuli judged as two or more). The abscissae represent the tonal series used

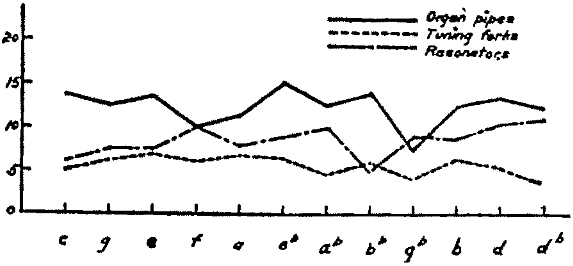

FIG. 14. FUSION: SINGLE TONE STIMULI, GROUP III
The ordinates represent units of absolute error. The abscissae represent the tonal series used

The influence of sequence is apparent in several ways. A unison following another interval than an octave is frequently called an octave, and vice versa. An octave is much more likely to be called 'one' tone if preceded by a second or major seventh than if preceded by a fourth or fifth. Similarly, the interval of the second following an octave or a unison is practically always perceived as two, and frequently as four or five. The effect of sequence is not only in contrast, but is apparent in auditory after-images and auditory memory. The longer the time interval between the successive clangs, the less the effect of previous stimulation and affective inertia on the specific judgment. This is, however, partly dependent on the nature of the individual listener; *Ss* with pronounced auditory imagery are more prone to direct comparison and contrast than those with poor auditory imagery. These subjects frequently report a difficulty in obeying the instructions to 'isolate' each disparate stimulus as such.

By reference to Figs. 7, 8, and 9, the striking differences in affective response among the three groups of subjects are immediately obvious. Using the resonator series as a standard, the following table may serve to illustrate these variations:

Preferences in terms of degree of Pleasantness

Group I:	M6	M3	4	8	5	m3	m6	m7	m5	M7	M2	m2
Group II:	M3	M6	4	m6	m3	5	m7	8	m5	M2	M7	m2
Group III:	M6	4	M3	m3	m6	m7	m5	M2	M7	5	8	m2

Two outstanding features of the various curves are the sharp decline of the minor second, and the high affective value of major thirds and major sixths. Among Group III the octave and fifth are relegated to a surprisingly low point, even the major and minor seconds and sevenths superseding them in affective interest. In the original records, octaves and fifths are most frequently called 'indifferent;' minor seconds are called 'unpleasant,' a difference in reaction which is highly significant, even though it cannot be incorporated in a curve built on 'pleasantness' judgments alone.

CONCLUSIONS

From analysis of the preceding facts, the following conclusions apropos of consonance seem justified:

(1) Consonance, if kept in strict accordance with its literal definition, cannot be defined in terms of either of the two criteria, fusion or smoothness.

(2) Tonal fusion, as such, is a purely sensorial phenomenon, perfectly distinct from the aesthetic or affective experience of consonance. This statement must be qualified, in that the lower stages of consonance and the lower stages of fusion almost universally coincide. The one marked example is the minor second. With the higher stages, there is not coincidence, but neither is there antithesis. For example, the octave is in few instances called unpleasant, but is quite generally indifferent.

(3) Smoothness, minus a special arbitrary interpretation on the part of the instructor, is equally insufficient as a criterion, denoting as it does, manifold connotations and depending obviously on the musical experience of the individual.

(4) Of the three criteria, the affective elements of pleasantness and unpleasantness seem most legitimately applied to consonance, and most competent to account for the historical shift in consonant intervals, and the personal idiosyncracies in harmonic judgment.

(5) Differences in the perception of consonant intervals is dependent more on the mental setting, *i.e.* the criteria of judgment and the musical nature and training of the individual, than upon musical differences of quality in the generating tones, or the absolute or relative intensity of the stimuli. Fusion and smoothness on the contrary are definitely conditioned by intensity, clang color, duration, time sequence, and all the other variables hitherto mentioned in connection with technique.

(6) In accordance with Max Meyer, it would appear from this study that consonance and dissonance are not antonyms other than as they apply to muscial tones and to noise. One universal exception to this conclusion is the minor second, which holds the lowest point on all the curves derived from these experiments.

DISCUSSION

As has been repeatedly implied, the significance of consonance, fusion, smoothness, in practical music, is a relation stated by many, and explained by few psychologists and aestheticians. Stumpf was the first psychologist to suggest that musical systems had their psychological roots in tonal fusion. Helmholtz considers the 'dissonant' intervals, or those involving beat phenomena, aesthetically unpleasant, and he regards the music of Wagner and Berlioz, for instance, as inferior to the simpler and smoother music of Haydn and Mozart, or even to the earlier Palestrina forms of polyphony. He says, to quote him directly, "that expression which modern music endeavors to attain by various discords and an abundant introduction of dominant sevenths, was obtained in the school of Palestrina by the much more delicate shading of various inversions and positions of consonant chords. This explains the harmoniousness of these compositions, which are nevertheless full of deep and tender expression, and sound like the songs of angels with hearts affected but undarkened by human grief in their heavenly joy. Of course such pieces of music require fine ears both in singer and hearer, to let the delicate gradation of expression receive its due, now that modern music has accustomed us to modes of expression so much more violent and drastic."[54]

Even musicians of the present day seem prone constantly to refer to Bach and Haydn or the early church chorales as 'pure' music, thereby implying that contemporary music is 'impure,' contaminated by intervals other than the octave, fourth, fifth, and sixth.

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[54] Helmholtz, *op. cit.*, 225 f.

Külpe likewise lays the psychophysical foundations of musical forms in consonance, even though he can give no final accounting for that term. He says that polyphonic music makes a more or less sharp line of distinction between consonant and dissonant compound clangs, and that it is reasonable to suppose that this distinction is based upon a definite psychological effect.[55]

Coming down to modern texts in harmony, counter-point, and fugue, we find the following familar 'laws' which wield theoretical absolutism, at least, over musical composition. Thus in Shepard,[56] "intervals are classified according to their musical effect, as (*a*) *consonant*, meaning those intervals upon which it is agreeable to pause, and which do not need to be followed by another interval to produce a pleasant effect; (*b*) *dissonant*, those not satisfying to dwell upon, or to use in the final chord of any composition. Being a dissonance, the two notes of the interval of the second and seventh should not be sung together, unless once or twice merely to show their dissonant character."

Similarly, we find in Richter's popular text, the statement that "a consonant interval is one whose tones are in so pure and satisfactory relation as to require no further special connection with any other intervals as a necessary completion of their harmony."[57] "Diminished primes, (major sevenths), seconds, and ninths, are harmonically inconceivable. The diminished sixth, like the augmented third, may occur only in exceptional cases, in which the perfect fifth may generally take its place, as the perfect fourth may be substituted for the augmented third."[58]

To an obedient composer, therefore, dissonances are restricted to melodic progression or else to mere passing notes.

It is further significant, that in our inherited musical vernacular, the octave, fifth, and fourth are classified as *perfect* intervals, by virtue of their perfect or complete consonance, whereas for thirds and sixths, whose consonance is 'incomplete' (whatever that may mean), the ordinary terms, major and minor, must suffice.

There is, of course, no agreement as to the origin of music. Max Meyer,[59] and Lipps,[60] defend a melodic basis; the majority of psychologists and aestheticians, however, seem either to accept or to modify[61] Wallaschek's derivation from the rhythmical impulse in man. Few have, however, attributed the origin of music to an 'impulse of consonance.' Yet the Hottentots, the Bechuana, and the Bachapin tribes of Africa were found to have gom-gom orchestras which played in parts,[62] and to sing their monotonous, undeveloped melodies of two or three successive tones in harmony. In Java, orchestras with native instruments play variegated and rather elaborate music in different parts, and though the harmony may sound odd and unmusical to our ears, it indicates, under analysis, an approach even to the intricacies of counterpoint. In Siam, appreciation of concords is highly acute and the Siamese are capable of tuning almost any instrument, native or foreign, to a highly exact pitch. The New Mexican Keres sing three-part songs, and in Guatemala primitive marimba orchestras play ensemble and accompany songs. According to Wallaschek, these ethnological facts refute the traditional theory that harmony is a modern or even a recent invention. He would make it coincidental with melody and both of them subsequent to rhythm.

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[55] Külpe, *op. cit.*, 280 f.

[56] F. H. Shepard, *Harmony Simplified*, 1912, 35.

[57] E. G. Richter, *Manual of Harmony*, 1912, 4.

[58] *Ibid.*, 3.

[59] Meyer, *op. cit.*, 2.

[60] Lipps, *op. cit.*, 131-135.

[61] Richard Wallaschek, *Primitive Music*, Chap. ix.

Whatever the origin of music, the following facts stand historically in relief: (1) A narrow, but nevertheless legal majority of primitive peoples prefer to sing or perform in unison. (2) The intervals of the octave, fifth, and fourth are, with a few exceptions, common to practically all nations. One striking exception is the highly developed Javanese music, which is lacking in fourths, leading notes, and also in the semitones. On the other hand, in some primitive scales, the fourth is much more prominent than the fifth. (3) Major and minor scales and harmonies occur with about equal frequency in primitive musical systems.

Consider the structural development of musical categories. The definite harmonic system which we have inherited, is obviously based on the scale which we have adopted. Our diatonic scale, in turn, is a heritage from the Greek tetrachord which used no tonic or keynote, and the Greek scale in its turn, represents a definitely melodic or homophonic musical era.[63] According to most authorities, natural differences in vocal apparatus led to the repetition of ceremonial melodies an octave, a fourth, or a fifth below. Then came the descanters, providing two distinct melodies and the real germ of polyphony. In the early Reformation, chorales involving 'tonality,' that is, key signature, influenced church music, and practically standardized the modern diatonic scale in its major and minor modes, which is based on the relation of consonant intervals. Bach, in turn, standardized the system of tempered intonations, and our musical expression since that time has continued in these categories.

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[62] *Ibid.*, Chaps. i-iv.

[63] Waldo S. Pratt, *History of Music*, 1911, Chaps. i and xv.

To present condensed history in another way, early composers of one European generation looked on music from a modal point of view, that is, continued the Greek and Roman emphasis on melody and scales, the sequence of single tones being the important element. From this historical circumstance we have our innumerable laws of melodic progression.

From Bach (1600), to the later Wagnerian era, composers looked on music from a diatonic, that is, a harmonic point of view. The restrictions of the diatonic system are apparent. It uses only two scales out of twelve, it employs definite key signatures and insists upon them, for finality.

With Wagner was introduced the real beginning of chromaticism, and with it, our present musical revolution and its corresponding hiatus between consonance and dissonance; likewise, the psychological feud between the musical eclectics and the insurrectionists. The chief element of chromaticism is condensation, *i.e.* the omission of connecting links which the ear has come to recognize. Consequently, many of the unfamiliar harmonic intervals and sequences are really by-products of this tendency. In addition to chromaticism, there are the following definite revolutionary phenomena in modern music—the practical existence of which is quite enough to render obsolete any standard harmony text, and most of the contemporary psychological theories of music:

(1) Sequence of sevenths, both diminished and major.

(2) Use of consecutive octaves and fifths.

(3) 'Free' resolution of discord.

(4) Abolition of key signature and tonal centers in composition.

(5) Abandonment of the dominant chord.

(6) Juxtaposition of chords not in the same key.

(7) Tendency to eradicate the whole diatonic system in favor of a 'pure temperament' 12-note scale, or a quarter-tone scale.

(8) The use of discords obviously for their own value, and not as mere contrast foils.

Hitherto the whole history of musical composition has presupposed chord-formation of which one consonant interval, at least, would form some part. Yet even the first and ninth symphonies of Beethoven enter on dissonant keys. In certain parts of Brahms' famous *Requiem* we are led from one dissonance to another, never arriving at the expected resolution. Even eminent critics of the time described *Tannhäuser* as a commonplace display of noise and extravagance, as shrill noise and broken crockery effects.[64] The history of musical biography is, in fact, a history of the struggle of the great composers to establish harmonies and rhythms against contemporary prejudice. Bach, Beethoven, Liszt, Schumann, all used dissonance for 'color.' Today we hear the principal theme in Stravinsky's *Le Sacre du Printemps* heralded fortissimo by trumpets in major seconds. Debussy, who has already become an accepted classic, writes in a whole-tone scale and uses sequences of major seconds and sevenths with no compunctions of conscience. Richard Strauss, in the *Domestic Symphony*, uses a combination of strong dissonances with simple strains. In Gustave Holst, we hear constantly and startlingly the juxtaposition of different tonalities, at times even a superimposing of different keys. Schoenberg completely telescopes our ordinary musical perception by a chronic ellipsis of all familiar points. *Black Roses* of Sibelius is really a succession of minor common chords, and the theme of his *Fourth Symphony* makes frequent use of the minor second. Minor seconds occur with striking effect in Bantock's beautiful cantata, *Christ in the Wilderness*, though their consummate master is Ravel in *Pavane* and *Shéhérazade*. Rimsky-Korsakov used dissonant chords, as he says 'for color,' but nevertheless leaves his audience frequently dangling in hopeless expectation of a consonant resolution.

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[64] D. C. Miller, *The Science of Musical Sounds*, 1926, 23 f.

Scriabin, who is perhaps the real revolutionary prophet in modern composition,[65] gives a completely new system of harmony, abolishes major and minor modes, annihilates modulation, and chromatic inflection; abandons all key signatures, and even establishes that most 'ultra-modern' of scales, the duodecuple. His favorite chord, one which gives a peculiar affective color to all his music, is a dominant 13th with a flattened fifth and a major ninth. He also uses chords of two unequal fourths, and innumerable major seconds, though these latter occur chiefly in suspensions. The naming of modern musical iconoclasts might go on indefinitely—Honegger, Casella, Mossorgsky, Dukas, Milhaud, and others have sought their free and individual paths in all the legitimate and illegitimate permutations of musical sound. We have as a result, a music suggesting the rugged, the grotesque, the exotic, the startling; and it is admittedly a contrast with the smooth, sophisticated, sinuous, romantic music to which we are accustomed. Melody has remained more or less immune from the cataclysm. The radical innovations have occurred in harmony, rhythm and instrumentation.

It is primarily at this point where the conundrum of consonance occurs. In the orthodox and scientific sense of the word, this music is not music, but dissonance. It represents a definite existing musical 'cult of the second' in which discord is its own excuse for being. Secondly, this dissonance is largely ridiculed by the musical laity, who quite reasonably claim that they cannot understand it. But whether or not they understand it, the fact remains that the best symphony orchestras play it, the best concert artists perform it, and the best teachers in the best conservatories teach it. Furthermore, the reason given by artists and teachers for encouraging it, is that they like it, that it is music of a very high order, and that it releases the whole field of musical aesthetics from the arbitrary consonance-dissonance shackles of the past.

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[65] A. E. Hull, *Modern Harmony*, 1915, 72.

On the other hand looms the evidence from physics, physiology, psychology, ethnology, and aesthetics. Physics asserts that the simplest ratio between ether vibrations gives the best and simplest fusion, and that this fusion corresponds to the 'natural' overtones of any generator. Physiology asserts that intervals with simple fusion are perceived with less difficulty, are more naturally attuned to the aural apparatus, and are therefore less fatiguing and injurious to the sense organ. Psychology asserts—or at least the 'furtherance-hindrance' theory does—that in general, simplicity of perception, undisturbed flow of nervous excitation, lack of fatigue in the sense organ, give rise to the affective concomitant of pleasure; that contrary conditions, complexity of perception, unrhythmical, interrupted flow of nervous impulse, fatigue, and injury in the sensory ending create unpleasantness. Ethnology asserts that the octave, fourth, and fifth are common, practically, to all music, though a few nations, and many children, regard seconds and sevenths with equal affection. History demonstrates the unison and octave as the original material of music, with the subsequent addition of the fourth and fifth, the still later addition of the third and sixth, the almost contemporary adoption of the diminished seventh, and the purely contemporary tolerance toward the second and major seventh.

It remains the difficult task of the aesthetician, who in this case must be primarily a psychologist, to arbitrate this feud of inconsistency. The solution herewith appended is no panacea, but strictly a mongrel and a compromise, based on the observable facts at hand. It would read as follows:

(1) Consonance is an aesthetic description, totally dynamic in nature, and is not a scientifically determinable constant.

(2) Its perception is conditioned by (a) natural sensory processes, within very wide limits, and (b) the empirical factors of training, environment, musical context, etc. The lower limit of these sensory processes is represented by the minor second, which appears to be universally unpleasant, even with

highly trained *S*s; and any smaller intervals potential in quarter-tone and eighth-tone scales which incur a *consciousness* of beats. These beats may not be in themselves unpleasant, but they distract attention from the sound as such and consequently detract from the purity of the tonal experience. They are, in fact, directly comparable to the pulsations of the lowest tones from pipe organs, which, if unrelieved over a long period of time, become an excessive strain and inhibit the thematic impressions of the music. If the objection be raised that these beats disappear under conditions of monaural clang perception, it may be stated in turn that without radically different evolutionary tendencies or the universal indulgence in some sort of intricate aural contrivances, sound and music will continue to be perceived binaurally. In light of present evidence, however, it is possible that, could its beats be somehow filtered out, the interval of a minor second might very well become a consonance.

(3) Fusion is apparently a simple sensory process, and on the basis of the Helmholtz theory in particular, may be legitimately classified as perfect, imperfect, and very imperfect. It may be a criterion of smoothness, and vice versa, but not of consonance, unless the real derivation and meaning of consonance is in turn arbitrarily identified with simple sensory summation. Fusion, apparently, would fit Malmberg's definition as a "purely cognitive process,"[66] whereas consonance will not. Not only the data from extensive experimentation, but the voluntary negations and involuntary admissions (witness the number of times the investigators speak of dissonance as 'unpleasant') would indicate that feeling cannot be outlawed as a major component of consonance. The romantic and even political struggle of the major and minor thirds and sixths toward their present high place in our affection, to say nothing of our frequent song-endings in the plaintive diminished seventh, are indisputable evidence for the contention that what we regard as pleasant we also regard as consonant. This attitude in turn, is a composite social heritage from our entire racial past, and our entire musical present, and is aesthetically and psychologically legitimate so long as its expression does not transcend obvious physical and physiological limitations.

Musical theory and aesthetic principle, as well as the psychology of music, are hence confronted with difficulty in fundamental terms. If we speak of 'dissonant' music, we are really combining antonyms; if we speak of 'consonant' music in the traditional, narrow sense of the term, we have left no name for the bulk of composition since Wagner's *Tannhäuser* overture set its first audience hissing. The proper compromise therefore,

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[66] Malmberg, *op. cit.*, 131.

MEANINGFUL BEHAVIOR IN HYPNOSIS

By Curt Rosenow, University of Kansas

Broadly speaking, the stream of present-day psychology flows along two channels, traditional academic psychology and medical psychology. The academic psychologist recognizes, or should recognize, that his basic concepts must be adequate for the description and explanation of all the facts, normal as well as abnormal. The logical position of the medical man is the same. Accordingly we find each of the contending parties claiming the entire field. On the one hand, medical psychology rejects the basic concepts of academic psychology as inadequate and sterile; the academician, on the other hand, brands the fundamental ideas of mental medicine as obscure, illogical and unscientific. In the present article I do not wish to become involved in this controversy. None, except possibly some academicians, seriously questions the immense importance and significance of the medical contribution. On the other hand, I must confess to that infirmity of small minds, the love of consistency and clearness of thought. Unconscious or subconscious knowledge, unconscious desire, repression, dissociation—somewhere behind these labels there are facts of profound significance. The facts themselves are neither logical nor illogical, neither clear nor obscure, neither consistent nor inconsistent with other facts—they are. But the clearness and consistency of our description is the measure of the intellectual control which we have over the facts. In the present article I shall try to make a contribution toward the problem of gaining intellectual control over the facts which medical psychology has brought to our attention.

The facts with which I shall deal are the facts of hypnosis. The basic descriptive and explanatory concepts which I shall utilize were presented by me in two previous papers.[1] In order to facilitate that utilization, it will be convenient to refer briefly to some of the ideas expressed in them. In these articles I did not offer a new solution of the old conventional problems. I stated a new problem. In all of academic psychology *meaning* is very troublesome. The objectivist relegates it to the limbo of 'subjective' fact and does not deal with it at all except perhaps to speculate about its 'objective' correlates. The dualist who follows the prevailing fashion of dealing with facts of be-

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[1] Curt Rosenow, Behavior and conscious behavior, *Psychol. Rev.*, 30, 1923, 192-216; The problem of meaning in behaviorism, this Journal, 36, 1925, 233-248.

havior and with facts of consciousness, explicitly recognizes meaning as an attribute of mental processes, or as an elementary mental process, an imageless thought, an *Aufgabe*, or the like. There has been some discussion of what meaning *is*. There has been some discussion of the meaning of meaning. The purposivists have preached that our descriptions of mental processes are vitiated by a neglect of the purposive aspect of mental life. Meantime there are countless human activities which undoubtedly mean something, but the meaning of which is unknown. In many of these cases the meaning is unknown even to the actor. But so far as I know, no academic psychologist has envisaged that state of affairs as a psychological problem. In the articles referred to above, I have called attention to that problem. I have discussed a psychology which sees its major problem to be *the discovery of the meaning of activities in cases where that meaning is unknown.* Furthermore, I have endeavored to point to the path which he who is interested in the solution of such problems should follow.

So far as psychology is concerned, that path has been obliterated by the fact that psychologists have taken for their point of departure positions which are essentially metaphysical positions. In many cases that is due to lack of familiarity with, or lack of understanding of, the history of philosophic thought. In few cases have these fundamental positions been chosen with adequate appreciation of their logical implications. In any case the result is that the technical terms of psychology (and behaviorism) are saturated with metaphysics. To escape from these metaphysical difficulties which, it seems to me, are irrelevant to the genuine problems of psychology, I fall back upon ordinary language, taking care to limit the connotations and denotations of a few terms in such a way as not to become involved in the conventional puzzles and contradictions. The next seven paragraphs are devoted to this task.

The idea of an incomplete act has reference of some kind to what the act would be, or would have been, were it to be completed, or had it been completed. The psychologist who speaks, glibly enough, about nascent or covert activity or about implicit behavior is urged to take note of this simple, obvious fact. Thus if I start to look at X but change my mind, the psychologist may, if he chooses, describe this event as 'nothing but' an eye-movement. But he should make it clear to himself (and to others) that the eye-movement has reference of some kind to *looking-at-X* even though, as a matter of fact, I did not look at X. Failure to do so will leave him without defense against the charge that he *pretends* to be able to understand nothing but muscle twitches. For it is impossible to believe that the behaviorist is as stupid as he pretends to be.

I use the verb 'to mean' as a general term designating a very large number of specific activities, just as the word locomotion is used to designate swimming, walking, running, etc. Just as nothing is added to the description of swimming by saying that the organism is locomoting, so nothing is added to an adequate description of the activity of planning the next move at chess by saying that it is a meaningful activity. Just as an individual is locomoting only while he is swimming, walking, or running, so the individual means (*e.g.* to answer the door bell) only while he is acting. Up to this point in the argument this use of the verb 'to mean' is taken from ordinary usage without modification or addition. I recognize of course that the word is in current use in other ways as well, but in order to secure clarity of thought I limit my use of the term as indicated above. The reader who cares to make the effort necessary to the understanding of this position *must* do the same. There are many synonyms such as indicate, imply, signify, denote, connote, etc., which make the procedure quite feasible. It is easy to say that the east wind signifies rain in order to avoid the ridiculous implication that a phenomenological entity, the east wind, is engaged in a meaningful activity.

The question "What does a given specific act mean?" is to be answered on the basis of relevant evidence. Thus, if the first man at bat makes what looks like a base hit and runs toward first base, the evidence supports the view that he is trying to get to first base before the ball. Of course that may be a mistake in any given instance. There are dishonest players. I assert merely that it is *psychologically* possible for human beings to mean to do things and that it is possible sometimes to secure evidence which enables us to state with considerable certitude what an act means or has meant. A psychology which makes it impossible to ask and answer questions of this character bears witness to its own sterility. On the other hand there is to be no a priori assumption that all living activity or all human activity is meaningful. What does an oyster mean when it opens its shell? Is the growth of the fingernails an activity of the living organism and, if so, what does it mean? In the absence of relevant evidence, we can dispense with cheap and easy generalizations which answer such questions. We do not know what, if anything, such activities mean—and that is the only honest answer which is possible at the present time.

With reference to nouns in common use, such as experience, knowledge, awareness, and the like, I note that ordinary speech frequently uses these nouns merely as convenient substitutes for the corresponding verbs which denote activities such as experiencing, knowing, and the like. Indeed common sense has no more difficulty with these terms than the physicist has with the word motion. But during the history of thought a distinction has arisen between the activity of experiencing and that which is experienced. Since the distinction has been made, the nouns have become somewhat ambiguous. In that state of affairs, the least that can be demanded of the psychologist is that he avoid this ambiguity in some way. Instead of that psychologists have achieved the following logical atrocity. Beginning with the distinction between the activity of experiencing and the phenomenon experienced, they have 'discovered' that the experienced phenomenon is itself a mental process. Accordingly we have our choice of two alternatives. In a concrete case where, *e.g.* an apple is seen, either the apple or the seeing-of-the-apple is the experienced phenomenon. In the latter case the distinction with which we started has somehow disappeared. In the other case we have one mental process, viz. the activity of seeing, somehow related to another mental process, viz. the seen apple. Here the distinction with which we started has been preserved at the expense of intelligibility. In order to avoid puzzles of that kind, in order to avoid ambiguity, I shall confine myself to the use of the verbs such as experiencing, knowing, etc., and when I do use the nouns it will be as a convenient substitute in order to secure ease of diction. Anyone who is interested in the problem of ultimate reality must face these problems which I am attempting to avoid, but, as I see it, that is not the task of the psychologist. It is legitimate to orient oneself toward the modern science of physics, but that orientation will prove a very easy matter if neither the physicist nor the psychologist insist on attacking the problem of ultimate reality.

In common with all biological science, I envisage an organism reacting to an environment. In common with these sciences, in describing the various relations of an organism to its environment, I describe facts and conditions not dependent in any way upon the meaningful activities of the particular organism in which I happen to be interested. Thus the presence of a stick of dynamite with a lighted fuse attached is important especially in the absence of any meaningful reaction to it on the part of the particular organism I happen to be observing. In the same way, I describe intraorganic facts and activities the meaning of which, if any, is unknown to me. Thus the development of a cancerous growth may be of importance and may need to be described. In addition, I ask, "What, if anything, do the activities of this organism mean?" and, so far as the evidence permits, I attempt to answer the question. It is possible to omit all reference to the meaningful activity of the organism, but description of this sort would be excessively cumbersome and, as a matter of fact, is never attempted in the case of highly developed organisms. Instead the objectivist uses such language as will serve his purpose, declaring that he uses that language in an 'objective' sense only. He may speak of a dog running after a rabbit, but he means to convey merely that the dog is running in the same direction as the rabbit. Such procedure is legitimate enough provided it is a genuine problem whether that particular dog is running after that particular rabbit, but if it is held that it is theoretically impossible for dogs to run after rabbits and that that popular illusion must be replaced by 'objective' description, we are dealing with a crude, antiquated form of metaphysics and may leave the amateur metaphysician to his not very intellectual devices. Every useful purpose served by the distinction between the objective and the subjective can be attained by substituting the distinction between the given and the problematic. By the term 'given' I mean that which I believe to be true, that which I can support by strong evidence, and that which I wish my reader to take for granted. The investigator who is dealing with a genuine problem and who has described his data in such a fashion need not bother with the distinction between the objective and the subjective.

Thus far we have not gone beyond the confines of ordinary language. We have limited a few terms, but we have not extended their significance beyond accepted usage. We enter now upon more questionable ground. I leave it an open question whether I use the verb 'to mean' as a synonym for the word 'purpose,' and I do not care to embark, in the present article, upon an exhaustive discussion of the latter term. Nevertheless some discussion will be useful. One of the chief difficulties we encounter is whether the organism which acts purposively 'knows' the end or purpose of its own act. If we deny it, we seem to become involved in a contradiction in terms. If we affirm it, we are confronted by instances where that contradiction appears actually to happen. We can of course take refuge in 'unconscious' purpose, but that is merely the fashionable way of admitting that we cannot resolve the intellectual difficulty and have found the effort wearisome. In this dilemma we may appeal to the genius for the concrete of William James. In discussing the broody hen, William James said that we must suppose that a nestful of eggs to a hen is the most interesting, never-too-much-to-be-sat-upon object in the world. It seems to me that this flash of insight will solve our problem. We must bear in mind that the behavior of the hen is not language behavior, but that we must describe it by means of language. The appearance of the eggs is not a fact of language, but, described by means of language, the eggs look like never-too-much-to-be-sat-upon objects. The fact that the-hen-sees-the-eggs-in-this-fashion and the fact that the hen-means-to-sit-upon-the-eggs is one and the same phenomenon. Accordingly I assert that the activity of knowing, experiencing, or awaring (pardon the neologism) and the activity of meaning (*e.g.* to sit) is one and the same activity. Most of our meaningful acts are never completed but mean their tentative completion. But this tentative completion is seen, heard, smelled, or felt. There is to be no disjunction between that which is seen (perceived, known, meant) and the activity of seeing it. The-hen-sees-never-too-much-to-be-sat-upon-objects; that *is* the act. Obviously the eggs are never-too-much-to-be-sat-upon objects only with reference to the acting hen. Considered as a part of the environment they can and should be described in other ways as well, but the existential status of the never-too-much-to-be-sat-upon objects (as such) is not a problem for psychology—neither is the existential status of the eggs. It is only by avoiding this disjunction, as ordinary usage avoids it, that the psychologist can avoid the logical obligation of embarking upon the troubled seas of ontology and epistemology. Indeed I have joined only what no psychologist should have put asunder.

It should be obvious to the psychologist that not all individuals are able to describe what they experience with equal skill. As a limiting case we have the very young infant who cannot describe at all. Moreover, the descriptive ability of the adult varies with circumstances. There is, for example, the well-known case of the man who has just looked at his watch but is unable to say what time it indicated. In many cases I have been able to restore the 'lost memory' somewhat as follows. I recall that I looked because I had an engagement at three-thirty. I remember that I found that I had plenty of time—about forty minutes. Then I remember quite vividly the position of the hands of the watch—two fifty-three. Now I hold that my looking at my watch was the *equivalent* of a question which I asked myself somewhat like, "Isn't it about time for that appointment?" Part of the behavior of seeing the watch was equivalent to the answer, "No, there is plenty of time." A moment later I knew that I had seen that there was plenty of time, but I did not know what else I had seen. Accordingly I distinguish between perceiving and knowing what one perceives. Just as perceiving is an activity, so knowing what one perceives is an activity. I hold with George H. Mead that it is the behavior of acting socially toward one's own activity of perceiving. This point will be elaborated and (I trust) clarified in the body of this paper in conjunction with the facts of hypnosis.

We come now to the topic of hypnosis. I take it for granted that the reader is acquainted with the current theories and with the need which exists for the clarification of ideas. In order to get a list of the hypnotic phenomena before the reader, I quote Bridges as follows.[2]

> (a) Rapport, the state of dependence of the subject upon operator. (b) Amnesia in the trance for some past events, hypermnesia for others; and amnesia afterwards for events of the trance. (c) Suggestibility and loss of initiative. (d) Paralysis, contracture, catalepsy, and other effects on voluntary muscles produced by suggestion. (e) Illusions, hallucinations, and delusions. (f) Anaesthesia and hyperaesthesia; negative hallucinations. (g) Post-hypnotic phenomena, suggestions given during the trance are carried out after waking.

These facts are usually explained (or described) through the use of such terms as dissociation, narrowing of the field of personal consciousness, repression, the unconscious, suggestibility, and the like. I submit that these terms are utterly unintelligible and, at best, serve only the purpose of superficial description. I propose to discard them all without introducing any terms to take their place. I propose to show that the phenomena are the results of the meaningful behavior of the hypnotized individual in ways which that individual does not understand. As long as the psychologist does not understand them either, he will have need for some of the terms enumerated above. After he comes to understand, he will have no further need of them.

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[2] J. W. Bridges, *An Outline of Abnormal Psychology*, 1925, 128.

The theory to be put forward is the result of the observation of more than 100 *Ss* during a period of four years. There was no formal investigation. Starting with a rather vague curiosity aroused by the similarity of hypnosis and hysteria, I began to hypnotize volunteer *Ss* and to study them without a well-defined objective. As my ideas crystallized and developed, I found it possible sometimes to modify the conduct of the experiments for the purpose of testing out some definite hypothesis. For the greater part, however, I had to be satisfied with observing the facts as they presented themselves. Within the scope of this article, it will be impossible to present all of my material. It will be impossible also to answer all the questions and reply to the numerous objections sure to occur to the reader who will be unwilling or unable to refrain from translating the language which I use into his particular psychological phraseology. At some time in the future I hope to be able to discuss these topics in the detail which they deserve. At the present time these views are put forward for what they may be worth.

Most of my *Ss* were college students. The degree or depth of hypnosis obtained was usually light. Indeed, followers of Janet would say that most of my *Ss* were not hypnotized at all and would accept only a few of my cases as genuine. Probably Bernheim would have been satisfied with all of them. I shall not concern myself with this ancient quarrel. I observed certain facts and these facts need to be explained. Janet might attempt to explain them by reference to the facile complacency of my *Ss*. I shall, however, let the facts speak for themselves.

I have said that the phenomena of hypnosis are results of the meaningful behavior of the individual in ways which the individual does not understand. Inasmuch as the same thing is true of a great many other phenomena, I wish to present some facts without inquiring whether or not they are facts of hypnosis. The facts may be presented as an account of a class experiment which I perform habitually. The class is instructed to clasp their hands, the fingers entwined as in prayer. They are told to maintain this position and to fixate a point on the blackboard. The position is maintained for about 5 min. and during that time I talk to them about the necessity of maintaining the fixaton. They are to avoid the slightest eye-movement; they are to maintain a clearly focalized vision, and the like. After it has become apparent that it has become very difficult to maintain the fixation, I tell them that they must maintain it no matter what else I may ask them to do. Then I tell them that they cannot pull their hands apart and challenge them to try. Many of them are unable to do so for a considerable period and nearly all of them experience considerable difficulty in getting them apart. None of them are ever able to give a sensible account of the phenomenon, although those who have heard about suggestion or dissociation appeal to it. Then I tell them to clasp their hands again and to keep them clasped tight at the same time that they are trying to pull them apart. By that time they usually understand that it was the maintenance of a firm grip due to the diffuse tenseness of the difficult visual fixation which was responsible for the phenomenon. They understand that the mysterious character of the phenomenon was due to their temporary ignorance of the mechanical aspects of their own behavior. They knew that they were trying to fixate, but they did not know that they were keeping their hands clasped tight.

So far as I know, this simple and obvious explanation of this familiar experiment has never appeared in print. On the contrary, it is usually performed as an illustration of suggestibility. But suggestion has little or nothing to do with it. In order to demonstrate that I have varied the procedure as follows. I instruct the class that keeping the fixation point at maximal clearness is their chief and all-important task, never to be abandoned for a moment. In addition they are to follow other instructions which I will give them later, but not to abandon their efforts at fixation under any circumstances. When I judge the time ripe I remind them of their instructions and tell them to pull their hands apart, omitting any suggestion that they cannot do so. In my experience the experiment succeeds about as well in this form as when the negative suggestion is employed. That is, about the same proportion of the class encounters difficulty.

Now let us go on to the facts of hypnosis proper. The first thing which should engage our attention is the variability of the phenomena. They vary from individual to individual and they vary with conditions. Some *S*s are very easily hypnotized by almost any method. Others present great difficulty, and it is this type which I have found most instructive. Still others are easily hypnotized by one method but not by another. After hypnosis is induced the phenomena which can be produced vary enormously with the individual and with the method of procedure. I have usually worked with (presumably) normal college students and have employed the method of 'talking sleep,' but have had some opportunity to work with less normal *S*s and have used other methods. There are very many methods of induction but all of them have certain features in common. I shall base my explanation upon the method I use habitually, but it should become obvious that it applies to all other methods.

The *S* is told that his coöperation and good will are essential to success and then instructed regarding the procedure. He is to lie down on a couch and try to go to sleep. He is to put everything else out of his mind, make his mind a blank, etc. Most *S*s who try to follow these directions close their eyes as a matter of course. If they do not I ask whether it will not be easier if they do, and I have never encountered an *S* who considered it easier to go to sleep with his eyes open. After about a minute I begin to 'talk sleep' in a soothing monotone. It is not my purpose, however, that the *S* should relax completely and really go to sleep. I want him to get drowsy, but to keep on *trying to sleep*. Accordingly, if I find him too relaxed, I raise my voice, or alter my talk, in order to rouse him slightly. I have asked 50 *S*s just how they tried to go to sleep. They replied in a great variety of ways. Some resort to time-honored devices such as 'counting sheep.' Others remark that they try to relax and 'think' of being sleepy. A few say that they find it helpful to listen to the monotonous drone of my voice. Nearly all of them are more or less puzzled by the question. Apparently trying to go to sleep is trying to go to sleep. Not a single one told me that he was keeping himself motionless, resisting most impulses to move, sometimes even retaining obviously uncomfortable positions in order to avoid moving. Yet, obviously, that is what all of them did. In some cases their efforts result in a rigid, mask-like appearance of the face with which all who are acquainted with these phenomena are familiar. I conclude that the *S*s do not know these details of the behavior of trying to sleep at the time they are engaged in this behavior. At the time that the verbal (or ideational) processes mentioned above are occurring, they know that they are trying to sleep, but they do not know these details of their behavior, or, if you prefer, they have only a dim, shadowy notion of them. Inasmuch as I shall make extensive use of this fact, I wish to point out that the same thing occurs in all other methods of induction. For example, in the eye-strain method, the subject becomes tense and motionless, but does not realize (or ceases to realize) that that is part of the behavior of strained visual fixation.

In order to determine whether the *S* is quietly falling asleep or is *trying* to sleep I use various indices. Frequent changes of position are efforts to relax; rigid immobility is what I look for and try to induce. A slighly open mouth indicates relaxation; firmly closed lips, tension. A frown is favorable, a smile unfavorable. Quiet immobility of the features is ambiguous and hard to interpret unless one has worked with the *S* before. Fluttering eyelids indicate difficulty in keeping the eyes closed, and, consequently, tension; but it may not be wise to suggest that the *S* cannot open his eyes, although individuals differ here as elsewhere. My favorite device is to take a finger and to put it in an unusual position which requires muscular tension for its maintenance. As a rule I support it for a few seconds and withdraw the support gradually and tentatively. If this position is maintained indefinitely, I conclude that the behavior of trying to sleep has eventuated in the maintenance of a diffuse postural tension. If the position is not maintained, I order the subject firmly and commandingly to try to sleep, putting more or less emphasis on the word 'try.' If now the finger is lifted again the position is maintained almost invariably although the time for which it is maintained will vary with the individual. After I have succeeded with the finger I take the arm and put it into a similar position (*e.g.* a horizontal position). Usually the position is maintained as well as in the case of the finger; frequently for 15 min. or more. Now *if* the *S* responds to the manipulation of the arm as a disturbance, *i.e.* as interfering with his efforts to sleep, *if* the response takes the form of increased effort to try to sleep, *i.e.* to hold himself motionless, and *if* that increase of tonus is especially strong in the muscles of the arm on account of the uncomfortable position of the arm, the behavior is accounted for. Failure is explained through the absence of one of these conditions. For example, the *S* responds to the manipulation of the arm as an attempt on the part of the hypnotist to achieve some sort of result and coöperates by relaxing the arm; or else he may become curious and, instead of trying to sleep, he watches what is going on. It may be said in advance of a more thorough discussion of amnesia that *S*s remember (or can be made to remember) after hypnosis most of the incidents which happened during hypnosis. If the position is maintained for any length of time, that incident is, usually, not remembered at all or only very vaguely or uncertainly. When the incident is remembered, some *S*s report that their arm *remained* in the position; others report that they *kept* it there. All of them say that they have only the dimmest notion of the length of time during which the position was maintained and that the whole experience was very hazy. (Further details will be given in connection with the discussion of anaesthesia.) Now if the *S* reacts to the manipulation of the arm mainly as a disturbance, it will be difficult to remember the incident because it is not essentially different from other disturbing factors such as accidental sounds and other details of uncomfortable postural attitude; but if his curiosity is aroused, the incident will stand out more clearly. Usually both attitudes are present and the clearness of memory is a function of their relative strength. Again, the *S* who reports that his arm remained in position is ignorant of the fact that his own effort kept it there. The *S* who reports that he kept it there because he thought that that was what I wanted was not hypnotized at all, if his memory is trustworthy. But frequently that 'memory' is a secondary elaboration of his dim appreciation of the fact that his effort to coöperate resulted in the maintenance of the position; if he had stopped trying to sleep, the arm would have relaxed. This point will meet us again and will be supported by additional evidence.

Let us consider another phenomenon. The *S* is told that he cannot open his eyes. If an observable response does not occur, he is told to try. From the various responses which I have observed, I select the following. (1) Complete failure. The *S* opens his eyes and is wide awake. (2) The eyes open with difficulty. The eye-balls are turned upward and inward as in sleep. The eyes close again spontaneously and the *S* accepts other suggestions. After hypnosis he does not remember that he opened his eyes. (3) The *S* contracts strongly the muscles of the forehead and other facial muscles, but the eyelids are motionless. After hypnosis, he reports that he tried hard, but could not open his eyes. Observers whom I have called in to witness this, do not notice the passivity of the lids until their attention is called to it. The *S*s themselves never recall the fact, but they do report sometimes that their lids seemed to be made of iron or the like. (4) As above, but the eyelids flutter. Obviously the opposing muscles are strongly contracted. This fluttering of the eyelids is frequently observed in advance of any suggestion while the *S* is trying to sleep, and it seems obvious to me that the *S* finds it difficult to keep his eyes closed. In explanation it may be said that keeping the eyes closed is with most of us a part of the behavior-of-trying-to-sleep. The reader is reminded of the fact that most of the *S*s closed their eyes voluntarily when they began to try to sleep. If the *S* understood the situation completely, he might say, "I have tried hard to keep my promise to do nothing but try to sleep. Several times I have been on the point of succeeding, but each time you have disturbed me by raising your voice or changing the monotonous talk about sleep to which I had just become adjusted. If it had not been for my promise, I would have opened my eyes and shifted my position a number of times, but, with some effort, I have kept myself from that. And now, just as I have become so accustomed to keeping my eyes closed in spite of these disturbances that I am doing it automatically, you are asking me, in effect, to keep on trying to keep my eyes closed and to try to open them. It's absurd." But inasmuch as the *S* does *not* have an adequate understanding of the situation, he endeavors to open his eyes and to keep on trying to sleep (*i.e.* to keep them closed) at one and the same time, and achieves one of the compromises described above, such as trying to open his eyes with his forehead and keeping them closed with his lids. We may say, subject to later modification, that the *S* cannot open his eyes because he is trying to sleep, but does not understand that the resistance he encounters *is* his effort to sleep.

It should be said also that the *S* is more or less drowsy. We meet here one of the difficulties encountered by a writer who tries to portray the simultaneity of concrete fact with words which must perforce follow one another. An examination of the activity of feeling drowsy is essential for an understanding of the activity of coöperating drowsily with the hypnotizer. The topic is worth detailed presentation and argumentation, but in the present article I must be brief and dogmatic. For an account of the physiology of the phenomena I refer the reader to the literature. We may note, however, that the physiology of the behavior (trying to sleep) which we have discussed so far is concerned mainly with postural and facial reflexes, whereas now we become involved in changes in circulation, respiration, and the like. But from our point of view we are concerned mainly with the realization that we are not concerned primarily with the analysis of the *experience* of drowsiness. My readers, I take it, are acquainted with the experience and I shall designate it merely as an organic experience *sui generis*. From our point of view we are concerned mainly with noting that drowsiness (like awareness) is a noun which designates an activity, viz. the activity of feeling drowsy—*and that this activity is different and distinct from the behavior of trying to sleep.*

Now it is a well-known fact that the drowsy state which precedes and follows sleep is characterized by great suggestibility. An individual in that condition will give verbal assent (sometimes more than verbal) to any proposition, no matter how absurd, provided the drowsy condition does not terminate. But if he objects, he is awake. He may promise to get up in 'just a moment,' but if he refuses, the hypnagogic state has come to an end. He may agree that the moon is made of green cheese, but if he objects to your nonsense, he is no longer very sleepy. Without going into the matter very deeply we may say that the drowsy individual perceives (hears) the language addressed to him mainly as a disturbance. The fact that the eyes of the sleepy individual close (frequently without his knowledge) and remain closed is itself a protective activity which the individual (usually) does not understand at the time he is engaged in it. Now when the hypnotized subject cannot open his eyes and encounters resistance, that resistance is composed of two components. In the first place he feels, but does not understand, that the behavior of opening his eyes is incompatible with the behavior of trying to sleep. In the second place he feels, but does not understand, that he prefers to keep his eyes closed. The phenomenon is the resultant of the fusion of two different and distinct activities. It should be noted also that the activity of trying to sleep is social behavior inasmuch as it is the form which coöperation takes. It is a response to the hypnotizer. But the activity of feeling drowsy is not social in character. I shall call it organic behavior and this distinction will be developed as we proceed.

Now at the beginning of the induction of hypnosis the wide-awake *S* engages in the activity (described in detail above) which he '*calls*' trying to sleep. At that time I called it by the same name merely emphasizing that that is the form which coöperation takes. When the *S* begins to feel drowsy that activity integrates with the former activity. At the same time the *S* ceases to realize what he is doing. We must appreciate that realizing what one is doing is an activity which is not necessarily an integral part of what one is doing. Indeed, when the hypnotized *S* feels very drowsy his condition differs from ordinary sleep only in that, with the activity of sleeping, there is integrated the activity which, up to now, we called trying to sleep. If such an *S* is left to his own devices he will cease trying to sleep and will merely sleep (normally.) Accordingly, I wish to emphasize now that trying to sleep is trying to coöperate and that the drowsy *S* is not in a condition in which he is able to engage in the activity of making fine distinctions between different forms of coöperation. Accordingly when the hypnotized *S* tries to open his eyes and encounters resistance, I prefer to say that this resistance is his drowsy effort to coöperate, instead of saying that it is his effort to sleep. Coöperating drowsily is the behavior of the 'organism as a whole.' The organism is not engaged in two activities, but in a single one which is the resultant of the integration of two activities.

Let us consider another phenomenon. The drowsy, coöperative, but unanalytical *S* is told that his arm or hand is getting rigid. It does. I tell him that he cannot close the hand and, if that does not produce a visible response, I tell him to try. Let us suppose that the suggestion succeeds, *i.e.* that muscular contractions ensue which look like unsuccessful efforts to close the hand. After hypnosis, some *S*s report that they could not close the hand, others that they seemed unable to try very hard, and still others say that they did not really try; they were not sure that I really wanted them to. With some of the latter I tried out a procedure of which the following case is typical. I pretended to be indignant with the individual because he had faked results, and assured him very emphatically that I did want him to try. At the same time I emphasized again that he must follow directions and must never stop trying to sleep until he had succeeded in falling asleep. The experiment was repeated and, so far as one could judge by appearances, the results were unchanged, *i.e.* the hand became rigid and the *S* seemed unable to close it. After hypnosis he reported that he was convinced now that he could not close his hand at the same time that he was trying to sleep, but he felt that he could stop trying to sleep at any time he chose. I proposed to him that he was to consider himself free to cease these efforts when I told him that he could not close his hand, but that he was to follow the old instructions up to that point, that is until after his hand had grown rigid. But when the experiment was repeated, the hand did not become rigid, although I used all the tricks in my repertory to bring about rigidity. Afterwards the *S* said that he did not seem to be able to give his entire attention to trying to sleep in the way he had done before. He said that from time to time he became afraid that I had succeeded in producing the same phenomena as before without his knowledge and that, consequently, his 'trying to sleep' was a broken and fitful performance. In other words, the *S* had understood from the beginning the facts which I am trying to put before you, but his understanding was extremely vague; as it became clearer, the phenomenon (of the rigid hand) disappeared. So much for the inability to close the hand. The maintenance of rigidity had become a part of the behavior of trying to sleep. As for the production of rigidity, if the respectful attention of a wide-awake *S* is secured, it will be found that he responds to the frequently repeated suggestion that his hand is getting rigid in a great variety of ways; subvocal, oculomotor, frequent changes in the tonus of the forearm, and the like. These, the so-called motor accompaniments of attention, are some of the ways in which a normal wide-awake *S* attends to the quasi anticipated phenomenon. In support I cite the following class experiment which I perform habitually. The class is told that the success of the experiment depends upon undivided attention and the members are warned that that will be somewhat difficult as the experiment is monotonous. Then I tell them for about 5 min. that their right hand is getting rigid. In a few cases that actually happens. As soon as the experiment is apparently abandoned I ask how many feel that their right arm feels tired and as if it had been under considerable strain. Almost the entire class agrees invariably. In the case of the drowsy, coöperating, unanalytical *S*, the subvocal and, perhaps, the oculomotor responses tend to drop out, and the coöperative attitude of the *S* exerts a selective influence on the changes of tonus. Relaxing is *felt* (not thought) to be non-coöperative, so that tensing is the only method of attending to the quasi-expected phenomenon left to the *S*. Accordingly the hand becomes rigid. The behavior of making it rigid *is* the behavior of testing out the truth of the assertion of the hypnotizer. Besides we must remember that the *S* is drowsy and, obviously, the chief purpose of the responses which a drowsy individual makes to attempts at conversation is to keep the disturbance at a minimum. Tensing the hand is combined more easily with the behavior of trying to sleep than relaxing it. I should like to say also that I am not attempting to show that the acceptance of suggestions *necessarily* results from 'trying to sleep' and being drowsy. Suggestions sometimes fail. Success depends upon the details of the behavior of coöperating.

It should be interesting to compare this account of the phenomenon with one involving the idea of dissociation. I quote McDougall, "The patient will fail to achieve a forbidden movement—because, when he tries to make the movement and succeeds in innervating the proper muscles, the antagonistic muscles come into play and prevent the movement. At this stage, then, there is manifested a certain splitting of the personality, a conflict of one part against another: the muscles of one set obey the one part, the conscious willing subject; the antagonistic muscles obey some other part of the personality, which understands and is subservient to the commands and suggestions of the operator. This simple experimental evidence of division of the personality is of the first importance; for just such division is the very essence of many functional disorders."[3] My explanation of the phenomenon is that it is a quasi-mechanical aspect of the behavior of the subject and that the subject does not understand these quasi-mechanical aspects. If McDougall, instead of preaching that behavior is purposive, had attempted to understand the purpose or meaning of the specific behavior which he was observing, he would not need to speak of *muscles which obey the will of a dissociated personality*, nor would there have been any need of elaborate assumptions about the condition of the nervous system which *explains* dissociation.

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[3] William McDougall, *Outline of Abnormal Psychology*, 1926, 87.

As the hypnotizer continues working with the *S*, the latter becomes more and more adapted to the situation. His behavior is to be thought of less and less as 'trying to sleep' and more and more as maintaining a coöperative attitude in which drowsiness and respect for the operator are important factors. The point throughout is not only that the *S* maintains this attitude, but that he understands only in a very dim, vague way what he is doing. (In deep hypnosis I should say he does not understand it at all.) In such a situation the acceptance of a suggestion is as a rule the path of least resistance. If a great authority tells us what is what, the easiest thing to do is to agree—with mental reservations. The hypnotized *S* omits the mental reservations, because he is too sleepy, too lazy, too indifferent to make them; and, most important of all, if a mental reservation should begin to stir, the *S* finds that he is blocked by an unknown incomprehensible force, just as he does when he tries to open his eyes. That force is his feeling that he is not coöperating—but the *S* does not understand that. Accordingly, when you tell him that he is at the North Pole and ask him to describe the scenery, he begins to talk about polar bears.

In the last paragraph the critical reader will have noted that in preparing the ground for the explanation of a different kind of phenomenon, viz. hallucination, there occurs a shift of emphasis in the description of the behavior. In explaining phenomena such as the inability to move the rigid hand, I have emphasized the fact that the individual is trying to sleep. It was not necessary to assume that the individual is drowsy and more or less unaware of the fact that he is trying to sleep and that, consequently, the individual, who is not engaged in the activity of making fine discriminations, may be tricked into exceeding his original instructions in which coöperation was defined as 'trying to sleep.' As we proceed, we shall find that individuals differ enormously in the extent to which they may be tricked. It will be best to begin with an account of the cases where suggestion fails. Let us suppose that I have succeeded in suggesting rigidity of the hand and inability to open the eyes. Next I tell the *S* that he is at the North Pole and ask him to describe the scenery. The *S* does not reply. I repeat my request several times. The lips move, the facial muscles work, and the individual seems to be trying to speak, but utters no sound. Now if I keep insisting on a reply, the *S* is likely to open his eyes, declare that he saw nothing, and that he was not asleep anyway. At the same time he remembers about the inability to move his arm or open his eyes and is unable to account for these phenomena. If asked, "Why did your lips move without speaking?" he is likely to reply that he found it difficult to speak.

(Certainly it looks that way in the majority of cases.) He is unable to account for this difficulty. I account for the failure by assuming that the *S* was not sufficiently drowsy—or not sufficiently stupid, or not sufficiently friendly, or not sufficiently filled with mystic awe—to suppress a critical reaction. Although the critical reactions are not suppressed entirely, it seems obvious, however, that they encounter a resistance which the individual does not understand. In cases where the suggestion (of an hallucination) succeeds, I assume that the critical tendency is weaker and the resistance stronger. Consequently the suggestion has about the same effect as the request to imagine himself at the North Pole (which of course is what the *S* actually does). Indeed, after I have failed with the suggestion, "You are at the North Pole," I have sometimes resorted to the request "Imagine yourself (*e.g.*) in Kansas City" and have succeeded. In such cases the *S* frequently reports afterwards that the experience had reality and is not to be confused with wide-awake imaginations. In short, in order to produce an hypnotic hallucination the hypnotist must work to arouse the tendency to imagine the desired experience and to weaken or abolish critical reactions. As hypnosis is usually conducted, the operator will be aided enormously by the fact that critical reactions encounter a resistance which the *S* does not understand. When the hypnotist does not understand that either, he will of course adopt one of the current theories of hypnosis.

With reference to hallucinations and illusions, the question may be raised whether the *S*s really see the hallucinations and illusions. It may be that we are dealing merely with a verbal reaction. That is to say, the *S* may be describing the scenery at the North Pole without actually seeing it. The problem involved here is extremely difficult. Meantime some of the facts may be of interest. The suggestion of an hallucination sometimes fails. In such cases, as stated above, when the *S* is asked what he sees, he will, as a rule, not answer at all. If an answer is insisted on, he may cease to coöperate, open his eyes, and declare he saw nothing. In other cases he may continue to coöperate but say that he saw nothing. On the other hand, if an *S* describes an hallucinatory experience and if, after hypnosis, memory is restored, the *S* invariably reports that he saw what he described. Moreover, it is possible sometimes successfully to suggest hallucinations to *S*s who do not accept any other suggestions. In such cases amnesia does not occur and the experience is remembered as a dream-like vision. In the case of illusions, the facts are more complex. For example, the *S*, whose eyes are closed, is told to play with the kitten and is handed a handkerchief. Perhaps he strokes the handkerchief. If told to put the kitten on the floor, he places the handkerchief on the floor. But after hypnosis he remembers clearly that he manipulated something that might have been a handkerchief and that was not a kitten. In such cases there is rarely an amnesia for the incident. In a few cases the *S*s say that they acted as they did because they thought I wanted them to, but in most cases they do not seem to have a clear conception why they acted as they did. They say it seemed to be 'all right,' or that there did not seem to be any reason why they should not, or the like. It seems to me that these are the cases where Janet's characterization of 'facile complacency' comes nearest to hitting the mark. Nevertheless if an *S* of this type is asked (during hypnosis) to describe the color and appearance of the kitten, the *S* will answer after a struggle, that he does not see a kitten, after the fashion of the incidents described above. Apparently there are limits to 'facile complacency' and it is not quite the same thing as deliberate simulation. On the other hand, if the *S* describes the color and appearance of the kitten during hypnosis, *it is an invariable fact* (in my experience) that he will declare after hypnosis that he really saw and felt it. As a rule, there is amnesia for an incident of the latter type and memory has to be restored by special methods. It seems to me that these *S*s are coöperating without having a clear idea where coöperation ends and simulation begins. My *S*s seem to draw the line at verbal simulation, but of course it does not follow that all *S*s do so. I incline to the belief that the testimony of my *S*s is entitled to the same credence as regards the visual character of their experiences as if they were reporting hypnagogic hallucinations.

In the case of color illusions the facts are somewhat similar. *S* is told, for example, that he will be shown a blue book. Instead he is shown a red book and asked what color it is. The suggestion may fail; but I have never had an *S* who said, in hypnosis, that the book was blue, tell me, after hypnosis, that it was red. As a rule, there is amnesia and, after memory is restored, some *S*s remember clearly that it was blue. Others say that it was not a very good blue, but looked kind of blue. One *S*, on being shown the book, tried to speak but was unable to do so for several seconds. Finally he said, "It's blue—blue." The tone of voice indicated (to me) relief and an effort to contradict any doubt of the blueness. After hypnosis there was complete amnesia, but when memory was restored he said that the book looked red at first and then turned blue. It seems to me that we have here two possibilities and no more. Either the *S* actually succeeded in seeing a blue book, or else he continued to see a red book but reacted to the total environment not only by telling me that it was blue but succeeded in convincing himself (as indicated by the tone of voice). The first alternative is perfectly possible in spite of the fact that its physiology is obscure. About all we can say is that the behavior of seeing a blue book (including the physiology of that behavior) occurred as a response to the total environment. I might add that the physiology of a 'subconscious' perception of a red book is equally obscure. The second alternative is possible also and opens up the possibility of 'tapping the subconscious' by means of automatic writing. In my opinion both alternatives occur actually and I hope to be able to test this opinion by means of experiment in the near future.

In the case of negative hallucinations we have, fortunately, a more complex problem. For example, *S* is told that Mr. X has left the room; that he will be unable to see Mr. X. Then he is told to count a group of three individuals one of whom is Mr. X. He counts two and ignores the presence of Mr. X in every possible way. When memory is restored, after hypnosis, he recalls that the group looked peculiar, but is unable to characterize that peculiarity more closely. It has been pointed out frequently that the negative hallucination implies some sort of positive reaction to the stimulus; somehow, and in some sense, the individual knows what not to see. The problem here is very similar to the one which confronted us in the case of positive hallucinations and illusions. Does *S* succeed in convincing himself that he does not see X in spite of the fact that he does see him, or does he react to an environment containing X by seeing *a peculiar group?* In this case, however, further discussion may be more profitable because, whereas we know practically nothing about the concrete details of the simple behavior of seeing blue, we are more fortunately situated with reference to the complex behavior of seeing an acquaintance. Normally there occurs at such times the beginning of the act of greeting the friend. The head and body begin to orient themselves toward the stimulus, there is the incipient handshake or wave of the hand, the beginning of a smile, the initiation of a verbal greeting, the beginning of being pleased (visceral components), and the like. These various activities, probably, do not arise with absolute simultaneity, but are initiated in rapid succession. We do not perceive these activities, but we perceive what these activities mean. We do not *feel* that we have begun to speak, but we *see* a to-be-spoken-to-individual. Furthermore, so long as that sort of activities is maintained, so long do we see that sort of an individual. The question is, did such activities occur in the situation in which *S* reported as having seen a peculiar group? In reply, I have no desire to be dogmatic. Doubtless the details vary from individual to individual, but I maintain that the following account is essentially correct and does actually occur at times. Some of the salutatory activities just mentioned become initiated and, if they developed and if that were all that occurred, they develop into the behavior of perceiving Mr. X. But, just as the meaning of his activities is beginning to dawn, just as he is beginning to perceive Mr. X, an avoiding reaction supervenes. *S* does not wish to see what he has begun to see. The details of the avoiding reaction might be the incipient behavior of closing the eyes, turning away, and the like. The salutatory reaction is suppressed. It is not repressed into the subconscious. It is annihilated. It does not occur. Mr. X is not perceived. The avoiding reaction is maintained throughout. Consequently, what the individual perceives is a situation with a problematic aspect. The other members of the group are seen clearly enough to be counted, but the group as a whole is *peculiar*. Furthermore, this peculiarity is not nearly as interesting to *S* as it is to us. If it were, the negative hallucination would not occur. The peculiarity is satisfactory and satisfying. It is perceived as a not-to-be-inquired-into-aspect of the situation. It seems to me that such an account of the negative hallucination is more satisfactory and more intelligible than the facile assumption of unconscious or subconscious perception.

We are confronted with similar problems in the case of cutaneous anaesthesia and analgesia. The *Ss* act and talk as if they did not feel contact or pain. If acting in that way is taken to be at least a part of the behavior of feeling pain, it becomes obvious that the response to the environment becomes profoundly modified in hypnosis. The question remains whether any part of the behavior of feeling contact or pain persists. In the case of contact we have the evidence furnished by automatic writing and through the report of so-called co-conscious personalities that it does, at least sometimes. But in case of stimuli which would ordinarily result in severe pain I am not familiar with reports of the subconscious or co-conscious persistence of pain. On the contrary, it is stated in the literature that even the so-called physiological indices of pain, such as the dilation of the pupil, are absent in cases of profound analgesia.[4] Accordingly, we may conclude that the behavior of feeling pain does not occur. The question remains, what sort of a response *is* made to the environmental stimulation. In order to sharpen the problem, I shall define pain as an unsuccessful avoiding reaction. Now then, if the hypnotized individual does not attempt to avoid or modify the lacerated condition of tissue, for example, *what does he do?* Surely, some sort of 'physiological' processes are initiated by the laceration. Can we succeed in discovering the meaning of that behavior? I think that my experience with the induction of these phenomena furnishes us at least a hint. At the beginning of my experiments I had little difficulty in inducing all of the phenomena described so far, but I had no success whatever in the induction of anaesthesia and analgesia. This fortunate incident, which was due probably to the character of my *S*s, had the effect of directing my attention to the search for favorable conditions. To be brief, I found, if the arm of the *S* was placed in a horizontal position and left in that position for about 15 min., that anaesthesia and analgesia would supervene spontaneously, without suggestion from me. Not only did the arm of the *S*s seem to be insensitive to contact, pinches, and pin pricks, but the *S*s declared themsleves ignorant of the position and even of the very existence of the arm (absence of localizing activity). Of course, the failure to feel the painful fatigue sensations which ordinarily supervene is itself an analgesia. Now, at the beginning of our discussion of hypnosis, we accounted for the maintenance of the position of the arm by saying that it was a part of the behavior of coöperating by trying to sleep (maintenance of relative immobility). Distractions of all sorts which would result in movement under other circumstances result in increased tension instead. In the same way, instead of the behavior of feeling pain, we have an increase of coöperative tension. This account of hypnotic analgesia agrees with the widely known facts of emotional analgesia when, *e.g.* during the heat of battle, an injury which would ordinarily be painful is not felt at all. The response to the injury is a reenforcement of the on-going activity. The behavior of feeling pain does not occur. There is no pain.

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[4] It is well known that the dilation occurs also in fear. It seems to me that the absence of fear in hypnosis has not been considered by previous authors.

We come now to the problem of amnesia. Before beginning its discussion it will be necessary to enlarge upon the topics of social behavior and self-conscious behavior touched upon briefly above. In *Behavior and Conscious Behavior*, I defined the social act as an unfinished act which meant its own completion, and as an act which, as I put it at that time, had a double meaning or reference.[5] In the *Problem of Meaning in Behaviorism*, I added that all unfinished acts, non-social as well as social, meant their own completion.[6] That left as the differentia of the social act, its double meaning. I knew of course that the activity of picking up a hammer to drive a nail refers directly to the hammer and indirectly to the nail, just as the social act of asking another to open the window refers directly to the other and indirectly to the window, but at that time I was not prepared to meet the logical difficulty and social behavior was left without a well-defined differentia. In order to meet the difficulty I shall have to remind the reader that we are dealing with the behavior of the organism as a whole. Considered in that way, the social activity of the organism is never purely social; it has a social aspect. I shall state this position first in a series of formal definitions and shall then develop it into a brief genetic account. In meaningful behavior the organism means to modify an experienced situation. If and so far as the behavior is social, the organism means to modify an experienced situation in which it perceives the meaningful behavior of another to be a factor. (The organism need not perceive correctly.) If and so far as the behavior is self-conscious, the organism means to modify an experienced situation in which it perceives its own meaningful behavior to be a factor. In other words, social and self-conscious behavior are not different kinds but different aspects of meaningful behavior. These aspects are not merely logical; they are psychological—or else logic is without psychological foundations, and psychology devoid of logic.

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[5] *Op. cit.*, 210, 212. [6] *Op. cit.*, 242.

Racially and ontogenetically regarded, social and self-conscious behavior have developed as aspects of organic and pre-social activity. The infant perceives neither social nor non-social objects because it does not perceive objects at all. The problem of describing what such behavior means, is perhaps the most obscure problem which confronts the psychologist. There is, for example, the famous solution of William James who said that the world of an infant must be a 'great, big, buzzing, blooming confusion.' Meantime the infant does not show the slightest indication of being confused. One might say that the meaning of infantile behavior is vague, but it does not seem to be possible to believe that the infant experiences a vaguely significant situation because that would seem to presuppose a contrast with experiences the significance of which is clearer. It seems to me that what infantile behavior means is neither vague nor confused, but is described best by the term *undeveloped*. This undeveloped character is represented best by very general terms such as pleasant and unpleasant, or satisfying and non-satisfying. Withdrawal and approach seem to me to be accidental consequences of organic infantile behavior; I cannot believe that the very young infant means to withdraw or approach. That is a later development as localizing behavior patterns become organized somewhat definitely. I would define the activity of experiencing satisfying experience as behavior which *means* to attain more of that kind of experience and the activity of experiencing dissatisfying experience as behavior which *means* to attain less of it. These very general terms imply, of course, a great deal of internal variety and variability. On account of the undeveloped character of the behavior, it is possible, however, to describe this internal variety only by means of reference to stages of development which have not as yet been attained. Thus the infant responds to an environmental circumstance such as a loud sound, not by hearing a sound, but by feeling distress. Out of this matrix the behavior of hearing sound develops. We may gain some acquaintance with the concrete nature of such experiences by recalling instances when we were on the verge of 'losing consciousness.' When we are very sleepy, when we are sick and dizzy, when we are suffering intolerable pain, the highly organized world fades out and is replaced by an undeveloped world of organic experience. At such times the organism reverts to infantile modes of behavior. In Freudian terms, we regress to infantile or even to intra-uterine experience. At any rate, out of such a matrix of organic activity there develops the behavior of perceiving *things or objects* more or less clearly demarcated from the organismic background. Demarcation develops as far as it does develop in any given instance. It is seen, heard, felt, or briefly, it is perceived as clearly as it is perceived. I am interested but slightly in the abstract statement that an absolutely perfect demarcation is unattainable. Presumably most people will regard the maximal clearness which they have achieved as perfection. But such perfection *is* an achievement and is preceded, developmentally, by lesser degrees of perfection.

As the activity of perceiving *things* is developing, it develops of course in a variety of ways. That is, we perceive different kinds of things. First of all the infant cries at situations. Later the cry comes to be focalized upon things or objects. Very gradually we learn not to cry at some objects and that negative form of learning continues through life. To be brief, we learn to react to human beings and animals in ways which differ typically from our reactions to sticks and stones. That is, we acquire social and non-social behavior. The detail and the degree of this differentiation varies with the organism, the stage of development, and with circumstances.[7] Accordingly it is impossible to define social and non-social behavior by complete enumeration. The attempt could result only in an arbitrary revision of the dictionary. The genetic transition from pre-social to social and non-social behavior is gradual and there is no exact point at which one ends and the other begins. In other words, considering the wide-awake human adult, the behavior of the organism-as-a-whole is never merely social or merely self-conscious, but in stating what such behavior means we are able to distinguish organic, pre-social, social, non-social, and self-conscious aspects.

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[7] After things have begun to emerge from the organismic background and before differentiation between different kinds of things has progressed very far, it might be advantageous to distinguish a pre-social stage.

I acknowledge freely that it is not always possible to discover what a given form of behavior means. I acknowledge that it is not always easy to determine whether a given aspect of behavior is social; but, inasmuch as such forms of behavior are not utilized in my account of hypnosis, these admissions do not weaken that account. In this article the term social is applied only to activities about the social nature of which there can be no reasonable doubt. Coöperation is social, and the fact that the meaning of the coöperative behavior is undeveloped (vague) is the fault of the facts and not of the description of the facts.

We may pause a moment for the double purpose of illustrating social, pre-social, and organic behavior and of reading these terms back into the account of hypnosis given so far. Thus, in the case of individuals who maintained a horizontal position of an arm for a long time we noted that the arm became anaesthetic, and that the individual lost all sense of the position and existence of the arm. We urged that the response to the 'fatigued' condition of the muscles is a reënforcement of the coöperative activity. That aspect of the behavior is social. But, just as the organic 'experiencing' of the infant has internal variety, so the social behavior of the hypnotized individual has internal variety which varies with the condition of the organism. Just as the behavior of hearing sound is undeveloped in the case of the infant, so the activity of feeling fatigue in the arm is undeveloped. The individual regresses to an infantile mode of behavior. Moreover that form of organic behavior is not particularly abnormal. The internal variety of much of our behavior is described best by saying that we feel tense or relaxed, comfortable or uncomfortable, or the like. Further development of such forms of behavior takes place comparatively rarely. Again, in the case of the negative hallucination described above, the *S* responded to a human being by perceiving him as a peculiar aspect of a social situation. Clearly, that is an organic aspect of social behavior. If he had perceived him as a chair (and such complications happen sometimes), the behavior would have been non-social. Or the undeveloped organic aspect of the activity, which may be described as the perceiving of the peculiarity of the situation, may develop into a non-social aspect; that is, an aspect in which the experience of an object but not of the meaningful behavior and attitude of that object occurs.

We may return now to the problem of amnesia. The facts of amnesia are the stronghold of the theory of dissociation. At best, this theory is not an explanatory theory. As an account of a contracture or paralysis it does not possess even descriptive value. But when a large number of real and imaginary experiences cannot be remembered in the normal condition and can be remembered in hypnosis, the term dissociation seems to have considerable descriptive force. Explanation, however, must consist in making the dissociating process intelligible. It is this problem which we shall attack.

Post-hypnotic amnesia is a matter of degree, varying from almost normal memory to so-called complete amnesia. Moreover, it is usually possible to restore 'lost' memories in one way or another. Accordingly we must explain why the subject remembers what he does remember, why he forgets what he does forget, and the technique of restoring memory. Now, fortunately, 'normal' remembering and forgetting are not so very different from the hypnotic variety. For example, I have read aloud to my sick father without having the faintest notion of what I was reading. At such times I am quite unable to 'remember' what I have read for the reason just indicated, viz. that I never knew. When we are reading a book, the activity is of course our own, but we do not perceive our own activity. What we perceive is very much like seeing (and hearing) the speech of another. Our reaction to what we see (*i.e.* to our own activity) is very much like our reaction to the speech of another. We are interested or indifferent, amused or bored, and the like. Our memories of what we have read are very much like our memories of what we have heard. We remember what we reacted to. We remember that the speech was interesting or the reverse, we remember a jest, a happy phrase, and the like. In reading to my father, I remember raising and dropping my voice in accordance with the punctuation, noting my failures, attending more closely to the business of reading adequately. I do not remember, however, the thought of the passages, because in the social situation which I created by reading the passages and reacting to them there was no thought—of the kind ordinarily referred to when we speak of the thought of a printed paragraph.

It makes little difference whether we call reading a form of self-conscious behavior. The essential thing is that a social situation is created through the activities of a single organism. Even in instances where the thoughts which I think are clearly 'my' thoughts, I cannot find that all of my thoughts are of equal interest and importance—to me. However important such material may be to the psychoanalyst, however important it may seem to me when I am in a psychoanalytic frame of mind, there are times when my thoughts seem very unimportant and are forgotten promptly.

Much of so-called post-hypnotic amnesia is of this character. Some of the facts are not forgotten at all; they were not known in the first place. Other facts were known, but seemed very uninteresting and unimportant and are forgotten on that account. Thus the individual does not forget that his arm was in an horizontal position for a long time. He never knew it, any more than I knew a moment ago that I was sitting cross-legged. The 'horizontal position' and the 'cross-leggedness' were perceived as undeveloped organismic aspects, not as separate facts. In the same way, in the case where the individual cannot close his rigid hand, if he perceives nothing but a slightly disagreeable tension vaguely localized, he will have difficulty in remembering that, but if this tension develops into the perception of an interesting fact, that is more likely to be remembered. Considerations of this sort have some force even in accounting for the forgetting of suggested hallucinations. Thus if the *S* is told that he is at the north pole and asked to describe what he sees, he may display very little spontaneity. He may say that he sees snow and ice, and stop. Asked for further details he gives one or two every time he is prodded. He does not deplore the poverty of his imagination nor does he try to make up for his lack of spontaneity by increased effort. Even in cases where the hallucination is more spontaneous and richer in detail, it is, as I think, a legitimate assumption that what the *S* talks about and, perhaps, visualizes is not nearly as interesting to him as it is to the spectators and to the hypnotizer.

Ignorance and lack of interest, however, do not suffice as an explanation of amnesia—not without further elaboration. In the first place these uninteresting memories can be restored. In cases where the hypnotized *S* engages in animated discussion, or when an exciting hallucination is suggested, the hypothesis of lack of interest may seem inadequate. The reader will recall that the behavior of coöperating is the maintenance of an attitude. That attitude is neither physical nor mental; it is a conscious, meaningful attitude. It is a way of experiencing experiences. Just as the enemy-to-be-run-away-from *looks* different from the enemy-to-be-attacked, so the hypnotic experience is different from the non-hypnotic variety. It is a commonplace that experience has a focus and a fringe—a figure and a background. In ordinary experience visual and auditory aspects are prominent in the background, and organic and kinaesthetic aspects are less prominent.[8] But when our attention becomes riveted tensely upon the person of an enemy in anger or in fear, the background becomes less visual and more organismic—we experience a tense situation.[9] In hypnotic experience also visual and auditory aspects are, I believe, less prominent in the background and *tension* is more prominent. That is, every hypnotic situation has a peculiar, characteristic aspect—it is a tense situation. Now I have not become converted to the position that meaning is kinaesthesis. I maintain that experiencing-a-tense-situation is a unitary act. Tension is not always coöperation, nor is coöperation always tension, but tension is the form which coöperation takes in hypnosis. In other words, the *S* responds to the suggestion of the hypnotizer by experiencing an hallucination—for the hypnotizer. Now, when amnesia supervenes, it is a fact that 'memory can be restored' by means of a partial restoration of the coöperative behavior. I conclude on the basis of this evidence that this characteristic, tense, coöperative aspect is the supremely interesting aspect of hypnotic experience. The *S* is not interested in the uninteresting fact that he is talking about the north pole (and, perhaps, visualizing it) because he is too intensely interested in visualizing and believing it; and, vice versa, when amnesia is more or less incomplete, I conclude upon the basis of the evidence that the *S* was more or less interested in the more or less interesting fact that he experienced an unusual experience under unusual circumstances.

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[8] The presence of this organic and kinaesthetic aspect is vouched for by the 'pressury' part of the 'pressury gray' of our 'trained introspectionists.'

[9] The shrinking of peripheral vision in hysteria can be connected with facts of this character.

Once more. When I was a boy I had numerous fights and while fighting I saw the enemy clearly and I did not see much else. These things are easy to remember because at the time of the occurrences I had adequate insight into the relevance of what I saw. I was engaged in a meaningful activity which had not been completed and the meaningful behavior of the other boy was supremely interesting. After the activity had been completed, I had adequate insight into what I had done and its relation to the rest of my social life. But when the hypnotized *S* hallucinates, his insight into the relevance of what he sees is practically nil, because he does not understand what he means to do (*i.e.* that he means to coöperate by trying to sleep) and is doing it too tensely to inquire into the relevance of what he sees. The *S* is told, "You are at the north pole." His critical reactions, if any, are suppressed. He looks, *i.e.* he visualizes (or believes); but he is too interested in visualizing (or believing) to inquire into the relevance of what he sees (or believes). In order to remember, he must first be given an opportunity to become interested in what he has seen or believed.

The amnesic *S* who is asked to recollect what happened while he was asleep is asked to look for he knows not what. He does not know what to look for and he does not know how to look. If you are asked to remember what your experiences were on August 2, 1923, you have something to start with. The amnesic *S* has nothing. If, however, we reinstate the activity which developed into the behavior of talking about the north pole in the first place, the repetition of that behavior is greatly facilitated—but mere repetition is not memory. The *S* who repeats his former performance exactly does not remember; he is re-hypnotized. In order to make it possible for the *S* to react to his own behavior in a way in which he did not react to it in the first place, we must avoid making the repetition too complete.

Now for our evidence. Bernheim has demonstrated in cases of apparently complete amnesia, that it is possible to restore memory by browbeating and cajoling the *S* until he does remember. I have repeated these experiments with similar results. But, *in cases of apparently complete amnesia,* I have found also that when these *S*s begin to remember, they are practically as suggestible as they were in hypnosis. Tell them that their hand is getting rigid—it does. Tell them they are asleep and they are re-hypnotized. These same *S*s may require several minutes of tedious preparation to hypnotize them the following day. While I am cajoling and browbeating, these *S*s try earnestly to do what I want them to do. They are coöperating. Now after cajoling and browbeating have been continued for 15 or 20 min., it begins to become somewhat monotonous. After all, the method has some points of similarity with other methods of inducing hypnosis. Partly on account of this similarity, partly by accident, coöperation takes the form which it took during hypnosis.

As additonal evidence I select the following cases. *S* is hypnotized, his left hand is made rigid, and he describes in considerable detail two hallucinations which are suggested to him. Next he is told that his left hand will become rigid after he has awakened when he is asked, "What is your name?" but that he will take no notice of the fact. He is awakened and asked for an account of what he remembers. Amnesia is almost complete. After considerable urging he remembers that there was something about the law building and something about a movie, but that is all. He is asked, "What is your name?" He replies and his left hand becomes rigid. Asked again to try to remember what happened during hypnosis, he describes the two hallucinations in almost the same words which he had used during hypnosis. *S* says, regarding these replies, that it is as if he were reading them out of a book. That is, a partial restoration of the attitude which he maintained while telling me about the hallucinations during hypnosis enables him not only to tell me about them again, but also to listen to himself and to understand what he himself is saying. That the partially restored attitude is coöperative is easily demonstrated. As soon as memory has been established, the *S* is told that his eyes are closing and that he is asleep again. The eyes close and the *S* accepts two other suggestions. He is told to wake up and to remember everything. He does so, and afterwards the two hallucinations are remembered normally.

To cite another case, one *S*'s post-hypnotic memories are vague, dreamlike, and very incomplete. He had been given the post-hypnotic suggestion that he was to answer 'Jack Dempsey' to the question 'Who is your roommate?' The suggestion failed and, in spite of considerable urging, the *S* did not remember having received any instructions. The sleep method induction had been used. In order to restore memory, I instructed the *S* to stare fixedly into my eye and, when he was beginning to get tense, I asked quietly, 'Who is your roommate?' He began to answer "Jack—," stopped, laughed, and remembered the whole incident completely and clearly. In this case, the *S* 'remembered' his instructions—that is, while he was maintaining an attitude similar to the one he had been maintaining when the original instructions were given, he began to reply in accordance with these instructions, and then, reacting critically to his own behavior, he grasped the meaning of the situation in which the response began to occur.

The way *S*s do and do not remember about their behavior during the execution of a post-hypnotic suggestion is very instructive. In some cases they re-enter the hypnotic state spontaneously, must be reawakened, and exhibit complete amnesia afterwards. Less extreme cases which exhibit a distinct tendency in this direction vary enormously in detail. But the following case is typical. The post-hypnotic suggestion had been that the subject was to answer "Denver" to the question, "Where are you going this afternoon?" The suggestion succeeded and the *S* did not seem to know that he had said anything odd or absurd. To the question, "What train are you going to take?" he replied, "The Santa Fe." The question, "What are you going to do after you get there?" seemed to puzzle him a little, but after a few moments he replied, "Hunt for a job."[10] I have observed a number of similar cases. As a rule hypnosis can be reëstablished simply by telling the *S* that he is asleep. But if, after execution of the post-hypnotic suggestion, the *S* is engaged in ordinary conversation, he will, as a rule, return to his normal condition. If, after normalcy is judged to have returned, he is asked about the trip to Denver, he may or may not remember the absurd statements he has made. But, if he remembers, he does not as a rule become aware of their absurdity until after he has remembered them. He may say that he does not know why he said what he did, or that something made him say it, or that he did it to please me. These are some of the attitudes *S*s take toward their own behavior after it has occurred. Perhaps they are elaborations of the attitudes taken originally in hypnosis. But if the original behavior was addressed to me, and if the *S* did not take any kind of an attitude toward his own behavior, it becomes difficult for him to remember what he has never known.

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[10] The reader will note that these replies are congruous with the original suggestion. The *S* is coöperating.

When an individual in the normal condition points his finger at an object it is not necesssary for that pointing activity to be that object. Moreover, such an activity is genetically and typically a social activity—the individual points out the object to another. When an individual talks about an experience it is not necessary for the talking activity to be that experience, nor need he be reëxperiencing the experience at the time he is talking about it. There is no need of a 'mental' representative of the experience; the speaking organism *re-presents* it to another and, usually, to himself. The speech activity, with or without 'visual' or 'auditory' components, *is* the re-presentation. However displeasing these facts may be, however unscientific it may be to mention them, the facts themselves can be verified by the observation of any one who has not been trained too thoroughly in a method based on antiquated and discarded metaphysics. (And, somehow, I cannot convince myself that it is the essence of scientific method to ignore observable fact.) Now in post-hypnotic amnesia the amnesic individual cannot re-present a former experience to another nor to himself. Originally the activity of experiencing that experience was an integral component of the behavior of coöperating with the hypnotizer in the peculiar, characteristic fashion described above. On the positive side, it is possible to enable the subject to re-present the experience by reinstating, in part, the peculiar, characteristic activity which made it possible for the *S* to *present* the experience to the hypnotizer in the first place. On the negative side, re-presentation in any other way is difficult or impossible because the activity of relating the experience to other experiences did not occur in the first place. Concretely, in the north pole hallucination, in spite of the fact that the hypnotized individual was re-presenting a scene which he saw at a moving picture theater (as brought out by subsequent investigation), the individual did not remember having seen the 'movie,' nor did the north pole experience seem strange or familiar; it just was. Similarly, the fact that he was talking about the north pole, and possibly seeing it, did not seem strange nor was it related to former experiences in any other way; when it is remembered, it is remembered as having happened—that is all. 'Dissociated hypnotic experiences' dwell in the same limbo as last winter's snows. But the activity of re-presenting these experiences is difficult, because it is connected in a peculiar, difficult, and unusual fashion with the person of the hypnotizer.

SUMMARY

By way of summary let me repeat that the phenomena of hypnosis are resultants of the meaningful behavior of the individual in ways which that individual does not understand. I have not dismissed this lack of understanding as a brute fact, but I have studied it. Upon study it has resolved itself into an undeveloped aspect of meaningful activity. The meaning characteristic of all forms of hypnotic behavior is meaning to coöperate with the hypnotizer. This article consists of an exposition of the concrete detail without which these general statements are vague and useless. In view of that it seems to me that additional summarizing can do no good. Some readers will find it possible to misunderstand the article without artificial aid.

These general principles have the widest possible application. In politics, religion, business, and in 'mental' disease, in all walks and aspects of human life, the undeveloped aspects of meaningful behavior are profoundly significant. The thing to do is to go to the facts and work out the details. If we do, it will become superfluous to study the attempts of Watsonians to 'behave' as if they were scientists; and we may join Freud in his efforts to translate his epoch-making intuitions into intelligible language.

When this article was begun, it was my intention to write on hysteria also and that is the reason why the article was not published two years ago. The account of hysteria which I have worked out thus far is analogous to the present account of hypnosis. The phenomena of hysteria are resultants of the meaningful behavior of the individual in ways which the individual does not understand. In hysteria the important behavior is the activity of emoting. In the face of the protean character of hysteria I have, however, found the working out of the details so difficult a task that I have decided to publish this study of hypnosis which is, in a sense, complete.

Finally, it seems to me that many of the facts reported in the literature need additional detail in order to be interpreted. When Braid reports that a person who could not lift 10 lbs. in his normal state could swing 50 lbs. around his head with one finger in hypnosis, one wonders what the 'normal' state was like. Obviously, many of these cases were hysterical, though the authors neglected to mention that, if they knew it. Again, the relation of physician and patient is a peculiar one and there is no limit to the complications which may arise. Janet has shown that Charcot's subjects were, virtually, trained performers. When hypnotic anaesthesia is used as an aid to surgery, the coöperative motive is quite unlike that of a student who volunteers to be a subject. Hope, fear, love, jealousy, and hate, are powerful *motifs*. Sometimes an author hints delicately at facts which might lead us to suspect that they might have played a part. Sometimes he says nothing. In my own work I have taken note of the emotion of hope which animates the soul of the student who means to 'work' the instructor for a grade; and I have noted other minor emotions. My account, however, attempts to explain only what I have observed, not what I have read.

PSYCHOLOGICAL EFFECTS OF FASTING

By John Arthur Glaze, Colorado College

The Effects of Prolonged Fasting

Problem. It was our purpose in the experiments here reported to study the psychological effects of relatively prolonged fasting. Three healthy adults, two men and one woman, went without food for from 10 to 33 days. Tests of several varieties were made at regular intervals before, during, and following the fasting periods.[1]

Earlier work on prolonged fasting.[2] One of the well-known fasts of some length was that of Succi, reported by Luigi Luciani.[3] During his 30-day fast Succi was observed only casually for effects upon mental processes. Other results, however, show that he made decisive improvement in tests of strength. His firm belief in the efficacy of fasting may have affected the results. Luciani doubted Succi's sanity and had little faith in other than the purely physical results.

One of the recent experiments on fasting was reported by Marsh.[4] This experimenter and his wife fasted for one week. The week preceding the fast they gradually reduced the amount of food taken, and the week following reversed this procedure. Short tests were used. The data secured during the control period were discarded for some unexplained reason. This period covered six days of testing, and should have been most significant for purposes of comparison.

The results secured in this experiment were not very definite. The experimenter thought there was some improvement in mental clearness and accuracy for both subjects. His conclusions about sex-differences have no value in view of the fact that only two subjects were used in the experiment.

The outstanding psychological experiment upon a long fast is that performed by Langfeld.[5] The subject was Agostino Levanzin, a lawyer from Malta, who believed a fast markedly increases mental acuity. He claimed to have pleaded a case in the courts of law after having fasted for 26 days.

Levanzin came to this country for the express purpose of demonstrating the beneficial effects of fasting upon mental processes. He entered the Boston Nutrition Laboratory of the Carnegie Institute of Washington and underwent a 31-day fast there. Eight tests were given daily during the experiment, all comparatively short ones. Unfortunately, the experimenter had no opportunity to run a long series of preliminary tests. The follow-up tests had to be discontinued because the subject withdrew from the laboratory after developing a colic. The ailment was attributed to the nature of the food taken immediately after concluding the fast.

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[1] This study was carried out in the Psychological Laboratory of the University of Chicago under the direction of Professor Edward S. Robinson.

[2] Experiments on short fasts are discussed at the end of this paper.

[3] Luigi Luciani, *Das Hungern*, tr. by M. O. Fraenkel, 1890.

[4] H. D. Marsh, Individual and sex differences brought out by fasting, *Psychol. Rev.*, 23, 1916, 437-445.

[5] H. S. Langfeld, On the psycho-physiology of a prolonged fast, *Psychol. Monog.*, 16, 1914, No. 71.

A year after the fast it was possible to test Levanzin again, but of course the time-interval involved was too great for the tests to throw any light on the process of recovery.

Levanzin showed a loss in muscular strength during the fast. Strength, as tested by the hand dynamometer, fluctuated from day to day, but showed a gradual falling off as the fast progressed. There was a gain in those tests involving the 'central processes'—by this phrase are meant tests of attention, perception, and association. Association and rote memory tests were also used, and several others. There was a decided gain in visual acuity as the fast progressed. At the end of the fast, Levanzin could detect the letter E, the largest one of the Snellen test card, in its correct position from twice the original distance.

The effects of practice in this experiment will never be clearly known. That there were practice effects is not denied. The most decisive facts brought out by the experiment were that muscular strength showed a falling off, and that the ability to improve mental operations through practice was not entirely arrested by the fast.

The lack of preliminary testing and a 'follow-up,' continuing for several days after a fast, has been a weakness of all previous experiments.[6]

Subjects. Three people were used as *Ss* in our experiment. They will be designated *A*, *B*, and *C*.

A is the writer's wife. She is a college graduate, and was engaged in the domestic duties of her household during the experiment. She did not prepare food for the family during the first few days of the fast, but found no difficulty in doing so after that. She fasted for 10 days.

A had undergone short fasts of a few days' duration several years previous to the present experiment, while attending college. She believed short fasts benefited her health, but she was not sure that they helped her mentally. She had never gone over 4 days without food before. Her previous fasts had been endured to get rid of very painful attacks of neuralgia.

She began the practice period on April 8, 1925, and was tested daily throughout the experiment. On May 18, 40 days after the first test, she began the fast. Testing continued during the fast as before. On the fifth day of the fast she developed a severe headache which continued for two days. She was in perfect health, as shown by physical and metabolic tests, preceding the fast. This was the only ill-health she experienced during the experiment. On May 27, after taking the tests, she concluded the fast. For 23 days after the fast, *i.e.* until June 19, testing continued. The entire testing period was 77 days in length. Further details are given below.

B, the writer, was a graduate student at the time of the experiment. He was pronounced in good health by an examining physician. Besides giving 5 hours daily, on an average, to the experiment, he carried two majors in the University—a course in experimental psychology and an advanced course in neurology.

B had never fasted before. He had no definite opinion about its mental and physical effects. He had read many claims about its rejuvenating effects and was interested in finding out what the results would be.

He began the practice period on April 25, 1925, and continued it for 32 days, or until May 27, the beginning of the fast. The fast was prolonged for 17 days, up to June 13. Testing was carried on daily throughout the entire experiment. The post-fast testing continued for 33 days, until and including July 15. The entire testing period was 82 days in length.

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[6] Langfeld, himself, realised this deficiency in connection with his own study, but he was faced with circumstances which he was powerless to correct.

The health of *B* was good during the fast except for a slight headache on the tenth day. A physical examination on the fifteenth day of his fast showed him to be in excellent condition, except, of course, for the loss of flesh.

C is a man about 35 years of age. (All *S*s were approximately the same age.) He had attained a secondary school education and is a lay scientist. He had fasted over a total of 300 days, previous to the present experiment, often in the interests of observing physiological effects. He had never taken psychological tests during a fast. He fasted 33 days for the present experiment. The longest fast that *C* had endured heretofore was one for 26 days. He had not undergone any fasts of over a few days length for several months previous to this experiment.

C believed that a fast improves one's health but doubted whether the results are worth the effort and inconvenience. He was quite certain that the "rational element," as he expressed it, is neither helped nor harmed by a long fast. He lived several miles from the psychological laboratory, and had to go back and forth from his home to take the tests. Fortunately, at this time, he was not employed in any occupation, and consequently could give considerable time to the experiment.

C began the practice period April 13, and was tested approximately every other day during the 110 days of the experiment. In only two instances were testing days less than a day apart. The greatest interval was two days, and it occurred several times. *C* took the tests 48 days out of the 110.

He was pronounced in good health at the beginning of the fast by a physician. On the 28th day of the fast he was given a cursory examination by a physician and was pronounced in good physical condition, despite rather extreme emaciation. Perhaps because he was accustomed to fasting, *C* found it unnecessary to take the twice-daily enema that the other *S*s did as a matter of precaution. No biliousness nor other serious difficulty was encountered throughout his fast.

Nature of the tests. Seven tests in all were used during these experiments. They were selected with a view to making each task as difficult as possible. Where the task was not a difficult one the time-limit of performance was increased. None of the tests was of a purely muscular nature. Several were decidedly fatiguing. The reason will be evident in the following discussion.

Robinson and Herrman[7] have shown that 3 *S*s were able to perform in tests about as well after 60–65 hrs. of insomnia as they did normally. These experimenters tested for an interval from 11–26 days preceding the sleepless period. They followed the period of sleeplessness with 4 or 5 days of testing. Relatively short tests were used; the total time for five tests took from forty-five minutes to an hour. Inasmuch as the *S*s doubtless put forth more effort during the period of insomnia, it was thought that we might partially offset this possibility by using tests that were rather fatiguing.

In another experiment on sleep Robinson and Robinson[8] secured from their *S*s ratings of the amount of effort that they felt they put forth in taking the tests. There were two groups, an insomnia and a control group. The experiment covered a period of three days. On the first day at the end of the tests, 41% of the insomnia group felt that they had put forth their best effort. The night following was a sleepless one for the insomnia group. At the end of the tests, on the day after the sleepless night, 75% of them, as against 38% of the control group, felt that they had put forth their best effort. The insomnia group during the next 24 hours made up much of their lost sleep. On the third day only 49% of them, compared with 17% of the control group, felt they put forth their best effort during the tests.

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[7] E. S. Robinson and S. O. Herrman, Effects of loss of sleep, *J. Exper. Psychol.*, 5, 1922, 19-32.

[8] E. S. Robinson and F. R. Robinson, *ibid.*, 5, 1922, 93-100.

These experimenters say: "The ratings of effort, considered in conjunction with those of tiredness, suggest that loss of sleep causes a general lowering of feeling tone accompanied by an increase of effort put forth upon a task. This increase of effort may so compensate for any ultimate loss in capacity that that loss may be relatively unobservable."

Further conviction along this line follows from the experiments of Kleitman[9] on the effects of prolonged sleeplessness. His *Ss* went from 40 to 115 hours without sleep. He found no appreciable loss in the speed in naming 100 colors on a card. But when one *S* named 1200 colors (12 cards), Kleitman did obtain definite effects of insomnia.

After extensive investigation of various tests the following were selected.

(1) *Arithmetic.* Four hundred multiplication problems of two by two-place numbers were arranged from chance combinations of the digits 2–9 inclusive. Each two problems contained all of the digits. No digit occurred twice in the same problem. It was hoped that this would make the daily task fairly homogeneous. The problems containing the combinations 25 were eliminated because they suggested short-cuts. In a day's task no identical problems occurred. *A* solved 20 problems daily, and *B*, 40.

The multiplicand was first called out by *E*. It was repeated and written down by the *S*. In the same manner the multiplier was given, whereupon *E* immediately gave the signal, "Go." *S* gave the answer as quickly as possible without further writing. The correct answer was required. Time and errors were observed and recorded.

C thought he would be "forever" working off the effects of practice with so difficult a task. Consequently, and at his request, we constructed on the same principle 200 addition problems of 4x4-place dimensions. He performed 40 of these each day. The procedure was the same as for *A* and *B*, except that *C* wrote down the answers as he obtained them.

In this, as in the three tests that follow, *S* sat opposite *E* at a wide table. Between them was a screen, about 2 x 3 ft., supported on legs approximately 4 in. above the table top. It permitted *E* to observe what *S* wrote, or his general method of attacking a test. What *E* recorded was never observed by the *S*. Emotional distractions, due to the knowledge that one is being closely observed, are much more apt to be prevented by employing this simple device.

(2) *Writing.* The letters "ab" were written on uniform ruled paper for 20 min. as rapidly as possible. The *S* made a stroke on the line at the sound of a tap on the table every 30 sec. From the data obtained in this manner it was possible to construct fatigue curves for each of the periods. The errors were few enough to be negligible after several days of practice.

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[9] N. Kleitman, The effects of prolonged sleeplessness in man, *Amer. J. Physiol.*, 66, 1923, 67-92; 67, 1923, 141-151.

The results consisted of the number of letters written during the 20 min. of the test. This type of activity had previously been used in connection with another investigation in the Chicago Laboratory.[10]

(3) *Color-naming.* Twelve cards containing 100 colors each were prepared. The colors were one cm. square and placed one cm. apart on the cards. Green, red, yellow, black, and blue were the colors used, 20 of each to a card and in random order. The cards were presented face downward to *S* who turned them over and named the colors as rapidly as possible. The cards were not of equal difficulty; some of the combinations proved easier than others. For this reason and due to the fact that we presented the cards in regular order every day, we could not construct representative fatigue curves for this test. We recorded the time that it took to name the 100 colors on each card, however. Errors consisted in skipping colors or misnaming them. (Although we recorded errors, we shall not present them with our results; they seem not to have been influenced by the fast.) In case *S* corrected himself *E* made note of the fact by a symbol. This test was adapted from Kleitman's experiment on sleep.

(4) *Backward reading.* This test was suggested by the Robinson and Herrman experiment on insomnia, to which reference has already been made. Patrick and Gilbert were apparently its originators. Three magazine articles each for *A* and *B* were taken from a current periodical and mounted on cards. Each article was of 12-point type, 1000 letters in length. Each was read every third day.[11] *S* began at the bottom of the article and named from right to left the letters as they appeared in the words. He advanced to the top of the article by taking each successive line upwards. Results consisted of the time to read the article. Errors of both the corrected and uncorrected type were also recorded, but they seem to have been unimportant. Because several of the tests were trying on his eyes, *C* asked to be excused from taking this test.

(5) *Steadiness.* The device described in Whipple's *Manual*[12] was used to measure the changes in steadiness that took place. *S* was required to hold the pointer for 30 sec. each in the 3rd, 5th, 7th, and 9th holes of the metal plate. These holes were 16, 11, 9, and 7 sixty-fourths of an inch in diameter. The needle pointer was of standard design. The interval of rest between the pointings was 30 sec. Both hands were tested. The results consisted of the errors made in touching the walls of the metal plate with the pointer. These were recorded automatically by electrical connection with a clock-counter. A strong current was used, sufficient to 'carry through' with the least contact, but of insufficient intensity to injure the mechanism of the clock. To decrease noise the clock was embedded in a box of sawdust. Each click was reduced to a dull thud.

S was seated at a table in a comfortable manner, with body tilted slightly forward. He was not permitted to hold his elbow against his side. When he was in position a switch was thrown at the signal 'go,' completing the circuit. In the interval of rest *S* always held the pointer in the hand that was being tested. Otherwise he disposed of his time as he desired.

(6) *Visual acuity.* In lieu of some standard test on visual acuity that fulfilled the requirements of this experiment, the following was constructed. Five rows each of 12-, 10-, 8-, 6-, and 5-point type, 25 letters to a row, were printed on a card. Letters that looked somewhat alike appeared next to each other in a mixed fashion. For instance, the letters P B R and O D G C appeared in different patterns, unspaced and in capitals, along with

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[10] E. S. Robinson and A. G. Bills, Two factors in the work decrement, *J. Exper. Psychol.*, 9, 1926, 415-443.

[11] I cannot commend this procedure for eliminating the effects of practice. Only one card should have been used.

[12] G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1914, 155-160.

most of the other letters of the alphabet. The purpose was to have a test of some length where fine visual discrimination could be measured.

This card was placed under a light in a dark-room. A reflector shielded the light, permitting no glare to the *S*s vision. A rheostat was inserted to cut down the current. A translucent white light ('milk glass') of 50-candle-power, using 16/50 amperes, was employed. *S* sat with his head against a head-rest; his eyes were 42 in. from the card. This distance and the amount of current used were determined by the available space in the dark-room. *E*, with the aid of a flashlight behind the curtain, checked *S*'s performance on a duplicate of the test.

S was asked to read the letters as steadily as possible, taking only the second, third, and fourth lines of each size of type.[13] The test began with the largest type and proceeded downward, including the smallest type; *S* read from left to right. He indicated by a short gutteral "umph" a letter that he could not identify, and proceeded to the next. *E*, with the aid of a long handle, opened and closed a small door covering the card, when necessary, during the test. A few seconds were taken between readings on each size of type to record time and omissions. *E* was never visible to *S* during the test.

This test required some time to prepare, and as a result did not get the benefits of a long practice period prior to the fasts. It was completed and used for the first time about 10 days before the first fast. Without anticipating our findings, we may say that the results secured were decisive, in view of the post-fast performances of all *S*s in this test.

(7) *Equilibrium.* This test is another measure of steadiness, in this instance of the ability to stand at 'attention.' A device known as the ataxiagraph, mentioned and illustrated by Doll,[14] was constructed. It consisted of a glass cylinder fastened upright to a head-band. A delicate spring was enclosed within the cylinder. From the end of the spring a stylus projected through a small hole in the stoppered cylinder.

This 'crown' was buckled on the head, the stylus pointing upward from the forehead. A fixed support held a metal plate upon which sagged rather loosely a smoked paper. When *S* was placed in position this plate was lowered on a standard until the stylus touched the smoked paper. It thus recorded the movements of the head or body by a tiny mark upon the smoked surface.

The position of the *S* was approximately at 'attention.' The feet formed an angle of 45–50°, the heels were together, and the body comfortably erect. The arms extended downward by the sides, palms inward. *S* stood in this position for 2 min. blindfolded and 2 min. with vision. Between the conditions there was a rest interval of 2 min. during which the *S* was seated. The order of the conditions was alternated from day to day.

The extreme distances from left to right and back and forth over which the stylus moved constituted the measurements for this test. These measurements were taken in mm. by the use of a needle-point divider, the spread being read from a steel scale.

General procedure. Preceding the experiment every *S* agreed that he would do his best very day throughout the entire experiment and endeavor to keep an open mind as to the outcome. No *S* was permitted to know his own performances from day to day on any test.

A and *B* performed the first four tests, namely, arithmetic, writing, color naming, and backward reading, in their home. They acted as *S* and *E* in turn for each other in all tests except that of visual acuity. In order to make this possible duplicate forms of some of the tests were constructed.

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[13] Thus a line was always immediately above and below the one being read, a fact which would not have been true if the first and last lines had been read.

[14] E. A. Doll, *Clinical Studies in Feeblemindedness*, 1917, facing p. 66.

Different materials were used for backward reading. While *S* read the material backward, E followed, using a copy on which the material was read forward. *E*'s copy was in large type and the words were not spaced, so that it looked to the *E* like a pied paragraph.

These 2 *S*s used the same arithmetic problems, but rarely solved the same set from the 400 on any given day. Whenever this did happen, as it was bound to when *A* solved 20 and *B* solved 40 problems each day, it was rarely, if ever, noticed by the person acting as *S* after having acted first as *E*. These *S*s never memorized the answers to any problems.

For the color-naming test the names of the colors were typed in large print for the benefit of *E*. This made it easy to follow *S*'s reactions.

It can be seen that these three tests are probably the only ones wherein *S* could have any advantage by having acted as *E*. We believe that the precautions explained above reduced to a minimum, if indeed they did not entirely eliminate, any advantage that might accrue to one acting the rôles of both *S* and *E*.

The writer always acted as *E* for *C* except in the test of visual acuity. A graduate student in psychology, Miss Marian Geohegan, to whom we are greatly indebted, acted the part of *E* in this test. All tests except the four given at home, were given in the Psychological Laboratory.

The results secured during the practice period were graphed by the *E*. The four major tests, arithmetic, writing, color-naming and backward reading were the ones especially observed. The fast was not begun in any case until the *S* had apparently reached a level of performance and remained there for several days. Dr. E. S. Robinson advised *E* as to the time when the fast should begin. The experiments were thus subjected to better control by having an experienced referee who was not a subject.

As a matter of convenience the results from testing *C* are graphed as if the tests were given every other day. As we have explained previously, only 48 out of the 110 days of the experiment were testing days, so this is not quite true.

RESULTS

(1) *General tendency to lose.* In the four major tests there was a general tendency for the *S*s to 'perform' at a lower level of efficiency during the period of fasting. This tendency was more conspicuous in the case of the longest fast, but was generally characteristic of the shorter ones as well.

Figs. 1-5 show the performances of the 3 *S*s throughout the experiment. In estimating the effects of the fast the early portions of the graphs can be ignored; these sections of the curves illustrate mainly the marked influence of practice upon the activities in question. When, hereafter, we make reference to the pre-fast performance, we are considering the relatively flat sections of the curves just preceding the fast. The cross-bar designates the point at which we assume practice to have become a minor factor. The broken portion of each curve represents performance during the fast.

Fig. 1 shows that *C* lost considerably in the time required to perform 40 addition problems when enduring the fast in comparison with the pre-fast and post-fast performances. For about 15 days he performed as well during the pre-fast period, but during the next 10 days his rate of performance was definitely decreased. During the last week of the fast he returned about to the level of the pre-fast performance.

The curves for *A* and *B* show little more than daily fluctuations in performance during the fast. Especially is this true in comparison with the pre-fast section of the curves. The stimulating effects of food after a fast, indicated by most of the tests, will be discussed in a later section.

Fig. 2 shows that both *A* and *C* suffered a distinct loss during the fast in ability to write, as measured by the number of letters written. After the first 2 days of fasting the output of these two *S*s gradually decreased. On the last 2 days of the fast *A* showed a tendency to recover some of the losses sustained. Toward the close of the fast *B* suffered a most marked loss of output. His strong physique doubtless was responsible for his ability to perform at a high level for almost 2 weeks during the fast.

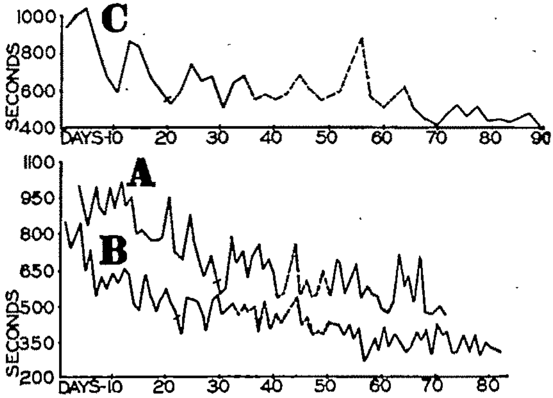

Fig. 1. Arithmetic. Performances of *A*, *B*, and *C*, before, during and after a 10-, 17-, and 33-day fast, respectively. Pre-fast begins with black bar; fast is in dotted line

Fig. 3 shows very clearly that *C* took a much longer time to name 1200 colors during his fast than he did previous to it or afterward. The differences between the pre-fast performances and those during the fast are much more marked in his case than they are for *A* and *B*. The after-fast acceleration again was great with this *S*. The only conspicuous result shown by the curve for *B* is the after-fast acceleration. The performances of *A* show little variation from normal.

No graphs are presented for backward reading. Of the two *S*s taking this test, *A* reacted more slowly during the fast than before or afterward, while *B* was apparently unaffected by the fast, though he did show a post-fast improvement. These facts are clearly enough shown in Tables I and II.

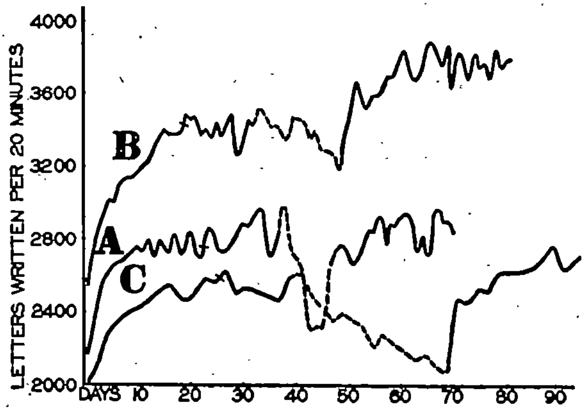

Fig. 2. Writing. *A*, *B*, and *C*, before, during, and after a 10-, 17-, and 33-day fast, respectively. Pre-fast begins at black bar; fast is broken line

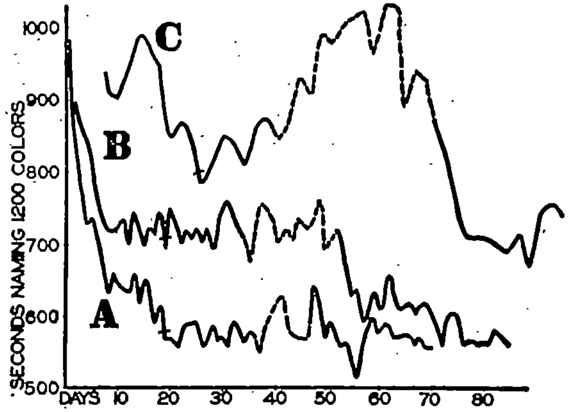

Fig. 3. Color Naming. Performances of *A*, *B*, and *C*, before, during, and after a 10-, 17-, and 33-day fast, respectively. Pre-fast begins at black bar; fast is broken line

Fig. 4 shows the relative time consumed in naming the letters in the visual acuity test. Every *S* proceeded at his own rate during this test. The graphs show very clearly that all *S*s took more time to perform during the fast than either before or afterward, in spite of the fact that the task performed after the fast was somewhat greater than that preceding or during the fast, *i.e.* the total number of letters read per test was greater after the fast.

Tables I, II, and III show the average performances of *A*, *B*, and *C* in all of the tests. The performances are grouped into the three main periods of experimentation—the pre-fast, the fast, and the post-fast. The pre-fast is that period before the fast but following the pronounced practice effects of the very early performances (see cross-bars on graphs). The averages show a general tendency of the *S*s to lose in efficiency during the fasts. The marked exceptions to this tendency will be given separate consideration.

TABLE I

PERFORMANCES OF *A* IN TESTS PRECEDING, DURING AND FOLLOWING A 10-DAY FAST.

Abbreviations: ′ = seconds; c = errors (contacts); mm = millimeters

Tests		Pre-fast	Med. Dev.	Fast	Med Dev.	Post-fast	Med. Dev.
1. Multiplication		675.0′	44.5	609.3	46.5	574.5	70.2
2. Writing (letters)		2813.4	68.6	2576.71	69.5	2834.3	84.7
3. Color-naming		575.3′	14.3	584.2	16.2	578.1	18.9
4. Backward reading		333.2′	6.2	364.7	15.5	309.3	18.3
5. Steadiness (hands)	Right	14.45c	6.4	13.8	5.38	11.52	6.5
	Left	15.55c	4.4	18.5	5.00	13.56	3.5
	Total	30.00c	6.0	32.3	12.88	25.08	10.1
6. Visual acuity	Letters	356.0	3.0	355.0	2.0	358.9	2.1
	Time	338.0′	8.0	394.0	34.5	316.4	32.6
7. Equilibrium Blindfold	L-R	37.7mm	2.3	34.4	9.0	38.9	8.9
	B-F	44.8mm	5.2	46.1	6.4	58.0	8.0
Seeing	L-R	30.5mm	9.5	29.8	3.8	30.9	6.9
	B-F	36.1mm	13.1	36.8	5.3	45.5	10.5
Totals	Blindfold	82.5mm	9.5	80.5	3.5	96.9	16.9
	Seeing	66.7mm	12.7	66.5	9.0	76.4	13.6

(2) *Increase in steadiness.* Fig. 5 shows graphically the results of the Whipple test of steadiness. The curves have different ordinates to prevent superposition but have the same abscissae. The results for both hands are combined in the interest of simplicity. Separate graphs of the results from testing the two hands showed no consistent differences between the performances of the right and of the left hands. The averages show only slight differences between the two hands, as can be seen from Tables I–III. The curves for *B* and *C* will be discussed first.

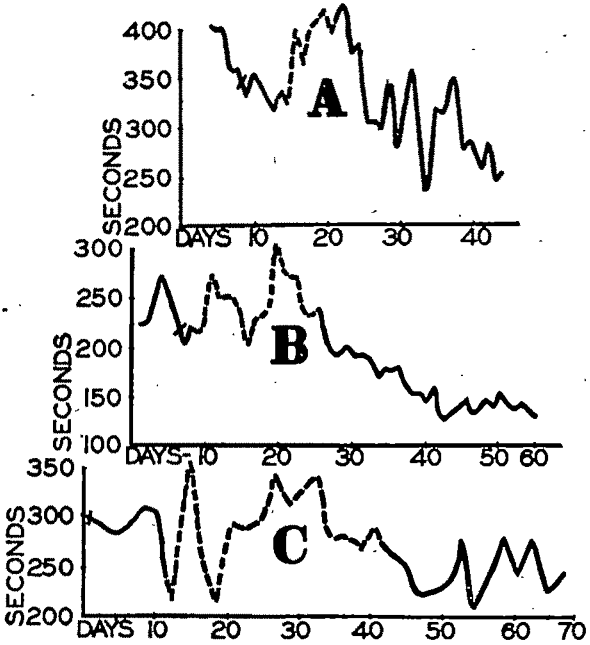

FIG. 4. VISUAL ACUITY. Performances of *A*, *B*, and *C*, before, during, and after a 10-, 17-, and 33-day fast, respectively. Pre-fast begins at black bar; fast is broken line

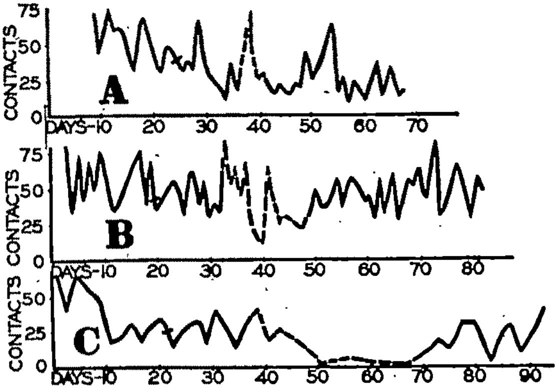

FIG. 5. STEADINESS. Performances of *A*, *B*, and *C*, before, during, and after a 10-, 17-, and 33-day fast, respectively. Pre-fast begins at black bar; fast is broken line

B and *C* show a gain in steadiness when the results during the fasts are compared with the other conditions. Due to the variations in the curve for *B* the gain is not so clearly shown as it is for *C*. The averages in Table II show that *B* did make a small gain during the fast in this test, though his gain is not statistically certain.

The gain of *C* is very conspicuously shown by the graph, Fig. 5. After the first few days (tests were given about every other day) he showed a steady increase approaching almost zero errors as the fast continued. He remained at this low level until the completion of the fast when, as can be seen by the curve, he became less steady. It was impossible to keep this *S* from knowing that he did better during the fast. Although *S* mentioned the fact several times during the fast, *E* never discussed the matter with him.

TABLE II

PERFORMANCES OF *B* IN TESTS PRECEDING, DURING AND FOLLOWING A 17-DAY FAST.

Abbreviations: ″ = seconds; c = errors (contacts); mm = millimeters

Tests		Pre-fast	Med. Dev.	Fast	Med. Dev.	Post-fast.	Med. Dev.
1. Multiplication		499.9″	26.9	459.9	44.1	369.9	38.9
2. Writing (letters)		3413.7	51.5	3380.9	82.9	3728.2	87.8
3. Color-naming		722.9″	15.9	724.7	17.3	610.6	14.4
4. Backward reading		264.5″	6.5	262.4	7.6	222.2	3.8
5. Steadiness (hands)	Right	25.2c	3.8	19.0	11.0	22.9	4.9
	Left	19.2c	5.8	23.1	8.1	24.5	5.5
	Total	44.4c	11.4	42.1	17.1	47.4	9.6
6. Visual acuity	Letters	374.4	0.4	373.4	1.6	374.9	0.1
	Time	213.7″	9.3	249.2	21.2	159.6	21.0
7. Equilibrium Blindfold	L-R	33.9mm	12.0	30.1	5.1	35.1	7.5
	B-F	34.1mm	5.1	40.2	8.8	44.2	8.0
Seeing	L-R	29.1mm	5.9	28.5	3.5	30.2	8.7
	B-F	32.7mm	3.7	38.2	10.2	39.3	7.3
Totals	Blindfold	68.0mm	13.5	71.0	11.0	79.2	11.5
	Seeing	61.9mm	8.8	66.7	14.3	69.6	14.6

Table III shows that *C* made on the average about 14 less errors during the fast than before it. When compared with the post-fast the errors are about 10 less. The corresponding gain for *B* (Table II) is not so marked.

Table I shows that *A*, who underwent the shortest fast, suffered a loss in steadiness during the fast. Taking the average of the first 7 days of fasting, every *S* showed a distinct loss over the two other conditions. Had we stopped the tests at the end of one week of fasting, and secured the same data for the post-fast performances of all 3 *Ss*, we could have said that the fast caused a marked falling off in steadiness for every one. The curves show that all *Ss* increased in steadiness after the first week of fasting, except for one bad day for *B*.

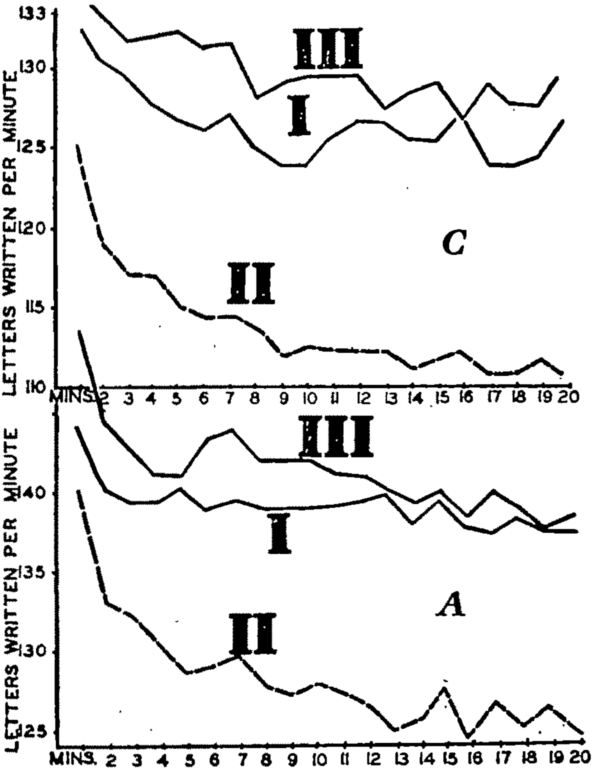

Fig. 6. Work-Curves. Curves of *A* and *C* for writing in the pre-fast (I), the fast (II), and the post-fast (III) periods

The other form of steadiness, *i.e.* equilibrium, was little affected by the fast. See Tables I-II.

(3) *Fatiguability.* Fig. 6 shows the work-curves of *A* for the writing tests. One curve is shown for every one of the three phases of the experiment. These curves show the general ten-

dency of *S* to become more fatigued as the day's task progressed. The pre-fast curve shows that he performed at a level considerably higher than that of the fast, but the important fact illustrated is the greater fatiguability during the fast. *A* began writing about 140 letters per min., on an average, and finished the day's task at the rate of 125 letters per min. During the pre-fast and post-fast the corresponding losses were a little more than half this amount.

The curves for *C*, Fig. 6, represent almost exactly the same thing, except for his tendency to perform at a little higher level during the last minute.

TABLE III

PERFORMANCES OF *C* IN TESTS PRECEDING, DURING AND FOLLOWING A 33-DAY FAST.

Abbreviations: ″ = seconds; c = errors (contacts); mm = millimeters

Tests			Pre-fast	Med. Dev.	Fast	Med. Dev.	Post-fast.	Med. Dev.
1. Addition			568.3″	37.2	559.5	40.5	451.3	19.3
2. Writing (letters)			2533.0	53.0	2306.9	127.1	2619.6	74.4
3. Color-naming			841.8″	28.3	975.3	66.3	742.5	30.5
4. Steadiness (hands)		Right	12.9c	7.0	5.0	4.0	9.2	3.8
		Left	11.8c	4.0	5.8	4.8	11.2	4.2
		Total	24.7c	8.0	10.8	8.8	20.4	7.4
5. Visual acuity		Letters	357.3	4.0	361.0	3.0	365.5	5.0
		Time	299.5″	9.5	290.0	25.0	247.1	19.1
6. Equilibrium	Blindfold	L-R	25.7mm	2.7	25.6	6.6	24.4	4.4
		B-F	41.1mm	1.9	32.9	6.1	32.5	4.5
	Seeing	L-R	22.4mm	5.4	22.5	5.5	20.2	4.2
		B-F	32.2mm	9.2	32.5	7.5	37.5	4.5
	Totals	Blindfold	66.8mm	4.2	58.5	9.5	56.9	4.9
		Seeing	54.6mm	10.6	55.0	11.0	57.7	3.7

The curves for *B*, which are not shown, indicate that his performances during the pre-fast and the fast went on at about the same level in this test. He showed less susceptibility to fatigue than the others.

(4) *After-fast acceleration.* With few exceptions, the after-fast performances were on a higher level of efficiency than either of the other conditions. This relationship is true of all tests except those in which a gain was made by the *Ss* during the fasts. It also applies more to *B* and *C*, who endured the long fasts, than to *A*, who fasted only 10 days. Although *A* showed a gain in the post-fast over the two other conditions in all but the

color-naming and equilibrium tests, her gains are not so outstanding as those of the two other *Ss*. The curves that most conspicuously show the acceleration of after-fast performances are Figs. 2 to 4.

It may be possible that the effects of practice are still operating to some extent in some of the tests. But Figs. 2, 3, and 4 seem to show an additional effect, especially for *B* and *C*. Fig. 2 shows that these *Ss* performed in writing almost immediately after the fasts at a higher level than ever before. Fig. 3 shows that these *Ss* began naming colors a few days after the resumption of food at a rate never before attained. *A*, and especially *B*, spent much less time in reading more letters, as shown by Fig. 4, immediately after the fast was concluded. These facts lead us to believe that there is a marked after-fast acceleration due to the stimulating effects of food.

Carlson[15] relates some of the after-effects of fasting for a few days at a time. Speaking of his physical and mental condition after a 5-day fast he says, "the writer felt as if he had had a month's vacation in the mountains. The mind was unusually clear and a larger amount of physical and mental work was accomplished without fatigue." All of our *Ss* spoke of the unusually heightened feelings they experienced during the after-fast period.

(5) *Decrease in weight.* Table IV shows the weights of the *Ss* at the beginning and at the close of the fasts.

TABLE IV

WEIGHT LOSSES DURING FAST

S	Weight		Loss		
	at beginning	at close	pounds	%	Pounds per day
A	121.3	109.1	12.2	10.1	1.22
B	166.3	144.1	22.2	13.4	1.31
C	148.7	118.6	30.1	20.3	0.91

Twenty-three days after the fast *A* had not regained her full weight. Since this was June 19, and she was only 4.7 lbs. underweight, it may be said that she was not far from normal. On the 19th day after concluding his fast *B* was 0.5 lbs. over-weight. *C* was 0.2 lbs. under-weight 29 days after concluding his fast, but since this was on July 29, almost mid-summer, it may be said that he was slightly over-weight.

CASUAL OBSERVATIONS

Increase in smell sensitivity. The most outstanding observation made by the *Ss* enduring these longer fasts, was the marked increase in the ability to detect odors during the fasts. Food, we believe, could be detected at a greater distance. *C* reported that perfumes and even body odors were keenly noticed by him when he met or passed people on the streets during his many walking trips while fasting. *B* refused to drink the city water after a few days of fasting because of the offensive odor of the chlorine in it, a fact rarely noticed at other times. (The office of the City Engineer informs me that a chlorine gas is used in the city water.) *B* purchased a supply of spring water to use for drinking purposes during the last 10 days of the fast.

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[15] A. J. Carlson, *Control of Hunger in Health and Disease*, 1916, 137.

Marsh hints at results of this nature when he says that taste and smell "were tested for two weeks and dropped, due to the delay entailed; but the results would have proven as interesting as any, had this section of our plan been completed."[16] Langfeld says that Levanzin called his attention to the fact that "heightened sensitivity for odors during his first fast made walking on the streets positively unpleasant."[17]

It is a well-known fact that hunters deny food to their dogs for several hours preceding a hunt. This is a common practice with fox hunters in a certain section of the South. The popular opinion is that the animals do better when the stomach is empty. Without denying the value of hunger as a stimulus to action, it seems probable that the animal's olfactory sense is much keener too. Thus the opinion that a hungry animal can follow a 'cold' trail better than when he is well-fed may, after all, have some basis in fact.

It was originally intended to secure some measure of smell sensitivity during the experiment, but it was difficult to get a test that met all of the requirements of the problem without over-loading the program. It is hoped that objective results will be obtained by some experimenter in the future on this apparent increase in sensitivity to odors.

Dreams. Dreams were prominent in only the longest fast. Only a few were reported by *A* and *B*. These were either very hazy and disconnected or were remotely related to food. *C* reported several dreams about food after three weeks of fasting, but none was very striking.

Sexual feelings. About a week after *C* had concluded his fast, the writer asked him about the effects of fasting on the sexual impulses. He substantiated what the writer had already observed, that food after a fast of some length tended to accelerate the activities of the sexual organs. *C* had noticed this, also, at the conclusion of previous fasts.

Someone has suggested that this condition is due to eating considerably more meat than ordinarily. It is quite possible that an increased meat diet will serve thus as a stimulant after a fast. It is more reasonable to assume that an increased sexual tone would naturally accompany the greatly increased body-tone that normally follows a fast. That the entire body-tone was heightened after the fast was reported by all 3 *S*s. This increase in sexual tone should also be thought of in relationship to the high mental efficiency shown by certain of the tests.

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[16] *Op. cit.*, 438.

[17] *Op. cit.*, 49.

Miscellaneous. The bodily temperature of *A* and *B* remained almost unchanged throughout the fasts. Respiration increased slightly. Both measurements were taken after 30 min. rest in a prone position. As *C* had endured several fasts it was thought unnecessary to observe him so carefully in these respects.

Every visit to the laboratory and the return home required the *Ss* to climb several flights of stairs. After about 10 days of fasting the climbing did not cause the limbs to ache as much as it did during the earlier part of the fasts.

Only one *S* was an occasional smoker. He smoked freely during the first two days of the fast, began to lose the taste for tobacco on the third, and gave it up completely on the fourth day.

CONCLUSIONS

The conclusions reached as a result of this experiment are as follows.

(1) The so-called 'mental' out-put is generally decreased during a long fast.

(2) A fast of more than a week's duration definitely increases steadiness of hand.

(3) A subject fatigues more rapidly during a fast than normally, when the task is of considerable length and homogeneity.

(4) Performance at some tasks after a long fast is much more efficient than normally, an effect which can hardly be attributed to practice alone.

(5) In casual observations it was noticed (a) that the smell sensitivity is greatly increased during a long fast; and (b) that sex feelings in males are considerably accentuated when eating is resumed after a long fast.

EFFECTS OF SHORT FASTS

In order to test the effects of short fasts *A* and *B* of the preceding experiment, and a new subject, *D*, a Chinese male graduate student, fasted one day out of every six. *A* and *B* fasted 7 times and *D* fasted 5. A period of about three weeks practice preceded the fasts. Arithmetic, writing, and the two tests of steadiness were used. These are identical with those used in the preceding experiment, and were given in the same way.

The results on the days of fasting were compared with those secured on the 3 days previous to and the 3 days following the days of fasting. Each fast was endured on a different day of the week.

No definite results were secured. On some fast days the *Ss* did better than on the pre-fast or post-fast days, and on others decidedly poorer. It was almost impossible for the *Ss* to maintain the same attitude toward the experiment on the day of the fast as on other days. This conscious factor undoubtedly did much toward obscuring the real results.

Wada[18] has shown a close relation between hunger contractions of the stomach and best performances in mental tests. She tested almost continuously for 10 to 12 hrs., after the *S*s had gone without food for the greater part of a day. Almost without exception, the best results accompanied hunger contractions of the stomach. Since our tests were all performed in about 1½ hrs. work a day it is readily seen where our difficulty lies. Although our *S*s were, in a general sense, hungry on the foodless days, we can have no assurance that there were hunger contractions during the actual test period.

We see no way of reconciling our negative results with the positive results obtained from short fasts by Weygandt and by Aschaffenberg.[19] Both of these students found short fasts to have markedly deleterious effects.

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[18] Tomi Wada, An experimental study of hunger in its relation to activity, *Arch. Psychol.* 8, 1922, No. 57.

[19] Cited by Langfeld, *op. cit.*, 47.

THE EFFECT OF BRIGHTNESS IN THE RANGE OF ATTENTION EXPERIMENT

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Introduction

From the point of view of structural psychology, sensation is to be defined in terms of its attributes. The attributes common to all sensations are quality, intensity, duration and clearness,[1] that these have independent status, is established by both logical and introspective analysis. The only possible confusion—that between intensity and clearness—is conclusively resolved by appeal to the familiar fact that attention to an intensively weak component of a sensation-complex brings it out clear and vivid in consciousness, without appreciably altering its relative intensity. So much is clear.

Whether independent status implies independent variability is a controversial question.[2] The ultimate answer to this problem can be arrived at only by appeal to actual experiment, supplemented by introspective report.

The object of this experiment is to arrive at a preliminary determination of the influence upon clearness produced by variation of stimulus-intensity. It were indeed a fond hope to expect anything more than a tentative and provisional answer to our question, based, as the results necessarily are, upon the highly specialized conditions of our experimental setting. Nevertheless, it is the writer's belief that conclusions, validly drawn from careful and accurate experimental results, cannot but throw some light upon this moot question.

Many ambiguous results have been obtained from experiments to determine whether or not intensity is increased by attention. With this phase we are not directly concerned in this study. Our principal concern is with a comparison of the clearness of perception resulting from objectively varied intensities of stimulus, and we have adopted, as our means of approach, the familiar problem of statistically calculated limens for visual apprehension of dots—the old so-called range of attention.

The writer is well aware of the possibility that the question may be raised as to the validity of his basic assumption, viz., that the limen values for apprehension of different stimulus-intensities afford a true measure-

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[1] Külpe, Dunlap, Warren, and others omit 'clearness' from the list of attributes of sensation and include 'extensity.' Külpe does not distinguish clearness from intensity; he says, "intensity itself is the property of sensation which enables us to compare it with others in respect of vividness." (*Outlines of Psychology*, 1895, 30.)

[2] Cf. E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 9 f.

ment of clearness of apprehension. The distinction and independent variability between attributive clearness and cognitive clearness, pointed out by Dallenbach,[3] are confirmed in this study. Despite this complication, however, we are convinced that our assumption is justified not alone by the insurmountable practical difficulty of maintaining constancy of attitude,[4] but also by the results themselves.

In regard to the problem of the relationship between intensity and clearness, Pillsbury states: "Whether the change in clearness is identical with, or dependent upon, change in intensity is as yet open to controversy."[5]

Titchener asks, "Is clearness ever independent of intensity?" and replies, "On the whole . . . the trend of psychological belief just now seems to favor an interdependence of the two attributes."[6]

The phenomenon of consciousness, in the large, has one fundamental characteristic: consciousness of something, whether it be sensation, image, feeling, idea, or perception. That is, there are always two levels of clearness to every conscious experience—a something which stands out and a background from which it stands out. This is the basis of the two-level concept of consciousness. The processes which stand out as relatively more vivid and distinct are called the focus, while the surrounding mass of unclear or obscure processes is termed the margin, fringe or background of consciousness. Whether more than one level of clearness exists in the background does not interest us here; but the possible existence of more than one clearness-level within the focal area is inextricably bound up with studies of the range of apprehension.

In the more recent tachistoscopic investigations of the so-called range of attention, just such distinction has been made between relative levels of clearness within the 'upper-level' or focal area of consciousness.

Fernberger[7] came to the same conclusion that had been reached earlier by both Whipple[8] and Dallenbach,[9] viz. that 'range of attention,' as applied in the older writings, was a misnomer and rightly should be called 'range of apprehension.' This conclusion was based on the observed fact that more than one degree of clearness was discriminated by *O*s within the focal consciousness.

From the structuralist point of view, there is general agreement that the term 'attention' is practically synonymous with maximal clearness,[10] the term 'apprehension'[11] is applied to lesser degrees of clearness found within the focal area, distinct from the definitely and sharply demarcated obscurity of the margin or background of consciousness.

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[3] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 229. N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 248.

[4] E. S. Rudisill, Constancy of attitude in weight perception, this JOURNAL, 36, 1925, 562-587.

[5] W. B. Pillsbury, *Attention*, 1908.

[6] E. B. Titchener, *A Text-Book of Psychology*, 1909, 212.

[7] S. W. Fernberger, A preliminary study of the range of visual apprehension, this JOURNAL, 32, 1921, 121-133.

[8] G. M. Whipple, *A Manual of Physical and Mental Tests*, I, 1924, 278.

[9] K. M. Dallenbach, The effect of practice upon visual apprehension in school children, *J. Educ. Psychol.*, 5, 1914, 321-334, 387-404; The effect of practice upon visual apprehension in the feebleminded, *ibid.*, 10, 1919, 61-82; especially, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 228 f.

[10] Titchener (*Text-Book of Psychology*, 267) states, "in the last resort, and in its simplest terms, attention is identical with sensory clearness." Breese (*Psychology*, 1917, 59) states, "Attention is clear and central consciousness." Pillsbury (*Attention*, 1908, 11) states, "Attention is fundamentally a change in clearness of some one phase or aspect of a mental process." Hunter (*General Psychology*, 1919, 114) says, "Attention is the clearness into and out of which objects move."

[11] Dallenbach, *opp. citt.*; Fernberger, *op. cit.*; Whipple, *op. cit.*; but see Dallenbach, Dr. Oberly on 'The range for visual attention, cognition and apprehension,' this JOURNAL, 36, 1925, 154-156.

Oberly,[12] starting with this distinction, added a third conscious pattern, which received introspective justification. The first process, which he designated as *Immediate*, was introspectively characterized by immediacy of apprehension, coupled with equal and maximal clearness of all elements; the second process, which he called *Grouped*, was similarly characterized by immediacy, but coupled with more than one level of clearness; the third process, which he called *Counted*, was marked by a temporal succession, due to a mediating process of re-imaging, coupled with more than one level of clearness. To these three conscious processes or patterns, Oberly applied the systematic categories: Attention, Cognition, and Apprehension, respectively.

In the present problem, the writer has adopted Oberly's method of procedure with but slight variation, believing it to be well adapted for application to the intensity-clearness problem. The necessary variation of stimulus-intensity was introduced by using three different brightnesses of dots (black, dark gray and light gray) on a white background; Oberly used only black on white. In an earlier experiment, Fernberger[13] had obtained intensity-differences by using black dots on a white ground, and black dots on a medium-gray ground.

As stated above, the purpose of this experiment was to determine whether or not any constant relation exists between stimulus-intensity and degree of perceptual clearness. We assumed that such relationship would appear in a comparison of the values of the limens and the shapes of the curves of the psychometric functions for the different stimulus-intensities and under the different patterns of conscious process. Such objective evidence was amplified by frequent introspections from the *Os*, in the hope that information might be obtained in addition to that afforded by the individual judgments of the stimuli.

EXPERIMENTS

Apparatus. The apparatus used in this experiment for exposing the stimulus material was a Whipple disk tachistoscope.[14] The adjustment of weights was regulated to give an exposure-period of 75 sigma, calibrated by the tuning-fork method. The pre- and post-exposure field was of uniform black, except for a white fixation mark at the center. The only illumination was supplied by a 25-watt lamp.

The stimuli consisted of 5 sets of white cards, 88 mm. sq.[15] on which were pasted in haphazard order dots, 6.5 mm. in diam., and ranging from 2 to 12 in number. There were 33 cards in each set, 11 of which had *black* dots, 11 *dark gray* and 11 *light gray*, each brightness group ranging from 2 to 12 dots. Thus there were in each set, 3 cards of each denomination: *one black*, *one dark gray*, and *one light gray*, all arranged in different haphazard orders. The arrangements were different for all five sets.[16]

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[12] H. S. Oberly, The range for visual attention, cognition and apprehension, this JOURNAL, 35, 1924, 332-352.

[13] Fernberger, *op. cit.*

[14] For a description of this apparatus see G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1924, 264.

[15] Hurlock's 8-ply Oil Crest illustrating board was used.

These stimuli will be referred to as Bk, D, and L, respectively. The contrast of these three brightnesses on the white background of the cards thus afforded *three intensities of stimulus.*

Method of procedure. The tachistoscope was set up on a low table and the *O* seated so that his eyes were 2 m. from the exposure field, which at this distance, lay entirely in foveal vision. Instructions, read to the *O* at the beginning of each observation period, were as follows:

"You will be shown successively, for a very short exposure, cards which contain a varying number of dots on a white background. Immediately after the exposure you will report verbally:

"(1) The number of dots which you have apprehended.

"(2) The degree of assurance or certainty of your judgment, using this scale for your report: 5—if it is a 100-to-1 bet, 4—if it is a 5-to-1 bet, 3—if it is a 1-to-1 bet, 2—if it is a 1-5 bet, 1—if it is a 'guess' or 'do not know.'

"(3) The method of determining your report, whether by *Immediate judgment*, *i.e.* ungrouped, by *Grouping* of dots, or by *Counting.*"

These instructions are the same as those used by Oberly,[17] except for the omission of the word 'black' before 'dots' in the first sentence.

Accepting Oberly's results and conclusions as our starting-point, we defined the methods of determining reports as follows: *Immediate*—where all the dots "were perceived in a single flash of consciousness, with an equal and high degree of clearness." *Grouped*—where immediate perception occurred, as above, "but owing to different degrees of clearness, there was an attentional grouping of the stimulus dots." (Concerning this characterization we shall have more to say later.) *Counted*—where "some subsequent re-imaging of the stimulus material" provided the basis of the report.

In the Results we have presented, under the category of Attention, limens and coefficients of precision calculated from the correct judgments, based on the Immediate method. Under Cognition, we have included measures based on the Immediate method plus the Grouped method, *i.e.* those showing more than one level of clearness. Under Apprehension, we include measures based on all three methods: *Immediate, Grouped,* and *Counted.*

As soon as the *O*'s gaze was directed toward the fixation-point of the pre-exposure field, the warning signal was given. This consisted of a 'Ready,' followed at an interval of approximately 1 sec. by a 'Now;' the catch was then released, which brought the exposure of the stimulus-card after the lapse of another second. Thus the actual stimulation followed the first signal at an interval of approximately 2 sec., allowing for maximal concentration of attention. The stimulus was exposed, as stated above, for a period of 75 sigma.

The *O*'s 3-fold judgment was recorded as soon as given; then the next card in order was exposed as before, preceded by the signal. In order to obviate, as far as possible, unconscious memorization of individual cards or their sequence, the 5 sets of cards were shown in order from I to V, the cards being placed in a given position, which was called the 'A' position. At the completion of the 5 series (165 cards) in this position, the cards were turned to the left through an angle of 90°, and all 5 series gone through in this 'B' position; then, similarly with the 'C' and 'D' positions. In this way, a given card was shown in a certain position only after the exposure of 659 other stimuli. Another variation was provided by starting at different points in the sequence of the individual set.

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[16] The papers from which these dots were cut were obtained from Milton Bradley Co., and are listed by them as Black, Neutral Gray No. 1 (Light), and Neutral Gray No. 2 (Dark). Matched on a color-mixer, they were found to contain the following: Neutral Gray No. 1—black 63%, white 35%, yellow 2% (this was our 'light'); Neutral Gray No. 2—black 73%, white 25%, yellow 2% (this was our 'dark'); Black—black 100%.

[17] Oberly, *op. cit.*

The experimental periods were so arranged that no *O* ever observed more than three days in one week, nor longer than one hour at a sitting. A rest period of 10 or 15 min. was always allowed after each 20 or 30 min. observation period.

Each of the *Os* was given a period of preliminary training to familiarize him thoroughly with the procedure, so that the complex judgment had become perfectly mechanized before recording was begun.

The first series of observations was begun in February, 1925, and completed in June, 1925. The second series began in October and ended in December of the same year.

Each series consisted of 10 exposures of each of the 5 sets (of 33 cards each). Thus every value of stimulus from 2 to 12 dots, for each of the 3 intensities (Black, Dark Gray, and Light Gray) was exposed 50 times in each series, making a total of 3300 observations for each *O*.

Observations were made, for the most part, in a photographic darkroom. Toward the latter part of the second series we were compelled to transfer our apparatus to a large store-room of the department. Although a small amount of light entered from a distant window, careful scrutiny of the results gave no evidence of any change other than the normal improvement incident to practice, which had been apparent from the beginning.

Observers. The four *Os*, all members of the staff, were: Samuel W. Fernberger (F), Henry Sherman Oberly (H), Earl S. Rudisill (R), and Malcolm K. Macdonald (M). F and M were highly trained in giving introspective reports on psychophysical observations: H and R were less experienced in this sort of work. All had participated in the Training Series and were, therefore, thoroughly familiar with the apparatus and method of the experiment.

Handling of results. The judgments included, as stated above, quantitative estimates of subjective assurance, on a 5-point scale; following Oberly's procedure, the writer omitted from calculation all judgments given with an assurance less than '3,' *i.e.* an assurance so low that an *O* would not back it with at least an 'even-money' bet was assumed to be too much in the nature of a 'guess' to be significant. It is to be noted that F, M and H were aware of the omission of the '2' and '1' judgments in Oberly's experiment. F, however, was the only one who knew positively that a similar procedure was to be followed in this experiment. The suspicion that it might be so may conceivably have influenced H and M to give '3' judgments in some cases where the natural inclination would have suggested a '2.' Even if this were so, it seems highly improbable that any material difference would have resulted, inasmuch as all three intensities of stimulus would presumably have been affected equally. R had no knowledge of the omissions, being unacquainted with Oberly's paper.

The tables of observed relative frequencies were subjected to statistical treatment in accordance with the phi-gamma hypothesis. Limens (S) and coefficients of precision (h) were calculated for the two series separately. In this way a sort of check upon the results of the First Series was afforded by those of the Second Series, as to tendencies shown and effect of progressive practice.

RESULTS

In Tables I to IV will be found the percentages of observed relative frequencies of correct judgments made by each of the four *O*s. The top line gives the systematic categories, Attention, Cognition and Apprehension, which we have applied to the three conscious patterns or 'methods of determining' the reports. In the second line appear the three stimulus intensities, Black, Dark and Light (Bk, D, and L) under each of the process patterns. The third line contains the three degrees of subjective assurance; 5, 4, and 3 (2 and 1 having been discarded as explained above) under each of the intensities.

In the first column are the values of the individual stimuli (2-12). In succeeding columns are to be found the percentages of correct judgments given for each stimulus value, with each degree of assurance, for all intensities, under each conscious pattern. The upper half of each table shows results for Series I, the lower half those for Series II.

In Tables V, VI, VII, and VIII, are indicated the calculated results for all the *O*s. The first column contains the systematic categories, Attention, Cognition, and Apprehension, each coupled with the three degrees of subjective assurance. The first line in each of the groups indicates the calculated results of the observed relative frequencies of correct judgments given with an assurance of '5' (100-1 bet); the second line shows the calculated results of correct judgments given with assurance '5' *plus* those with assurance '4;' the third line shows the calculated results of observed correct judgments given with all three degrees of assurance ('5,' '4,' and '3') in summation.

These calculated results are arranged in 6 columns: the first 3 containing the limens (S) for the different intensity-degrees (Bk, D, and L); the second 3 containing the values of h, or coefficient of precision, under the various categories. To facilitate comparison of results for the two series, both have been grouped under their respective categories.

Analysis of Results. In the formulation of our problem, it was assumed that if perceptual clearness bears some constant relation to intensity of stimulation, such relation would be reflected in the size of the limens and the shapes of the psychometric curves. In selecting stimuli we accordingly chose those which seemed of sufficient intensity-difference to produce such variation in the size of S and h, if the relationship is found to exist.

Determination of significant difference in size of these measures required the setting up of a standard. It was arbitrarily decided to adopt a difference of 0.5 as our criterion of signifi-

TABLE I
Observer F

Series	No. Stimuli	Category																										
		Attention									Cognition									Apprehension								
		Bk			D			L			Bk			D			L			Bk			D			L		
		5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3
First	2	96	—	—	84	—	—	46	—	—	4	—	—	14	—	—	52	—	—	—	—	—	2	—	—	2	—	—
	3	82	—	—	48	—	—	40	—	2	18	—	—	50	—	—	52	4	—	—	—	—	2	—	—	—	2	—
	4	78	—	—	12	—	—	16	2	—	22	—	—	72	4	—	66	4	2	—	—	—	10	2	—	10	—	—
	5	34	—	—	4	—	—	8	2	—	50	6	—	44	2	—	30	4	—	4	6	—	14	24	8	10	26	14
	6	8	—	—	—	—	—	—	—	—	22	2	—	10	—	2	2	—	2	20	24	16	38	22	10	26	26	20
	7	—	—	—	—	—	—	—	—	—	4	—	—	—	—	—	—	—	—	10	18	34	16	24	22	4	14	36
	8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	16	10	44	—	12	22	—	10	18
	9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	2	34	—	2	12	—	—	8
	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4	18	—	—	4	—	—	6
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10	—	—	—	—	—	—
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	2
Second	2	86	—	—	64	—	—	50	—	—	14	—	—	34	—	—	50	—	—	—	—	—	—	—	2	—	—	—
	3	68	—	—	40	—	—	38	—	—	28	—	—	58	—	—	60	2	—	4	—	—	—	2	—	—	—	—
	4	70	—	—	22	—	—	30	—	—	30	—	—	74	—	—	64	—	—	—	—	—	—	—	4	4	2	—
	5	50	—	2	12	—	—	6	—	—	40	—	—	48	2	—	52	—	—	4	2	—	22	14	2	14	14	10
	6	2	—	—	—	—	—	—	—	—	16	—	—	12	4	2	12	—	—	46	22	8	22	32	16	20	24	30
	7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	12	28	36	10	36	24	18	18	48
	8	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	—	10	24	34	—	8	34	6	—	38
	9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4	4	40	—	2	32	—	—	30
	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6	22	—	—	6	—	—	6
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	22	—	—	4	—	—	2
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	4	—	—	—

TABLE II
Observer H

Series	No. Stimuli	Category																										
		Attention									Cognition									Apprehension								
		Bk			D			L			Bk			D			L			Bk			D			L		
		5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3
First	2	100	—	—	100	—	—	100	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	3	98	—	—	98	—	—	100	—	—	2	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—
	4	86	—	—	78	—	—	92	—	—	12	—	—	18	—	—	8	—	—	2	—	—	4	—	—	—	—	—
	5	22	—	—	24	—	—	16	2	—	54	2	8	64	4	—	64	2	4	14	—	—	8	—	—	6	—	4
	6	—	—	—	—	—	—	—	—	—	48	6	6	48	4	10	70	2	8	26	8	2	22	2	2	6	—	—
	7	—	—	—	—	—	—	—	—	—	26	6	2	24	10	4	22	4	16	22	14	12	26	4	14	18	8	12
	8	—	—	—	—	—	—	—	—	—	4	4	14	6	4	12	10	6	12	4	16	18	10	8	14	6	2	20
	9	—	—	—	—	—	—	—	—	—	2	6	4	4	—	4	—	2	4	4	2	10	—	—	10	4	2	4
	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	4	—	—	6	—	—	2
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Second	2	100	—	—	100	—	—	100	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	3	100	—	—	96	—	—	98	—	—	—	—	—	4	—	—	2	—	—	—	—	—	—	—	—	—	—	—
	4	68	—	—	66	—	—	70	—	—	32	—	—	32	—	—	26	—	—	—	—	—	2	—	—	4	—	—
	5	18	—	—	12	—	—	24	—	—	76	—	—	84	2	2	70	—	2	4	—	—	—	—	—	2	—	2
	6	—	—	—	—	—	—	4	—	—	38	2	16	52	8	—	68	4	10	38	—	6	18	4	6	10	2	—
	7	—	—	—	—	—	—	—	—	—	46	6	4	50	4	4	30	2	10	18	8	4	28	2	6	22	6	22
	8	—	—	—	—	—	—	—	—	—	18	2	8	14	—	10	18	4	18	24	4	14	28	8	28	18	—	22
	9	—	—	—	—	—	—	—	—	—	6	2	6	6	—	6	2	—	6	14	2	28	2	—	14	16	6	16
	10	—	—	—	—	—	—	—	—	—	4	—	2	2	—	—	—	—	4	8	—	20	2	—	8	4	—	6
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4	—	10	2	2	4	—	—	4
	12	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

TABLE III
Observer R

Series	No. Stimuli	Category																										
		Attention									Cognition									Apprehension								
		Bk			D			L			Bk			D			L			Bk			D			L		
		5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3
First	2	100	—	—	100	—	—	100	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	3	38	—	—	52	—	—	46	—	—	62	—	—	48	—	—	54	—	—	—	—	—	—	—	—	—	—	—
	4	24	—	—	12	—	—	26	—	—	56	6	—	52	4	—	34	4	—	12	—	—	26	4	—	22	10	4
	5	6	—	—	6	—	—	2	—	—	62	4	—	66	12	—	66	6	—	4	8	4	2	2	2	—	6	4
	6	—	—	—	—	—	—	2	—	—	18	6	—	20	10	—	20	10	—	4	12	—	10	20	6	6	22	2
	7	—	—	—	—	—	—	—	—	—	4	—	—	6	4	—	6	—	—	—	24	10	4	24	10	4	20	10
	8	—	—	—	—	—	—	—	—	—	—	2	—	—	2	—	—	—	—	2	8	2	—	10	10	—	6	12
	9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10	—	—	2	—	—	—	6
	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	4	—	6	—
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4	—	—	—	—	—	—
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	2	—	—	—	—	—	2
Second	2	100	—	—	100	—	—	100	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	3	86	—	—	82	—	—	86	—	—	14	—	—	18	—	—	14	—	—	—	—	—	—	—	—	—	—	—
	4	80	—	—	56	—	—	64	—	—	16	—	—	34	—	—	28	—	—	4	—	—	8	—	2	8	—	—
	5	18	—	—	24	—	—	4	—	—	70	—	—	32	2	—	74	2	2	2	—	—	2	4	—	6	—	—
	6	—	—	—	—	—	—	—	—	—	18	—	2	30	8	—	50	2	2	20	24	6	22	4	—	6	12	2
	7	—	—	—	—	—	—	—	—	—	2	2	—	8	—	—	8	—	—	8	22	8	18	28	4	10	24	2
	8	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	—	2	20	6	2	14	12	2	10	8
	9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6	6	—	—	8	—	2	4
	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	6	—	—	—
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	8	—	—	—	—	—	—
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	4

TABLE IV
Observer M

Series	No. Stimuli	Category																										
		Attention									Cognition									Apprehension								
		Bk			D			L			Bk			D			L			Bk			D			L		
		5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3
First	2	74	—	—	94	—	—	82	—	—	22	—	—	4	—	—	18	—	—	4	—	—	2	—	—	—	—	—
	3	98	—	—	82	—	—	90	—	—	2	—	—	16	—	—	8	—	—	—	—	—	—	—	—	2	—	—
	4	18	—	—	2	—	—	8	—	—	80	—	—	94	2	—	90	—	2	—	—	—	2	—	—	—	—	—
	5	—	—	—	—	—	—	2	—	—	94	—	—	96	—	—	96	2	—	2	—	—	2	—	—	—	—	—
	6	—	—	—	—	—	—	—	—	—	70	2	2	84	6	2	86	4	—	22	—	—	2	—	—	6	2	—
	7	—	—	—	—	—	—	—	—	—	48	14	8	62	26	2	56	16	2	4	2	—	4	2	—	2	4	—
	8	—	—	—	—	—	—	—	—	—	26	22	12	12	26	6	16	32	6	4	4	2	4	4	—	—	—	—
	9	—	—	—	—	—	—	—	—	—	6	42	12	—	10	16	—	18	16	—	2	4	—	—	—	—	4	—
	10	—	—	—	—	—	—	—	—	—	—	16	22	—	4	6	—	4	18	—	4	—	—	—	2	—	2	6
	11	—	—	—	—	—	—	—	—	—	—	—	10	—	—	4	—	—	—	—	—	2	—	—	2	—	—	2
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Second	2	52	—	—	76	—	—	56	—	—	48	—	—	24	—	—	44	—	—	—	—	—	—	—	—	—	—	—
	3	66	—	—	58	—	—	58	—	—	34	—	—	42	—	—	42	—	—	—	—	—	—	—	—	—	—	—
	4	4	—	—	4	—	—	—	—	—	96	—	—	96	—	—	100	—	—	—	—	—	—	—	—	—	—	—
	5	8	—	—	—	—	—	6	—	—	90	2	—	100	—	—	92	—	—	—	—	—	—	—	—	—	—	—
	6	—	—	—	—	—	—	—	—	—	100	—	—	88	4	—	96	2	—	—	—	—	2	—	—	2	—	—
	7	—	—	—	—	—	—	—	—	—	76	8	—	82	10	2	78	10	4	6	2	—	2	—	—	2	2	—
	8	—	—	—	—	—	—	—	—	—	66	24	2	36	30	10	56	24	12	—	—	—	—	2	2	—	—	—
	9	—	—	—	—	—	—	—	—	—	14	26	10	12	26	6	12	24	14	—	14	2	—	6	—	2	4	4
	10	—	—	—	—	—	—	—	—	—	8	18	18	—	6	14	2	18	8	—	10	2	—	4	8	—	12	2
	11	—	—	—	—	—	—	—	—	—	—	6	6	—	6	14	—	2	8	—	6	12	—	6	14	—	—	12
	12	—	—	—	—	—	—	—	—	—	—	—	2	—	—	4	—	—	—	—	—	2	—	—	6	—	—	6

cance for the limens and one of 0.2 for the precisions. Differences less than these we believed would be non-significant or, at best, of doubtful significance.

Turning to examination of the calculated results, Tables V to VIII, we find progressive increase in the size of the limens for all *O*s, from Attention to Cognition, to Apprehension. This was, of course, to be expected, inasmuch as the categories are progressively inclusive, as stated above. The improvement incident to progressive practice is evident by comparison of limens for Series II with those of Series I.

TABLE V

Observer F

Category	Series	Assurance	Limen			precision (h)		
			Bk	D	L	Bk	D	L
Attention	First	5	4.479	2.961	2.015	0.541	0.686	0.321
		5+4	4.479	2.961	1.975	0.541	0.686	0.293
		5+4+3	4.479	2.961	2.033	0.541	0.686	0.296
	Second	5	4.496	2.600	2.833	0.324	0.367	0.436
		5+4	4.496	2.600	2.833	0.324	0.367	0.436
		5+4+3	4.463	2.600	2.833	0.335	0.367	0.436
Cognition	First	5	5.677	4.874	4.619	0.995	0.661	0.671
		5+4	5.784	4.930	4.785	1.092	0.667	0.695
		5+4+3	5.784	4.960	4.809	1.092	0.657	0.725
	Second	5	5.502	5.118	5.110	0.622	0.656	0.841
		5+4	5.502	5.200	5.143	0.622	0.628	0.967
		5+4+3	5.525	5.236	5.143	0.616	0.617	0.967
Apprehension	First	5	6.169	5.726	5.117	0.518	0.531	0.566
		5+4	6.673	6.597	6.089	0.447	0.567	0.537
		5+4+3	8.443	7.420	7.192	0.359	0.486	0.405
	Second	5	6.396	5.661	5.650	0.588	0.561	0.592
		5+4	6.972	6.587	6.216	0.426	0.468	0.609
		5+4+3	8.956	7.937	7.871	0.341	0.378	0.442

For H and M improvement is most marked in Cognition and Apprehension, while in Attention there is a decrease, which is probably due to a tendency to characterise as *Grouped* some judgments originally given under the category of *Immediate*, rather than to an actual decrease of ability to grasp the stimuli immediately.

For F there is slight improvement Attention, especially for L stimuli, less for Bk and none for D. Under Cognition, F shows no improvement for Bk, slight for D and more marked for L. Under Apprehension, F shows uniform improvement (with one slight inversion), the greatest being again in L.

R shows marked and steady improvement throughout; he stated in one of his introspections, "there were more *Immediate* judgments to-day, for I seemed more capable of grasping an entire field. Re-imaging larger groups of dots was also easier."

Limens compared for the several Os. A comparison of the limens of Attention, Cognition, and Apprehension for the different stimuli and the different degrees of assurance reveals the following:

H, Table VI. The data show that in Series I, Attention, assurance 5, the order of the limens are L highest, Bk intermediate, D lowest; the differences, however, fall far below our criterion of significance; L being 0.029 greater than B., and 0.069 greater than D; Bk 0.04 greater than D. With assurance 5 + 4 and 5 + 4 + 3 the samé order is maintained, the only variation being an increase of 0.018 for the L limen, a difference which is still much below what we decided to consider significant. In Series II, Attention, similar relations are found as in Series I, only exaggerated; here the limens are the same for all three degrees of assurance, which indicates that judgments were given with assurance 5 only. L is 0.130 higher than Bk, and 0.228 higher than D; while Bk is 0.098 higher than D. No significance.

Table VI

Observer H

Category	Series	Assurance	Limen			precision (h)		
			Bk	D	L	Bk	D	L
Attention	First	5	4.556	4.516	4.585	1.091	1.001	1.710
		5+4	4.556	4.516	4.603	1.091	1.001	1.623
		5+4+3	4.556	4.516	4.603	1.091	1.001	1.623
	Second	5	4.338	4.240	4.468	0.966	1.048	0.870
		5+4	4.338	4.240	4.468	0.966	1.048	0.870
		5+4+3	4.338	4.240	4.468	0.966	1.048	0.870
Cognition	First	5	6.032	6.149	6.290	0.549	0.525	0.550
		5+4	6.140	6.353	6.825	0.414	0.505	0.485
		5+4+3	6.642	6.675	6.926	0.418	0.443	0.479
	Second	5	6.418	6.668	6.613	0.392	0.488	0.552
		5+4	6.552	6.796	6.714	0.384	0.506	0.535
		5+4+3	6.966	6.905	7.218	0.376	0.419	0.459
Apprehension	First	5	6.787	6.854	6.687	0.523	0.583	0.522
		5+4	7.255	7.173	7.005	0.424	0.543	0.488
		5+4+3	8.224	7.957	7.863	0.567	0.519	0.526
	Second	5	7.626	7.463	7.379	0.381	0.450	0.454
		5+4	7.906	8.204	7.632	0.391	0.480	0.459
		5+4+3	9.118	8.802	8.832	0.379	0.577	0.573

The results for Cognition, Series I, Assurance 5 show complete inversion, L being highest, D intermediate and Bk lowest; the differences being: L 0.141 greater than D, and 0.258 greater than Bk; D 0.117 greater than Bk. No significance.

With Assurance 5 + 4, similar though greater differences obtain; L is 0.472 greater than D and 0.685 greater than Bk. Here is our first occurrence of a significant difference, but it is reversed, *i.e.* the limen for the least intensive stimulus is higher than that for the most intensive!

With Assurance 5 + 4 + 3, the order is the same, viz. L, D, Bk; but the differences are non-significant, due to a greater relative increase in the size of Bk than for either D or L, and greater for D than for L. Under Cognition, Series II, Assurance 5 and 5 + 4, there is a shift in the order to D, L, Bk; with Assurance 5 + 4 + 3 another shift to L, Bk, D, but with no significance in any case.

Passing to the limens for Apprehension, Series I, Assurance 5, we find another shift to D, Bk, L, and again a change in the order for Assurance 5 + 4 and 5 + 4 + 3, to Bk, D, L; still without significant difference. In Series II, Assurance 5, the relative order remains the same, viz. Bk, D, L; not significantly different. With Assurance 5 + 4, a shift to D, Bk, L, occurs with differences as follows: D 0.0298 greater than Bk, D 0.572 greater than L, and Bk 0.278 greater than L. Here is a significant difference as between D and L, the first and only time for this *O* when a more intense stimulus produced a significantly higher limen than that for the less intense!

TABLE VII

Observer R

Category	Series	Assurance	Limen Bk	Limen D	Limen L	precision (h) Bk	precision (h) D	precision (h) L
Attention	First	5	2.576	2.966	2.917	0.418	0.611	0.509
		5+4	2.576	2.966	2.917	0.418	0.611	0.509
		5+4+3	2.576	2.966	2.917	0.418	0.611	0.509
	Second	5	4.352	4.148	4.030	0.719	0.576	0.906
		5+4	4.352	4.148	4.030	0.719	0.576	0.906
		5+4+3	4.352	4.148	4.030	0.719	0.576	0.906
Cognition	First	5	5.157	5.000	4.999	0.639	0.474	0.376
		5+4	5.358	5.269	5.160	0.638	0.457	0.425
		5+4+3	5.358	5.269	5.190	0.638	0.457	0.355
	Second	5	5.592	5.613	5.790	0.832	0.696	0.636
		5+4	5.610	5.705	5.808	0.814	0.673	0.656
		5+4+3	5.622	5.705	5.866	0.812	0.673	0.642
Apprehension	First	5	5.431	5.540	5.358	0.699	0.640	0.543
		5+4	6.210	6.455	6.378	0.353	0.529	0.413
		5+4+3	6.399	6.799	6.595	0.304	0.452	0.339
	Second	5	5.927	6.196	6.074	0.741	0.679	0.703
		5+4	6.634	6.815	6.585	0.461	0.522	0.545
		5+4+3	6.999	7.050	6.901	0.330	0.423	0.346

It is to be noted, at this point, that from a quantitative point of view there is less intensity-difference between the two grays than between the dark gray and the black stimulus, as is shown by the relative percentages of black noted under *Apparatus*. With Assurance 5 + 4 + 3, the order again changes to Bk, L, D, the differences being non-significant.

Summing up the results for Observer H, we find that there are but two cases where a significant difference is shown between limens for different intensities of stimulus under the same process and with the same degree of assurance. In Series I, Cognition, Assurance 5 + 4, the L limen was 0.685

higher than the Bk. In Series II, Apprehension, Assurance 5 + 4, D gave a limen 0.572 higher than did L. In 10 cases L was highest; Bk and D were highest in 4 cases each.

R, Table VII. Without going into so minute detail in examination of R's limens, we may summarize the results as follows:

The relative order under Attention, Series I, all assurances, (judgments under this category being given with assurance 5 only) is: D, L, Bk. For Series II, it is Bk, D, L, with non-significant differences. Under Cognition, Series I, all assurances, the order is Bk, D, L; Series II, all assurances, L, D, Bk. Under Apprehension, both series, all assurances, the D limens are high in every case; intermediate values are given for Bk in 3 cases and for L in 3 cases.

In no case was there a significant difference for R. In 9 cases D was highest, in 6 cases Bk, and in 3 cases L.

TABLE VIII

Observer M

Category	Series	Assurance	Limen			precision (h)		
			Bk	D	L	Bk	D	L
Attention	First	5	3.124	3.277	3.211	0.542	1.113	0.766
		5+4	3.124	3.277	3.211	0.542	1.113	0.766
		5+4+3	3.124	3.277	3.211	0.542	1.113	0.766
	Second	5	2.607	2.863	2.710	0.411	0.739	0.390
		5+4	2.607	2.863	2.710	0.411	0.739	0.390
		5+4+3	2.607	2.863	2.710	0.411	0.739	0.390
Cognition	First	5	6.848	7.148	7.081	0.373	0.396	0.496
		5+4	8.038	7.619	7.716	0.254	0.395	0.417
		5+4+3	8.737	7.960	8.308	0.253	0.361	0.361
	Second	5	8.062	7.672	7.899	0.529	0.599	0.615
		5+4	8.903	8.493	8.708	0.475	0.491	0.499
		5+4+3	9.275	8.994	9.164	0.417	0.392	0.456
Apprehension	First	5	7.277	7.228	7.138	0.446	0.481	0.627
		5+4	8.489	7.804	7.948	0.338	0.475	0.489
		5+4+3	9.237	8.207	8.534	0.304	0.409	0.407
	Second	5	8.134	7.714	7.951	0.553	0.642	0.625
		5+4	9.355	8.770	8.966	0.481	0.456	0.457
		5+4+3	9.908	9.579	9.636	0.435	0.329	0.413

M, Table VIII. M's results show the following features: In Attention, both series, all assurances, the D limens are the highest, the intermediate values going to L. The differences are all small, however, the greatest being that between D and Bk, Series II, D being 0.256 higher than Bk. In Cognition, we find the order Bk, L, D in every case but one, where it is D, L, Bk (Assurance 5, Series I); with Assurance 5 + 4 + 3, Series I, occurs the first significant difference: Bk 0.777 greater than D, but L 0.348 greater than D.

In Apprehension, both series, all assurances, the order is the same as for Cognition, viz. Bk, L, D, in every case but one, where it is Bk, D, L (Assurance 5, Series I).

In 5 cases we find significant differences, always in favor of the Bk: 3 times against D, and twice against L.

Thus we find M showing a significant difference 6 times: none under Attention, once under Cognition, 5 times under Apprehension, and always in favor of Bk.

Considering absolute differences, we find: Bk highest 11 times, D highest 7 times, L highest no times, Bk intermediate no times, D 1 time, L 17 times, Bk lowest 7 times, D 10 times, L once.

F, Table V. In F's results we find a striking contrast to those of the other *Os*. In every case the Bk limens are the highest; in 15 cases the D limens are greater than the L, but in only 3 cases are the limens higher than D. Significant differences are shown between Bk and L, in every case except Cognition, Series II, all assurances; Bk, is significantly higher than D in every case in Attention, both series; in every case in Cognition, Series I. In 3 cases under Apprehension, D is significantly higher than L; also in Attention, Series I, all assurances; and in Apprehension, Series I, Assurances 5 and 5 + 4. Bk highest 18 times, D no times, L no times; D higher than L 15 times; L higher than D 3 times.

Introspections. Additional evidence of the inconstancy of effect of intensity-differences upon the size of the limens is afforded by some spontaneous introspective reports of the *Os*.

F, who, it will be remembered, knew in advance of the intensity-differences, stated at the close of the first observation period that "in not over 6 of the cards was he conscious of any differences in brightness of the dots."

H reported after his first observation period, "there are, I believe, several kinds of cards with dots of black and gray. On not more than three cards was I conscious of the difference. One card seemed to 'blare,' because of the great number of black dots." Three weeks later he reported, "Eight cards definitely gray, three definitely black." And, again, one week later, "I noticed that there were cards with black dots that were almost impossible for me. They seemed to be too bright, while the gray dots were not nearly so hard to attempt. I do not think that there were more than 10 cards exposed today in which I was conscious of the difference in color of the dots." Two months later, he said: "A very few times I was conscious of difference of brightness; some cards with black dots were very vivid."

R gave, at the very last, in the course of a detailed introspection on every card of a whole series, the following varied reports for the different intensities:

Bk stimuli. "They stood out boldly" [the number apprehended was incorrect]. "Clear at first, but I couldn't grasp any on presentation (of report)" [Number wrong]. "Clear—re-imaged," [Number wrong].

D stimuli. "The dots seemed very bright—the clearness was quite pronounced" [Number wrong]. "Very clear" [Number correct]. "Not so clear, but certain at once" [Number correct]. "Fairly clear—no uncertainty" [Number correct].

L stimuli. "The whole thing faint" [Number correct]. "Not very clear" [Number wrong]. "Not so clear—absolute certainty—came at once—no re-imaging required—dots seemed large but not saturated" [Number correct].

The introspections of R are strongly confirmatory of Dallenbach's finding relative to the independent variability of attributive and cognitive clearness. With attributive clearness high or low may be combined either high or low cognitive clearness.[18]

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[18] Dallenbach, Attributive *vs.* cognitive clearness, *loc. cit.*

The introspective reports, quoted above, of F, H, and R, and others less specific of M, all indicate clearly the extreme variability of the intensity effect.

Size of h *compared for the several Os.* Observation of the size of the coefficients of precision (h), for the 3 categories for the several *Os*, shows the following features.

For H, the values of h are highest for Attention, for all intensities and all degrees of assurance; there is about equal division of intermediate values between Cognition and Apprehension, with no definite trend toward any one intensity.

For F, the h-values are highest for Cognition with Bk and L; for Cognition and Attention with D.

For R, Cognition shows the higher h-values for Bk and D, in Series II; and for Bk in Series I, whereas Attention gives the higher values of h for L in Series II, and for D and L in Series I.

For M, Attention gives higher values for h for Bk, D, and L in Series I, and for D, in Series II. Apprehension gives higher values for Bk and L in Series II.

The significant differences—the criterion of 0.2, it will be remembered, was fixed upon as suitable—are, in the main, as indicated above. In several cases, in addition to the foregoing, significant differences are shown between the value of h for assurances 5 and 5 + 4 + 3, but without altering the ratios.

No constant trend whatever appears in these h values, in relation to either differences of intensity or conscious process. These results confirm Fernberger's findings in his study of the range of visual apprehension in which he states, "it was expected that the form of the curves might vary for the different series, thus showing the influence of the brightness of the backgrounds" [he used black dots on a white and on a medium gray background] . . . but "no such variation is present for any subject."[19]

DISCUSSION

Table IX summarizes the relative frequencies with which the possible orders of precedence as to size of limens occurred for the several *Os*. For instance, the number of times that the limens occurred in the order, Bk highest, D intermediate, L lowest was for F, 15 times; for H, 3 times; for R, 6 times; for M, once. At the other extreme, the order L high, D middle, Bk low, occurred only 6 times: 3 times each for H and for R.

By using different combinations, we may arrive at determinations of various relative frequencies of occurrence. For example, we find by combining the figures lying in the first and second rows, in any vertical column, the number of times the Bk stimulus gave the highest limen, *e.g.* for F, 18 times; for H

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[19] Fernberger, *op. cit.*, 129.

4 times; etc. Similarly, the number of times L produced a higher limen than Bk, for all *O*s, may be found by adding the totals of rows 4, 5, and 6.

From Table IX it will be seen that for F, Bk was by far the most effective stimulus, giving the limens higher than both the grays in every case; D was next in order of effectiveness, in that its limens exceeded those of L in 15 out of 18 times.

TABLE IX

RELATIVE FREQUENCIES OF DIFFERENT ORDERS OF LIMENS

Order of Limens	*O*				Totals
	F	H	R	M	
Bk—D—L	15	3	6	1	25
Bk—L—D	3	1	0	10	14
D—Bk—L	0	2	3	0	5
D—L—Bk	0	2	6	7	15
L—Bk—D	0	7	0	0	7
L—D—Bk	0	3	3	0	6

For M, too, Bk was the most effective, as shown by its frequency of predominance: the Bk limen was higher than both D and L, 11 times. On the other hand, L limens exceeded Bk 7 times, and D 10 times, indicating intermediate effectiveness of L, instead of D, as shown in F's results.

For R, D appears the most effective of the 3 intensities, exceeding Bk 12 times, and L 15 times. Bk is next most effective, and L least.

For H, L shows higher than Bk 12 times, and higher than D 11 times. Bk comes next with 11 limens higher than the D, and 6 higher than L.

From the foregoing it appears that intensity does not exercise a constant effect upon the size of the limens. For F, the order of effectiveness is Bk, D, L; for H: L, Bk, D; for R: D, Bk, L; and for M: Bk, L, D.

TABLE X

RELATION OF THE LIMENS

Category	Relation	*O*				Relation	*O*			
		F	H	R	M		F	H	R	M
Attention	L — Bk	0	6	3	6	Bk — L	6	0	3	0
	D — Bk	0	0	3	6	Bk — D	6	6	3	0
	L — D	3	6	0	0	D — L	3	0	6	6
Cognition	L — Bk	0	6	3	1	Bk — L	6	0	3	5
	D — Bk	0	5	3	1	Bk — D	6	1	3	5
	L — D	0	3	3	5	D — L	6	3	3	1
Apprehension	L — Bk	0	0	3	0	Bk — L	6	6	3	6
	D — Bk	0	2	6	0	Bk — D	6	4	0	6
	L — D	0	2	0	5	D — L	6	4	6	1

Referring to Table X, we find the relative frequencies of predominance arranged under the systematic categories or conscious processes.

In the first 4 columns appear the number of times, for each *O*, that a less intense stimulus gave a higher limen than did a more intense, while in the second 4 columns the reverse is shown. Each group is fractionated into the 3 systematic categories or conscious processes, under which the frequencies occurred.

For F, only under the category of Attention, there appeared a predominance for a less intense stimulus: L was greater than D 3 times. In every other case the more intense stimulus always gave higher limens.

For H, a predominance of L is shown, over both Bk and D, every time under Attention, and a similar predominance of Bk over D. Under Cognition, the same relation holds for L as compared with Bk; D, however, takes the lead over Bk in 5 cases out of 6; while the L predominance over D drops to 50% (3 out of 6). Passing to Apprehension, we find the superiority definitely shifted to the more intense stimuli: L going below Bk every time; D below Bk 4 out of 6 times, and L below D 4 times.

For R, the predominant effectiveness of D as conditioning higher limens is definitely shown in passing from Attention to Apprehension.

For M, the tendencies noted earlier are here confirmed under Cognition and Apprehension, viz. Bk greater than D, Bk greater than L, and L greater than D.

From these data it seems safe to generalize the relations as follows: In general under the conditions of this experiment, objective intensity of stimulation is most effective in conditioning perceptual clearness under the process of Apprehension, less so under Cognition, and least under Attention.

Let us now scrutinize more closely the conscious processes here involved, in an attempt to get some further light on the differences shown above.

It is obvious that in passing from Attention, to Cognition, to Apprehension, we are traversing an ascending scale of mental complexity. Attention is the simplest process; in it the stimulus material comes to consciousness in a single flash of awareness, with an equal and high degree of clarity. In regard to this category, the introspective reports of the *O*s agree.

R reported: "When I characterized a judgment as *Immediate*, it was immediate, no intervening process—all happened in an instant and I was certain at once." Again: "Equal clarity—all in an instant—no re-imaging—absolute certainty and all came at once—no verification was needed."

H reported: "The *Immediate* judgments are based on the grasping of the dots without any conscious grouping or counting." "*Immediate* reports were clear—no question in my mind, and no subsequent process was involved."

"*Immediate* reports were given when all dots were clear—a focus which left no doubt in my mind."

M reported: "By *Immediate* I mean that all dots are clear and are perceived immediately upon the fall of the shutter."

It seems evident that this category is correctly described. In Cognition, which we defined tentatively as one in which "immediate perception occurred, but owing to different degrees of clearness, there is an attentional grouping of the stimulus dots," all *O*s agree as to the process of becoming aware of groups of dots.

H states: "In *Grouping* there is a definite grouping of the dots, generally accomplished by taking in twos, threes, or fours."

R says: "When I answer *Grouped*, several dots 'stick together' forming a unit, while another or several other units are formed at other locations."

M reports: "The dots seem more or less naturally to fall into groups. If there is merely one of these groups I report an *Immediate* judgment; if two or more groups I report *Grouping*."

So far as the grouping is concerned, there is no question, but when the factor of immediacy of the *Grouped* judgments is scrutinized, serious doubts arise.

R reported: "The *Grouped* judgments are almost immediate, with some one group particularly clear, the others less clear, but all somehow belonging together." "Got 3 groups of 2 each—it seemed the middle 2 first, then 2 above and then 2 below—no re-imaging—but there seemed to be temporal succession" (this report was in response to a card with 6 L dots). "Three and three, got those to right slightly ahead of those to left" (this on another 6-stimulus card, D dots).

H reported: *Grouped* judgments as based on a reading of the after-image, *i.e.* the *Anhang* or perseverative image, subsequent to actual stimulation.

M reported: *Grouped* judgments as based on a 'successive' awareness of the dots, *e.g.* a '2' judgment was "one dot, then another;" '3' was "two and one —succession."

These introspective reports from three of the *O*s definitely indicate a temporal sequence as characterizing this process, yet without the mediation of re-imaging. The actual retinal stimulation gives rise to an immediate awareness in which all the dots are of equal and high degree of clearness; the judgment based on this momentary flash of consciousness is what we have called Attention. The conscious effect of stimulation is not, however, confined to this momentary flash, but continues for a more or less durative period as the perseverative image. It seems that the *Grouped* judgment (Cognition) is arrived at by a reading of this image, by 'successive attentions'[20] to smaller groups, and a rapid totaling of these part-judgments.

The *Counted* judgments, or Apprehension as we have called this process, seem definitely to be characterized by a re-imaging or calling back into consciousness of a process which has, wholly or in part 'faded out.' Frequently, if not always, some group or groups remain in consciousness, as part of the perseverative image which has not 'faded;' this forms the nucleus which is later amplified by the re-imaged dots.

H said: "With *counting* I use any method, mixing up *grouping* and *counting*. I generally have an immediate flash of the dots at the point of fixation and begin to group or count from that point."

R states: "With large numbers in *counted* judgments, I usually get a part of the field immediately, usually the upper left."[21]

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[20] Pillsbury, *Attention*, 1908, 67.

[21] It is interesting to note that the three *Os* who mentioned the direction in which the image was read were all different in this respect: R read the dots from left to right; M from top to bottom; and H from bottom upward.

Thus, it appears, as Oberly observed in his paper,[22] that progressive elaboration of mental process occurs as the complexity of the stimulus material increases, *i.e.* as the number of dots is increased.

Reference to Tables I to IV shows the relative frequencies of individual judgments for the several *O*s in respect to this elaboration of process.

F, Table I. F shows a gradual shift from Attention to Cognition. The great majority of his Bk 2-, 3-, and 4-dot cards, and most of the D 2- and 3-dot cards were grasped under the process of Attention; the L stimuli being for him less effective, required an earlier shift to Cognition. The shift from Cognition to Apprehension, occurs rather suddenly at the 5-dot cards for D and L, and at the 6-dot cards for Bk. F depended upon the more complex process of Apprehension for the grasping of everything above 6, with the exception of four Bk 7-dot cards.

H, Table II. With H the shift from Attention to Cognition occurs rather suddenly at 5, and that from Cognition to Apprehension at 6 and 7. This *O* shows an almost equal distribution of Cognition and Apprehension judgments.

R, Table III. With R, the break from Attention to Cognition occurs at 3, and a rather high percentage of 6 come under Apprehension. The 2nd Series shows the great improvement made by this *O* as a result of progressive practice.

M, Table IV. M's results show an almost complete dependence upon Cognition. It seems a rather significant fact that this *O* was of strongly visual concrete imaginal type. This fits in well with Hylan's findings that length of span was correlated with relative duration of visual after-image.[23] This *O* showed the highest limens of all the *O*s for both Cognition and Apprehension. Inspection of his results for Series II shows also the very high assurance with which his judgments were given: only 8 had assurance less than 5 up to and including the 6-stimuli.

Thus it would seem that for the lower values of stimulus, the univocal process of Attention is adequate, and the awareness comes immediately and with maximal clearness. With the intermediate stimulus-values a more complex conscious process is required, which takes, sometimes at any rate, a certain length of time for exploration of the after-image. With the higher values, the organism is pushed to its utmost to grasp the material presented, and has recourse to whatever additional means are at its disposal, viz. a re-imaging of the material. If this analysis is correct, the reason is plain why intensity differences fail to become operative in the majority of cases until the more complex processes are brought into play. The experimental setting and the process of training bring about the establishment of a mental 'set' that is of varying efficacy for the different *O*s.

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[22] Oberly, *op. cit.*

[23] J. P. Hylan, The distribution of attention, *Psychol. Rev.*, 10, 1903, 373-403, 498-533.

For instance, with H, the experimental conditions were such that it was not until the higher stimulus values were presented that he was forced to avail himself of every factor in the situation that could serve to bring the awareness for which he was striving. Under the process of Apprehension, with its expedient of re-imaging added to the operations used in Attention and Cognition, the factor of intensity at length became operative and gave the correspondingly higher limens for Bk and negligible differences between the D and L, due to too slight intensity differences.

With F, greater experience in introspective work along the lines of psychophysical observation enabled him to avail himself of the intensity-differences at an earlier stage.

R's predominance of D over Bk, and the equality of intensity effect as between Bk and L in Apprehension, offer an anomalous situation, as do also, though in less degree, M's higher limens for L than for D.

Pending further illumination on this perplexing question, we may be justified in formulating, in a tentative way, the conclusion that:

Intensity differences are of variable effectiveness in conditioning clearness of perception, the latter being presumably reflected in the size of the limens as shown in this experiment.

Summary

(1) Liminal differences are shown for all three values of stimulus-intensity.

(2) Stimulus intensities do not, however, seem to have any constant effect for any of the *O*s.

(3) The stimuli effective in conditioning highest limens are: for F, Black definitely the most effective; for M, Black most effective, with Light predominant over Dark; for R, Dark; for H, Light.

(4) In general, a predominant effectiveness is found for the more intense stimuli under the category of Apprehension, less so under Cognition, and least under Attention.

(5) Confirming Fernberger's findings, no constant variation is shown by the values of the coefficients of precision as reflecting the influence of variations of stimulus-intensity.

THE RANGE OF VISUAL ATTENTION, COGNITION, AND APPREHENSION FOR COLORED STIMULI

By JOHN R. HART, JR., University of Pennsylvania

Recent experiments on the 'Range of Attention' problem have made it evident that the usual experimental procedure and instructions give results for a mental process much more complicated than could possibly be included within the term Attention.[1] In order to further study this difficulty, Oberly,[2] on the basis of preliminary introspective reports, calculated the statistical limens for three typical processes which he called attention, cognition, and apprehension.

Oberly employed black stimuli on a white background exposed tachistoscopically. He found that each of his three systematic categories gave characteristically different thresholds: those for attention being smallest, cognition next, and apprehension largest. In the present study, Oberly's systematic analysis of the problem has been accepted and an attempt is made to determine the effect of color upon the statistical limens for each of Oberly's categories.

EXPERIMENTAL PROCEDURE

The introspective reports of the *Os* indicate that there are possible, and of very frequent occurrence, 3 conscious patterns which form the basis of the verbal report of the stimuli perceived. These 3 patterns were described as follows: (1) *Immediate.* All the dots were perceived in a single flash of consciousness, with an equal and high degree of clearness. (2) *Grouping.* All the dots were seen in a single flash of consciousness, but owing to different degrees of clearness, there was an attentional grouping of the stimulus dots. Both of the 2 conscious patterns characterized above, however, gave rise at once to the verbal report without any mediating conscious processes. (3) *Counting.* This process was different from either of the other two, inasmuch as the report was always made on the basis of some subsequent re-imaging of the stimulus material. The mediating process varied from *O* to *O* and for the same *O* from time to time. The processes consisted frequently in visual images, the dots of which were then counted verbally; or of a verbal counting with ocular-motor fixation of the position of the dots or other similar sorts of processes. Also, all of the *Os* reported wide differences in the degree of subjective assurance with which the judgments were reported.

Apparatus. Three sets of gray cards 87 mm. square were prepared. From 2 to 12 round dots of each color (6.5 mm. in diam.): blue, green, yel-

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[1] Cf. S. W. Fernberger, A preliminary study of the range of visual apprehension, this JOURNAL, 32, 1921, 121-133.

K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 183-230.

[2] H. S. Oberly, The range for visual attention, cognition, and apprehension, this JOURNAL, 35, 1924, 332-352.

low or red of the Hering papers in each set, were pasted in haphazard arrangements on the gray backgrounds. Each set was homogeneous. All of these were exposed by means of a Whipple tachistoscope with an exposure of 40 sigma. There were 44 cards in each set, making 132 in all. The order of the stimulus cards was different in each of the three sets and was arranged in haphazard fashion. After the exposure of all the cards, they were turned 90° so that the right edge of each card was now the bottom of the card for the next group of exposures. This method of rotating the cards was continued throughout the experiment. Thus any stimulus card was seen in the same (identical) position only after each 528th exposure. In this manner and also by starting at the different ends of each series the probability of the *O*'s memorizing the order of the series or of his recognizing any particular card was very much reduced.

The photographic dark room of the laboratory was used in order to keep the illumination on the stimulus card and the general illumination constant, the only source of light being that of the exposure lamp. The *O* was seated 1.9 m. from the pre-fixation screen. Each *O* gave 2,200 judgments for each fractionated series, making 4,400 total, or 400 judgments for each number of dots, 100 judgments for each color of each number of dots.

Instructions. The directions which were read to the *O* at the beginning of each experimental sitting were as follows: "You will be shown successively, for a very brief exposure, cards which contain a varying number of dots. Look at these and report as follows.

"(1) The number of dots you have seen.

"(2) The degree of assurance or certainty of your judgment, using the following scale for your report: 5—if it is a 100 to 1 bet, 4—if it is a 5 to 1 bet, 3—if it is a 1 to 1 bet, 2—if it is a 1 to 5 bet, 1—guess or do not know.

"(3) An introspective characterization as follows: (1) *Immediate*, which means that the dots were perceived in a single flash of attention and were ungrouped. (2) *Grouping*, which means that the dots were seen in a single flash of attention but were grouped. (3) *Counting*, which means that the dots were re-imaged in some fashion, subsequent to the exposure, other than for verification, and were subsequently counted."

Each *O* was given some practice in following these directions before the record was taken of his judgments.

It is obvious that the introspective characterizations do not correspond to the systematic categories that have appeared in the psychological literature.[3] It was necessary therefore to transform the results obtained into some form of systematic categories for the purpose of study and of calculation. From a study of the literature it seems that the introspective characterization which has been described above and which appears in the instructions as 'immediate' corresponds to one-level attention. These results have therefore been calculated under this heading.

The term 'cognition' as it appears in the psychological literature seems to be a process which is immediate but which does not imply only a single level of clearness. The principal aspect which characterizes cognition is its immediacy. It will be observed that the introspective categories of *Immediate* and *Grouping* are both characterized by their immediacy, although the second is further characterized by different degrees of clearness which lead to an attentional grouping. The results of these two introspective categories have therefore been combined under the title of cognition.

The term 'apprehension' as it appears in the psychological literature signifies the obtaining of a knowledge of the stimulus without restriction of the processes underlying such knowledge. All three of the introspective categories have therefore been combined for purposes of calculation for this systematic concept. In other words, the results of both *Immediate* and *Grouping*, as the two ways of getting knowledge without subsequent process, have been combined for *Counting*, which involves such a mediating process.

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[3] This argument follows Oberly's analysis noted above.

The results for Attention consist solely of those which were characterized as immediate and ungrouped by the *Os*. The results for Cognition are those which the *Os* characterized as immediate plus those characterized as grouped; and finally, the results for Apprehension are a combination of the results which were characterized by the *Os* under all three introspective headings.

It was arbitrarily determined to use for purposes of calculation only those reports which were correct and which had an assurance of 5, 4, or 3. This means that the *O* was sure enough of his report to put even money on it, and this procedure eliminates the doubtful judgments. Therefore, judgments with the assurance of 2 or 1 were eliminated from the calculations, but not from the reports. Two *Os* (YN and MM) worked in entire ignorance of this elimination.

The results for each of the 3 systematic categories were fractionated into 2 groups of 50 judgments for each value of the stimulus in the order taken. The calculations were made in accordance with the phi-gamma hypothesis, and these calculations were simplified by the use of Urban's Tables. Such a calculation yields a statistical threshold as well as a statistical index of precision of the results, for each of our 3 systematic categories, for each of the 4 colors, and for each of the 3 degrees of assurance that were retained.

Observers. Four *Os* were chosen from the staff of the department. They were H. Sherman Oberly (SO), Stanley F. Cooper, (SC), Yale Nathanson (YN), and Miles Murphy (MM), Instructors of Psychology. Two of the *Os*, SO and SC, were well acquainted with the materials and the methods of exposure. YN and MM were trained in giving introspective reports in psychological experiments, but were not trained in this particular experiment.

RESULTS

The tables of the observed relative frequencies of correct judgments for each of the 3 systematic categories and for each of the 3 degrees of subjective assurance—16 tables in all—have been omitted in an effort to conserve space. In every case, the curves approached the ogive type, starting with high relative frequencies for the smaller values of stimulus and descending to small observed relative frequencies for the higher values of stimulus. Very few inversions are noted in the curves and those that do occur are slight. These observed relative frequencies were therefore treated statistically by the phi-gamma hypothesis assisted by the use of Urban's Tables.

Such a treatment yields two sets of values of statistical significance. (1) The statistical threshold or limen (S) which gives that value of stimulus which is correctly reported with a probability of 0.5 or which is as likely to be correctly reported as not. (2) An index of precision (h) which gives an index of the degree of scatter or of the steepness of the curve. These statistical values (S and h) will be found in Tables I to IV.

Each table contains the complete results for a single *O*. Each table is divided into three parts: the upper third containing the results for subjective assurance 5 alone; the middle third for assurance 5 and 4; and the lower third for assurances 5, 4, 3. The results for each color are found in two columns, the first giving the values of the limens (S) and the second the values of the indices of precision (h) respectively. In each third, the first 2 rows contain the results for Attention, the next 2 rows those for Cognition, and the last 2 rows those for Apprehension. The first and second series into which the results have been fractionated are indicated in each case.

TABLE I
Observer SO

Assurance	Category	Series	Blue		Green		Yellow		Red	
			S	h	S	h	S	h	S	h
5	Attention	I	4.06	1.07	3.90	0.75	4.43	1.15	4.56	1.15
		II	3.64	0.23	3.27	0.21	4.12	1.24	6.02	0.63
	Cognition	I	6.42	0.53	6.31	0.39	6.40	0.60	6.75	0.49
		II	7.39	0.44	7.49	0.45	7.69	0.59	8.12	0.46
	Apprehension	I	7.18	0.56	6.85	0.41	6.73	0.54	7.34	0.46
		II	8.28	0.45	7.99	0.48	8.32	0.54	8.73	0.39
5 + 4	Attention	I	4.06	1.07	3.90	0.75	4.43	1.15	4.56	1.15
		II	3.64	0.23	3.27	0.21	4.12	1.24	6.02	0.63
	Cognition	I	6.59	0.49	6.49	0.34	6.54	0.63	6.97	0.48
		II	7.46	0.44	7.49	0.45	7.77	0.58	8.21	0.46
	Apprehension	I	7.56	0.56	7.07	0.38	6.96	0.57	7.89	0.52
		II	8.38	0.36	8.25	0.45	8.49	0.53	8.97	0.42
5 + 4 + 3	Attention	I	4.06	1.07	4.00	0.91	4.43	1.15	4.56	1.15
		II	3.64	0.23	3.27	0.21	4.12	1.24	6.02	0.63
	Cognition	I	6.84	0.49	6.69	0.36	6.90	0.53	7.33	0.42
		II	7.76	0.38	7.98	0.32	7.91	0.56	8.61	0.39
	Apprehension	I	8.62	0.44	7.98	0.35	8.20	0.44	8.85	0.40
		II	9.02	0.33	9.01	0.45	8.93	0.44	10.31	0.38

The results with regard to the thresholds (S) come out very similar to those of Oberly. The thresholds for Attention are smallest, those for Cognition next, and those for Apprehension largest. This is true for all 4 *O*s and for all degrees of assurance. As one compares the results for the different *O*s there is surprisingly little variation; the average threshold for Attention being approximately 4, that for Cognition, 7 to 8; that for Apprehension of the magnitude, 9 to 10. As one compares the results of the first series with those of the second fractionated series, a certain amount of progressive practice is evident. The degree of increase in the threshold as the result of practice, however, is much greater for apprehension than for cognition and attention. This would indicate better methods of re-imaging the stimuli, and an improvement in the mediating process.

The examination of the size of the thresholds (S) for SO, Table I, indicates that for all 6 series, Attention, Cognition, and Apprehension, the limen for Red is invariably higher than that for any of the other colors. The position of the other colors is not quite so evident, although Green has certainly the lowest limen (second—1; third—5; fourth—12 of the total eighteen cases). Yellow and Blue are intermediate and not very different. Yellow, however, was in second place more often than Blue (Yellow—11 seconds; Blue—6 seconds). The average position of the colors seems clearly to be Red with the highest limen, Yellow, Blue, and Green with the lowest limen.

TABLE II
Observer SC

Assurance	Category	Series	Blue		Green		Yellow		Red	
			S	h	S	h	S	h	S	h
5	Attention	I	5.18	1.20	3.94	0.74	4.62	0.57	5.28	0.93
		II	5.73	1.15	5.12	1.72	5.00	1.68	5.42	2.84
	Cognition	I	7.36	0.54	5.24	0.34	6.44	0.30	7.16	0.51
		II	7.85	0.70	7.21	0.56	6.92	0.52	7.89	0.55
	Apprehension	I	7.40	0.54	5.33	0.32	6.49	0.30	7.33	0.50
		II	7.87	0.69	7.04	0.54	6.96	0.52	7.85	0.54
5 + 4	Attention	I	5.18	1.20	3.97	0.74	4.72	0.49	5.28	0.93
		II	5.73	1.15	5.12	1.72	5.00	1.68	5.42	2.83
	Cognition	I	8.10	0.54	5.90	0.32	7.43	0.41	7.85	0.43
		II	8.28	0.71	7.80	0.53	7.58	0.58	8.38	0.53
	Apprehension	I	8.43	0.53	6.40	0.27	8.01	0.27	8.35	0.40
		II	8.56	0.60	8.18	0.51	7.87	0.51	8.61	0.48
5 + 4 + 3	Attention	I	5.18	1.20	4.00	0.73	4.72	0.49	5.28	0.93
		II	5.73	1.15	5.12	1.72	5.00	0.17	5.42	2.83
	Cognition	I	8.02	0.54	7.89	0.42	7.77	0.48	8.05	0.51
		II	8.52	0.66	8.05	0.48	7.80	0.65	8.80	0.61
	Apprehension	I	9.82	0.34	8.25	0.27	8.84	0.38	9.89	0.35
		II	10.89	0.25	9.92	0.28	9.52	0.23	10.26	0.33

The results of SC, Table II, would indicate that Red and Blue are tied for first place with regard to highest limen, each being in the first position 9 times and in the second position 9 times. Average differences between Red and Blue, however, are not very great. These results hold for all 3 categories of Attention, Cognition, and Apprehension, and for all 3 degrees of subjective assurance. Similarly, Green and Yellow fall into another and significantly different group for third and last place. Green has the third highest limen 10 times and the fourth highest limen 8 times; Yellow has the third highest limen 8 times and the lowest limen 10 times. Again the sizes of these limens are not significantly different from those of the limens for Red and Blue.

In Table III, the results for YN would indicate that Red has clearly the highest limen for all the cases of judgment and for all 3 degrees of subjective assurance—the limen for Red being highest in 14 of the 18 cases and in second place 4 times. A somewhat anomalous condition was found with regard to the relative position of the limens for Yellow and Blue. If Attention and Cognition are alone considered, Yellow is clearly in second place, the score being 2 firsts, 8 seconds, and 2 thirds for Yellow, and 2 seconds with 10 thirds for Blue. The values, however, are not significantly different. In the case of Apprehension, Blue seems clearly in second place with 2 seconds and 4 thirds for Blue as against 2 firsts and 4 fourths for Yellow, but again, the differences are not significantly different. Green very clearly gives the lowest limen, being lowest 14 times, second twice and third twice.

TABLE III
Observer YN

Assurance	Category	Series	Blue		Green		Yellow		Red	
			S	h	S	h	S	h	S	h
5	Attention	I	4.32	0.42	4.22	0.58	4.45	0.43	4.46	0.40
		II	5.24	0.68	4.86	0.52	5.30	0.54	5.45	0.67
	Cognition	I	6.14	0.36	6.11	0.33	6.82	0.29	6.41	0.33
		II	7.04	0.30	6.80	0.23	7.11	0.32	7.54	0.26
	Apprehension	I	6.57	0.42	6.32	0.34	7.15	0.39	7.11	0.25
		II	8.16	0.48	8.14	0.44	7.99	0.44	8.81	0.43
5 + 4	Attention	I	4.26	0.36	4.21	0.44	4.47	0.43	4.50	0.41
		II	5.24	0.68	4.86	0.52	5.30	0.54	5.45	0.67
	Cognition	I	6.95	0.28	6.62	0.28	7.15	0.31	7.02	0.31
		II	7.50	0.25	7.45	0.19	7.49	0.27	8.17	0.21
	Apprehension	I	7.59	0.35	6.95	0.29	8.53	0.38	7.98	0.43
		II	8.58	0.39	8.71	0.41	8.45	0.41	9.37	0.41
5+4+3	Attention	I	4.33	0.36	4.26	0.43	4.47	0.43	4.51	0.40
		II	5.24	0.68	4.86	0.52	5.30	0.54	5.35	0.67
	Cognition	I	7.38	0.25	7.15	0.25	7.40	0.30	7.51	0.25
		II	8.14	0.17	7.75	0.16	7.88	0.21	9.99	0.19
	Apprehension	I	8.26	0.30	8.45	0.35	7.95	0.34	8.48	0.30
		II	9.28	0.35	8.91	0.36	8.82	0.36	9.87	0.36

In the case of MM, Table IV, there is slightly more scatter than will be observed in the results of the other *Os*. However, Red gives, in general, clearly the highest limen, being in first place 11 times, in second place 7 times, and in third place only once. Similarly, Green gives in general, the lowest limen, being lowest 10 times, in first place 4 times, and in third place 4 times. These thresholds for Red and Green on the whole would seem to be significantly different from one another. Blue and Yellow are clearly intermediate. It is with difficulty that one would determine which had, in general, the highest values. The score stands, Blue first, 3 times; second, 5 times; third, 4 times; and lowest, 6 times. Yellow gives the highest limen once, the second highest, 6 times, third highest, 9 times, and the smallest limen, twice.

TABLE IV

Observer MM

Assurance	Category	Series	Blue		Green		Yellow		Red	
			S	h	S	h	S	h	S	h
5	Attention	I	3.94	0.48	4.27	0.71	4.14	1.14	4.26	0.97
		II	4.61	1.80	4.59	1.86	4.71	2.07	4.90	1.28
	Cognition	I	6.91	0.80	6.58	0.73	6.65	0.71	6.82	0.74
		II	7.54	0.81	7.83	0.68	8.02	0.66	8.93	0.36
	Apprehension	I	6.99	0.79	6.63	0.76	6.85	0.67	6.86	0.77
		II	8.26	0.75	7.96	0.68	7.17	0.21	8.54	0.68
5 + 4	Attention	I	4.05	0.50	4.14	0.60	4.16	1.10	4.26	0.93
		II	4.61	1.80	4.59	1.86	4.71	2.07	4.90	1.28
	Cognition	I	7.25	0.79	6.97	0.67	7.09	0.73	7.23	0.70
		II	9.10	0.95	8.47	0.73	9.45	0.55	9.04	0.43
	Apprehension	I	7.98	0.46	8.76	0.63	7.92	0.59	8.17	0.63
		II	9.03	0.89	8.22	0.57	8.84	0.70	9.58	0.41
5 + 4 + 3	Attention	I	4.01	0.50	4.34	0.60	4.16	1.10	4.28	0.93
		II	4.61	1.80	4.59	1.86	4.71	2.07	4.90	1.28
	Cognition	I	7.24	0.84	7.02	0.63	7.19	0.72	7.33	0.65
		II	8.86	0.96	8.56	0.71	8.88	0.55	9.04	0.43
	Apprehension	I	9.09	0.67	8.67	0.49	8.66	0.59	9.24	0.55
		II	9.96	0.68	10.99	0.25	10.09	0.42	10.60	0.48

Table V clearly indicates the relations discovered with regard to the relative magnitude of these threshold averages.

In every case, Red in general gives a high limen and Green a low one. Yellow and Blue have a tendency to be intermediate and variable. This situation is in general true for all 3 systematic categories and for all 3 degrees of subjective assurance for every *O*.

TABLE V

O	Relative positions of limens			
	Highest	2nd	3rd	Lowest
SO	R	Y	B	G
SC	(R..........	..........B)	(G..........	..........Y)
YN	R	(Y..........	..........B)	G
MM	R	(B..........	..........Y)	G

For SC and SO, trained *Os*, there is also observable a characteristic difference between the size of the coefficient of precision (h) for Red and Green. The coefficients for Red are almost invariably larger than those for Green; the individual variations, however, not being exceedingly dissimilar. No such relationship can be discovered for the coefficients for Blue and Yellow. For the untrained observers, YN and MM, however, no such relationship in the size of the coefficient of precision is noticeable.

DISCUSSION

Introspective reports were given by all the *Os* frequently during the experiment. An analysis and consideration of these reports did nothing more than indicate to the *E* that the conditions for the formation of the different judgments remained constant throughout the experiment. There is nothing in the introspective reports which throws any light upon the threshold differences which have been discovered for the different colors. The explanation of these results might depend upon several possible facts.

(1) *Intensity of the colors.* It would seem unlikely that the intensity of the colors would be the correct explanation in the light of the results obtained by Cooper in which he discovered that no constant effect was produced in 3 of his 4 *Os* when he exposed materials of Black, Medium Gray, and Light Gray on a White background.[4] His method and procedure was the same as that indicated in the present study; the only difference being the nature of the materials exposed. If intensity of the present materials were to give the effect indicated, it would be obvious that a still greater effect would have been observable in the uncolored materials which Cooper employed.

(2) *Saturation of the Stimuli.* It will be remembered that in the set of Hering papers, Red and Blue are highly saturated while the Yellow and Green are less highly saturated. Saturation might account for the relationship position of Red giving the highest limen and Green giving the lowest limen, but when one considers the unsaturated nature of the Yellow stimulus and its high position on the liminal scale, it would seem that such an explanation is highly improbable.

(3) *Anklingen times for the different colors.* It will be remembered that Kunkel has discovered that the order of rapidity of the *Anklingen* times gives Red as fastest, Green as slowest, with Blue intermediate.[5] The actual times he gives are, for Red 0.057 sec., Blue 0.091 sec., and Green 0.133 sec. Such a situa-

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[4] S. F. Cooper, The effect of brightness in the range of attention experiment, this JOURNAL, 40, 1928, 254-274.

[5] Kunkel, *Arch. f. d. ges. Physiol.*, 9, 1874, 197.

tion would explain very obviously the threshold differences when we consider that we are dealing with materials tachistoscopically exposed for an exposure of approximately 40 sigma. More recently, however, Bills has shown that the order of the *Anklingen* times varies considerably as the result of the methods employed and as a result of the intensity of the stimuli.[6] Bills does find, however, that when one approaches threshold values of stimuli, the order is Red highest, Yellow, Blue, and Green.

We are not prepared to state dogmatically that this is an explanation of our data. It would seem, however, as if the differences in *Anklingen* were a very possible explanation when one considers that the tachistoscopic exposure was shorter than the time necessary for any of these colors to have reached their perceptual maximum.

SUMMARY

(1) The judgments for the range of Attention, Cognition, and Apprehension fall into curves of the phi-gamma type for red, yellow, blue, and green stimuli.

(2) The limens for Attention are smallest, for Cognition intermediate, and for Apprehension the largest, in confirmation of Oberly's results with uncolored stimuli.

(3) The limens for Red are constantly largest for each of the three systematic categories and for the different degrees of subjective assurance. The limens for Green are similarly smallest, with those for Yellow and Blue intermediate.

(4) No such relation is discovered with regard to the magnitude of the values of the coefficients of precision.

(5) Brightness and saturation differences do not seem to account for these liminal differences, and the *Anklingen* times of the colors possibly offer a more satisfactory explanation.

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[6] M. A. Bills, The lag of visual sensation in its relation to wave lengths and intensity of of light. *Psychol. Mon.*, 28, 1920, no. 127, 1-101.

THE RELATION OF THE INTELLIGENCE OF PRE-SCHOOL CHILDREN TO THE OCCUPATION OF THEIR FATHERS

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The existence of a rather marked relationship between the results of intelligence tests and the social status of the individuals tested was observed at a very early period in the history of intelligence testing. A report by Decroly and Degand[1] giving the results of the application of the Binet 1908 scale to 45 children of superior social status attending a private school in Paris showed that none were below Binet's standards for their respective chronological ages, only 20% tested 'at age,' while the remaining 80% tested from one to three years 'above age.' In a critical discussion of these findings, Binet[2] calls attention to the marked difference in the social background of these children as compared to the groups used in the establishment of his norms and suggests that this may be an explanation of the apparent discrepancy.

Similar findings have been reported by other workers, both in this country and abroad. The earlier reports are as a rule based upon rather loose descriptive classifications such as "children in a good neighborhood as compared to those in a poor neighborhood," while the more recent studies are likely to adopt some more nearly objective system of classification, such as racial stock, amount of education received, occupation, income, etc. As a simple and easily ascertained basis for classification, the kind of occupation engaged in possesses a number of points of superiority. Since it forms a part of nearly all records, embarrassment or hesitancy is rarely encountered when the information is asked for directly, and in case there is no direct contact with the individual, the data can usually be obtained from other sources. Statistical errors due to incomplete or inaccurate data are thus reduced to a minimum. Moreover, accurate comparative figures for any locality may readily be compiled from the census reports, a matter which is obviously of much importance. A knowledge of the occupation also makes possible a rough estimate of the financial status of the individual, and of the amount of education which he is likely to have received.

During recent years a fairly large number of studies dealing with the relationship between intelligence and occupation have appeared. These studies may be classified under four main heads, as follows:

(1) Studies based upon the judgment of supposedly competent individuals. Of these, the Barr scale for occupational intelligence is fairly representative.[3] One hundred carefully defined occupations were ranked

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*Accepted for publication June 20, 1927.

[1] O. DeCroly and J. Degand, La mesure de l'intelligence chez des enfants normaux d'après les tests de MM. Binet et Simon, *Arch. de psychol.*, 9, 1910, 81-108.

[2] Alfred Binet, Nouvelles récherches sur la mesure du niveau intellectual chez les enfants d'école, *Année psychol.*, 17, 1911, 145-201.

[3] F. E. Barr, A scale for measuring mental ability in vocations and some of its applications, *Stanford Univ. Publ.*, 1918. Also described in Terman's *Genetic Studies of Genius*, I, 1926, chap. iv.

by thirty independent judges according to the comparative amount of intelligence deemed necessary to achieve an average degree of success in each. These judgments were then converted into equivalent P.E. units by the usual statistical procedure, and the results arranged in the form of a rating scale for occupations.

(2) Studies of the social origin of famous men. Galton's *Hereditary Genius* is one of the earliest of these.[4] Among the more recent studies may be mentioned those by Cattell[5] and Cox.[6]

(3) Studies based upon the actual examination of persons employed in different occupations. Of these, the reports from the Army Intelligence Tests have been most widely quoted.[7] A number of special studies of various groups of civilian employees have also appeared, and there have been several reports based upon the examination of unemployed men. Reports on the relative standing of students in training for various types of occupation might also be included.

(4) Studies in which the performance of school children on standardized tests of intelligence is compared with the occupation of their fathers. Studies which compare the intelligence-test standing of children coming from contrasted social classes less specifically defined (as public versus private schools, good neighborhood versus poor neighborhood, etc.) also belong under this head, since the factors which determine social segregation of the kind mentioned are largely dependent upon paternal occupation.

In spite of minor variations which are probably due in the main to differences in employment standards, to fluctuations in industrial conditions at different periods or in different localities, and similar circumstances affecting the selection of the groups, the essential nature of the results is the same. Whether the comparison is made between persons actually engaged in the occupations considered or their offspring, whether the data are gathered here or abroad, whether individual tests or group tests are used, it is invariably found that the individuals belonging to the professional classes exceed those belonging to the laboring classes in average standing on practically any of the recognized tests of intelligence. Moreover, whenever selective factors can be so controlled as to secure equally representative samplings of the various occupational groups, it is found that fairly well defined hierarchies of mental ability have been established within broad occupational classes of a given type. Thus, expert accountants on the average rank higher than bookkeepers; bookkeepers exceed filing clerks. Skilled mechanics or electricians earn a higher average standing than boiler makers; boiler makers exceed railway track layers. University professors do better than city high school teachers, high school teachers exceed grade teachers. It is, of course, necessary to remember that these statements apply only to the average standing of the groups mentioned, and that these averages must be based upon a sufficient number of cases to insure a reasonable degree of reliability for the findings. There is always a considerable degree of overlapping of scores for the various groups, or, in other words, there are many individual cases which show a marked departure from the average performance of their occupational class. These exceptions do not obscure the general tendency.

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[4] Francis Galton, *Hereditary Genius*, 1869.

[5] J. McK. Cattell, A statistical study of eminent men, *Pop. Sci. Mo.*, 62, 1903, 359-377; Families of American men of science, *ibid.*, 86, 1915, 504-515.

[6] C. M. Cox, The early mental traits of three hundred geniuses, in Terman's *Genetic Studies of Genius*, II, 1926.

[7] *Memoirs of Nat. Acad. Sci.*, 15, 1921, part 3.

At least three fairly distinct types of explanation have been put forth by way of accounting for these findings. The first ascribes the lower standing of the laboring classes to social heredity. It is said that these groups are made up largely of individuals who have lacked educational opportunity and intellectual stimulation. The low standing of their children is explained in like manner, since persons who are themselves lacking in intellectual accomplishments are not likely to provide intellectual surroundings for their children.

The second explanation is in some degree allied to the first, but the emphasis is placed upon the tests as related to the environment, rather than the environment alone. It holds that there are qualitative as well as quantitative differences in mental ability, and that these differences are primarily the outgrowth of differences in life experience. According to this theory, the intelligence tests in common use are unduly weighted in favor of the kind of performance best facilitated by the training and environment ordinarily found among the upper social classes; hence the lower standing of the laboring classes does not imply a lesser degree of ability, but merely that their ability has been developed along other lines not measured by the tests used.

The third explanation, while it recognizes that the factors just mentioned may have some bearing upon the results, nevertheless, places the major emphasis upon differences in native endowment, upon biological rather than social heredity. It takes the point of view that a greater degree of true mental ability is required for success in the learned professions than for success as a day laborer, and considers that industrial selection, rather than differences in opportunity accounts for the tendency toward a segregation of abilities in the various occupational groups. The results of this selective process are carried over into the next generation, since children tend to resemble their parents in mental as well as in physical traits.

Inasmuch as all the studies which have been reported up to the present time are based upon either adults or school children, it has seemed worth while to compare the findings with those obtained through a study of children of pre-school age recently carried out at the University of Minnesota Institute of Child Welfare. A group of 380 children, all of whom were between the ages of 18 and 54 months, were given the Kuhlman Revision of the Binet tests twice, the two examinations being separated by an average interval of six weeks. A brief application form, which was filled out for all the children, included a question as to the occupation of the child's father. The occupations of adult males between the ages of 21-45 years as listed in the 1920 census report for the city of Minneapolis had previously been classified into six main categories, using the Barr Scale[8] for occupational intelligence and the Taussig Industrial Classification[9] as guides. The percentages falling within each of these categories were then computed and the figures thus obtained have been used as a basis for determining the representative character of groups selected for study at the Institute.

The exact make-up of each of the six occupational categories is stated in the monograph describing the results of the tests.[10] The general nature of the classification may be indicated as follows:

Group I. Professional.

Group II. Semi-professional (accountants, draftsmen, etc.) and managerial.

Group III. Clerical and skilled trades.

Group IV. Semi-skilled trades and minor clerical.

Group V. Slightly skilled.

Group VI. Unskilled labor.

The 380 children included 190 boys and 190 girls. Taking age to the nearest birthday, there were 122 two year olds, 126 three year olds, and 132 four year olds. The distribution as to occupational categories is almost identical for the three age groups. The mean IQ earned by the children of the various occupational categories on each of the two examinations given is shown in the following table:

TABLE I

MEAN IQ OF CHILDREN WHOSE FATHERS BELONG TO DIFFERENT OCCUPATIONAL CATEGORIES

Examination	Group					
	I	II	III	IV	V	VI
First	116.1	111.7	107.7	105.3	104.3	96.0
Second	125.0	119.7	113.4	108.0	107.4	95.8
Number of cases	56	29	129	79	48	39

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[8] *Op. cit.*

[9] F. W. Taussig, Industrial classification of occupations, in Terman's *Genetic Studies of Genius*, I, 1926, chap. iv.

[10] F. L. Goodenough, The Kuhlman-Binet tests for children of pre-school age; a critical study and evaluation, *Univ. Minnesota Inst. Child Welfare, Mon. Series*, 2, 1928.

Inspection of Table I reveals two notable characteristics. Not only do both examinations show a steady decrease in the average IQ of the children as we go down the scale of paternal occupations, but this tendency is distinctly more marked in the results of the second examination than in those of the first examination. The fact is one of importance, since it was shown in the course of the main study that the second examination is the more valid of the two measurements. The generally high ratings are due to inaccurate standardization of the tests at the early age levels.

It was felt that it would be interesting to compare these findings with the results reported by other investigators. Three studies were selected for this comparison: the Barr Scale values using the frequencies reported for the city of Minneapolis in the 1920 census, the distribution of Alpha scores for the white draft,[11] and Haggerty and Nash's study of elementary school children in New York State.[12] In the case of the latter two studies, the farming group was excluded, since our data are based entirely upon city children, and it was desired to make the material as nearly comparable as possible. Where medians only were given, these have been treated as the equivalent of means, a fact which undoubtedly introduces a slight though unavoidable error. The standard deviations are based upon the original distributions which include the farmers, since the data were not presented in such a form as to make it possible to correct for the withdrawal of this group. (The distributions of scores for the separate occupations are not shown in either report.) In both instances, the true variability of the group included is presumably somewhat smaller than that of the total group from which this figure was derived.

In order to facilitate comparison, the findings from each study have been converted into standard x/σ scores. In the case of the Army and the Haggerty data, these scores are probably reduced somewhat below the value which would have been found if the farmers had not been included in the distributions from which the standard deviations were derived. The results are shown graphically in Fig. 1. The data of Fig. 1 are given in Table II.

Since both the Barr Scale values and the Army scores have reference to persons actually engaged in the various occupations, although the former is based merely upon a consensus of the opinions of certain individuals and the latter is derived from empirical data, while the Haggerty and the preschool material have to do with the offspring of these persons, it is to be expected that the former would show a much clearer separation of the various occupational classes than the latter, as a result of the law of filial

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[11] *Memoirs Nat. Acad. Sciences*, 15, 1921, part 3, 547-877.

[12] M. E. Haggerty and H. B. Nash, Mental capacity of children and paternal occupation, *J. Educ. Psychol.*, 15, 1924, 559-572.

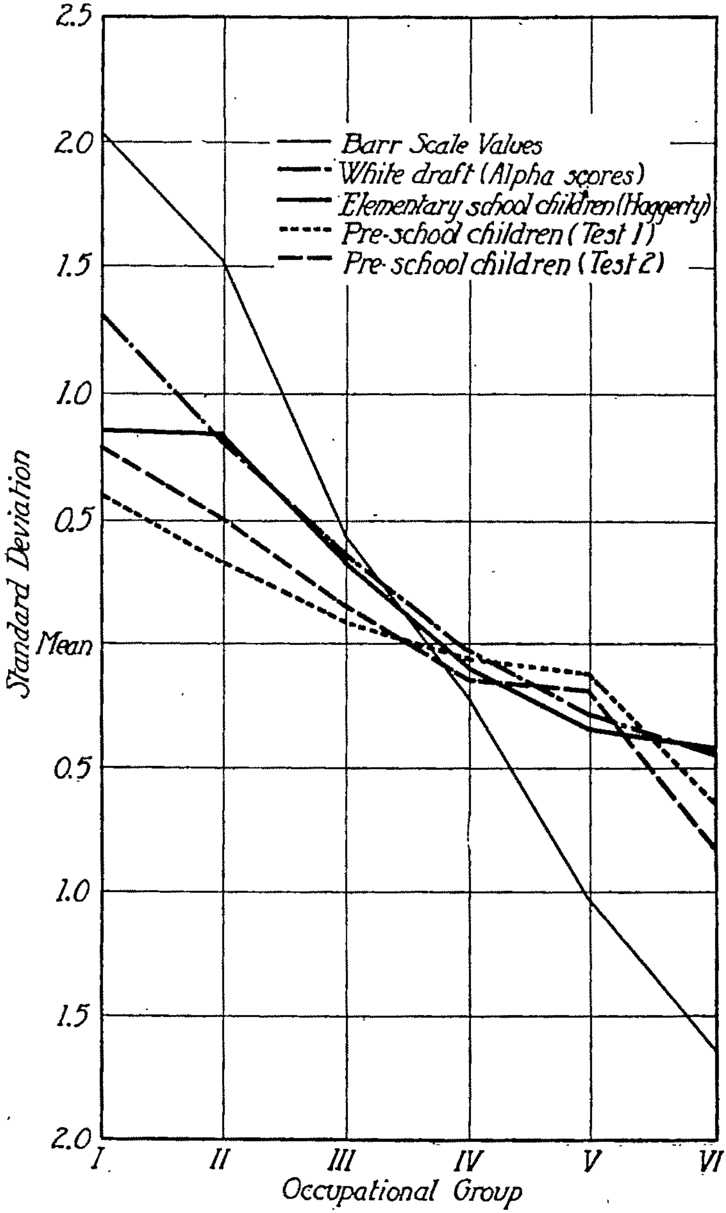
Barr Scale Values
White draft (Alpha scores)
Elementary school children (Haggerty)
Pre-school children (Test 1)
Pre-school children (Test 2)
Standard Deviation
2.5
2.0
1.5
1.0
0.5
Mean
0.5
1.0
1.5
2.0
I
II
III
IV
V
VI
Occupational Group

FIGURE I

TABLE II
COMPARATIVE STANDING OF THE DIFFERENT OCCUPATIONAL GROUPS ON THE VARIOUS MEASUREMENTS IN TERMS OF RAW SCORES AND THEIR x/σ EQUIVALENTS

	I		II		III	
Measurement	Raw mean	x/σ	Raw mean	x/σ	Raw mean	x/σ
Barr Scale Values	15.65	+2.04	14.01	+1.53	10.49	+0.44
Army Alpha Scores (White draft)	113.75	+1.32	94.68	+0.82	76.83	+0.36
Haggerty elementary school Mean IQ	113.4	+0.86	113.2	+0.85	103.1	+0.32
Minneapolis pre-school, Test 1 Mean IQ	116.1	+0.60	111.7	+0.33	107.7	+0.09
Minneapolis pre-school, Test 2 Mean IQ	125.0	+0.80	119.7	+0.50	113.4	+0.15

	IV		V		VI		Average	
Measurement	Raw mean	x/σ	Raw mean	x/σ	Raw mean	x/σ	Mean dis.	σ dis.
Barr Scale Values	8.38	−0.21	5.71	−1.03	3.74	−1.63	9.03	3.25
Army Alpha Scores (White draft)	62.25	−0.02	52.43	−0.28	46.70	−0.43	63.12	38.36
Haggerty elementary school Mean IQ	95.1	−0.09	90.4	−0.34	89.0	−0.41	96.9	19.2
Minneapolis pre-school, Test 1 Mean IQ	105.3	−0.06	104.3	−0.12	96.0	−0.64	106.3	16.2
Minneapolis pre-school, Test 2 Mean IQ	108.0	−0.14	107.4	−0.18	95.8	−0.82	110.6	18.1

regression. Inspection of the curves will show that the slope of the line for the Barr Scale ratings is much steeper than for any of the others, but that for the army scores is only slightly greater than the ones for the elementary school or the pre-school children. These facts call for further consideration.

It is quite probable that there was a constant tendency on the part of the individuals who made the ratings for the Barr Scale to exaggerate the differences in the intellectual requirements of the various occupations, and that the unusually marked slope of this curve is in part a function of this tendency. On the other hand, it is practically certain that the distribution of Alpha scores for the white draft does not afford an adequate basis for comparison of the occupational extremes, since this distribution does not include the illiterates who took Beta, practically all of whom were found in the lower half of the occupational classification, and whose intellectual level, on the average, was distinctly below that of the group who took Alpha. In the case of the day laborers (Group VI) the median Alpha raw score for those who took Alpha was 46.7, or a C rating; the median Beta weighted score for those who took Beta was 65.8. A rough translation of the Beta score into an equivalent value in terms of Alpha would place it not far from 20.0 points, as compared to 46.7 for those belonging to the same occupational class who took Alpha. Since this group includes 1453 cases, the likelihood of obtaining a difference of this magnitude by chance is negligible. That the selection due to the non-inclusion of the group who took Beta is not equally distributed over the different occupational categories is amply demonstrated by a comparison of the percentages in each occupational class who took Beta as compared to the percentages who took Alpha. In Group I, 5.4% took Beta, 94.6% Alpha; in Group II, 5.3% took Beta, 94.7% took Alpha; in Group III, 7.8% took Beta, 92.2% Alpha; in Group IV, 18.5% took Beta, 81.5% Alpha; in Group V, 24.7% took Beta, 75.3% Alpha; in Group VI, 32.7% took Beta, 67.3% Alpha. It is obvious that in a comparison of the standing of different occupational classes based upon Alpha alone, the lower classes are less clearly differentiated from the general population than would have been the case if the men taking Beta had also been included.[13]

A similar lack of differentiation exists in the Alpha scores at the upper end of the occupational distribution. This is in large measure due to the large proportion of professional men who volunteered for the officers' training camps and were therefore not included in the draft army. It should also be noted that physicians and dentists, who constitute one of the large professional classes, were drawn into a special service. It is true that the average standing of the medical group was somewhat below that of the generality of white officers, a fact which, as Cobb and Yerkes have shown, was probably due to selective factors.[14] Nevertheless, even this group ranks decidedly above the men we have classified as Group I in the draft army. The median score of the medical group was 129, of the dental group 117, of white officers in general 139, of Group I in the draft army 113.75. Since Group I is supposedly made up of men belonging to the professional classes for which college training is ordinarily an essential, it is not out of place to compare these figures with the many reports for college students to whom the Alpha tests have been given. Data for more than twenty colleges and universities throughout the country have been summarized by Pintner.[15] The medians for undergraduates average about 135, varying somewhat with the college year (whether freshmen, sophomore, etc.) and with the type of institution. For graduate students in universities of good standing the median is usually above 150. For 97 men in the draft army sample who reported college graduate work it was 146.

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[13] In the table given in the *Memoirs* showing the standing of the men according to occupation, the results for the Beta group are given in terms of weighted scores, those for the Alpha group in raw scores. The reason for this is not clear, since practically all other tables dealing with Beta, as well as those for Alpha are in raw scores, and the table of equivalents used for converting scores earned in one test into their most probable value in terms of another test includes only the raw score values. An attempt to include the Beta group in the distribution according to occupational categories used in this study was discontinued because of inadequate data upon which to base the conversion of scores. The estimate for the laboring group given above is derived by interpolation in the scale of letter grades. It is probably not entirely accurate; but it is unlikely that the error amounts to more than one or two raw score points.

[14] M. V. Cobb and R. M. Yerkes, Intellectual and educational status of the medical profession as represented in the U. S. Army, *Bull. Nat. Research Council*, 1, 1921, part 8.

It is of course true that the inclusion of the volunteer officers and the Beta group would have somewhat increased the standard deviation of the distribution, but this increase would certainly have been small in proportion to the change in the means at the occupational extremes. In the case of our data, the tendency would be somewhat counterbalanced by the omission of the farming group, as was stated in an earlier paragraph.

Other selective factors affecting the army occupational data should be mentioned. Because of the youth of the drafted men, a large proportion of them had not yet settled down to a life work. Very many of the occupations reported were undoubtedly of a temporary nature and would be considerably modified later on through the operation of industrial selection and personal initiative. This has probably resulted in a tendency toward a flattening of the curve beyond the position which it might fairly be expected to assume later on. Differential selection of men for the 'preferred' occupations in civilian life must also be considered, as well as the possibility that there was some systematic tendency for the men to falsify their statements on occupation in an attempt to secure admission to particular arms of the service. These and other similar points are discussed at some length in Part III of the *Memoirs*.

It appears highly probable, therefore, that the curve based upon the army data is considerably flatter than would be the case if the average age of the men included had been greater, and if the selective agencies mentioned had not been operative. Whether it would have reached the position indicated by the Barr Scale is impossible to say; though from the evidence at hand it seems unlikely that the change would be of quite this magnitude. That the comparative absence of filial regression when the army scores are compared with those derived from the two groups of children is an artifact due to selection seems, however, reasonably certain.

The Haggerty scores show practically no difference between occupational Groups I and II. This also is presumably a result of unequal selection of cases. It was stated in an earlier paragraph that this group included both rural and city children. In order to make the distribution of cases conform more nearly to our group of Minneapolis children, the farmers' children have been omitted in working up the data. While this would exclude the greater number of the rural children, Group I, which is the professional class, would still contain the children of country doctors, school teachers, etc. Because of the strong competition in the cities, there is always a tendency for the less competent members of the professional groups to be forced out into the rural districts, where the cost of living is lower and the competition is less keen. The average rating of the children in the Haggerty Group I is quite probably lower than would be the case had it been made up exclusively of city children from the professional classes. Group II, on the other hand, includes no occupations which are likely to be found in the rural districts, hence the country children are automatically excluded.

The points just mentioned are illustrative of the sort of error which is likely to enter into material of this kind. In spite of minor inconsistencies, however, the five curves in Fig. 1 show a general similarity which is indeed surprising in view of the fact that they are based upon data derived from such widely different population groups, examined by different methods and under quite dissimilar conditions. In every instance, the mean falls between occupational Groups III and IV; a point which coincides with its theoretical position according to the census data for Minneapolis which probably do not differ greatly from those for other northern cities. The upper three groups include 49% of the male population of that city, the lower three include 51%. The mean, therefore, should be located between Groups III and IV as was stated above. It should also be slightly nearer Group IV than Group II, and a glance at Fig. 1 will show that this expectation also is verified. In each of the separate curves the mean for each successive occupational category is lower than that for the one immediately preceding, although in the case of Haggerty Groups I and II the difference is very small. Moreover, in the upper half of the distribution, there is no crossing of the curves based on the different investigations. In the lower half, some slight irregularities occur, chiefly in the case of the army data and that obtained by Haggerty, where Group VI is less clearly differentiated than is the case in the remaining three curves.

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[15] Rudolf Pintner, *Intelligence Testing*, 1923.

In working up the pre-school material, we have presented our findings for the three age groups combined, but treatment by ages separately showed no reliable tendency for the difference between occupational classes to vary with age. Taking the figures at their face value, the clearest separation of the occupational groups was found among the two-year-olds, but this variation from the findings for the children of three and four years was well within the limits of chance. The difference between the means for the two occupational extremes (Groups I and VI) in the case of the first examination given to the pre-school children is almost identical with that shown by Haggerty's data for New York State elementary school children enrolled in grades 3-8. On the second examination, which we have elsewhere shown to be the more reliable of the two measurements,[16] the differences are even greater than those found by Haggerty. The correspondence of both these studies with the army report, making due allowance for filial regression and for the selective factors noted in preceding paragraphs, is also striking.

These data show that intellectual differences between social classes are well established by the ages of two, three, and four years. It is perhaps unnecessary to add the caution that the findings have to do only with the present generation, and with educational and industrial conditions as they are found in this country. Prior to the industrialization of society such differences were presumably much smaller than at present, although the studies by Clarke,[17] Cox,[18] Havelock Ellis,[19] and others show that some degree of segregation along these lines has been operative for a number of generations. The findings of Duff,[20] McDonald,[21] and others demonstrate the existence of a similar relationship between intelligence and occupation in European countries; though the differentiation appears to be slightly less marked than in this country. It is not unreasonable to expect that with increasing specialization in industry and increasing equalization of educational opportunity, differentiation of occupational classes along intellectual lines will become even more marked in the future than has been the case in the past, though non-intellectual factors will always continue to play a considerable part.

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[16] Goodenough, *op. cit.*

The finding of such marked intellectual differences between social classes as early as the age of two years must be regarded as highly significant. The fact that no appreciable change takes place in the position of the various occupational groups from the age of two up to the close of the elementary school period affords strong evidence that the underlying factors, whatever may be their nature or origin, are non-cumulative in their relative effect upon mental development. Furthermore, while one may plausibly advance the hypothesis that the home of low cultural standards does not afford a sufficient opportunity or an adequate stimulus for the acquisition of the more complex and precise language concepts or the fund of general information necessary to achieve a notable degree of success with the tests standardized at the upper age levels, it is less easy to understand the process by which these factors serve as a handicap to the two year old who is judged upon the basis of his response to such simple commands as, "Throw the ball to me;" his ability to name simple objects; or to draw a rough circle with considerable help. Even the least cultured of modern city homes provide, it would seem, adequate opportunity for the acquisition of these early accomplishments of the young child. The fact that children of different social classes show as great differences in their performance of these extremely simple tasks as they afterward manifest in regard to the relatively complex problems of later life, lends support to the theory that under ordinary conditions of modern life, variations in mental growth are more directly dependent upon innate factors than upon differences in post-natal opportunity or stimulation.

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[17] E. L. Clarke, American men of letters: their nature and nurture, *Studies in History, Economics, and Public Law*, 1916.

[18] Cox, *op. cit.*

[19] Havelock Ellis, *A Study of British Genius*, 1904.

[20] J. F. Duff and G. H. Thomson, The social and geographical distribution of intelligence in Northumberland, *Brit. J. Psychol.*, 14, 1923, 192-198.

[21] Hector McDonald, The social distribution of intelligence in the Isle of Wight, *Brit. J. Psychol.*, 16, 1925, 123-129.

A COMPARISON OF THE SPANS OF 'ATTENTION' AND MEMORY

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INTRODUCTION

In a recent study[1] Guilford and Dallenbach review various methods which have been used to determine the span for immediate memory, and then present a study in which the method of constant stimuli was used to determine the threshold for the immediate memory of 5 *Ss*, and for a group of 100 *Ss*.[2] They conclude that "an exact determination is obtained more readily and more quickly [by this method] than by any of the other methods thus far proposed." Brotemarkle concludes a discussion of memory span problems at the college level with: "the memory span score is not sufficient in itself; it requires a complete analysis of the complexity of the mental processes involved, and assurance of the directness of response."[3] Miller states in his discussion of the diagnostic value of the memory span test that "in the case of higher scores the result has been exaggerated by means of grouping, and the factor of planfulness plays an important part."[4] We observe that Jones used five types of memory span tests in studying superior children in order that she might find the true 'operative' span. She states that "with intelligent subjects, grouping and other devices supplement the true operative span."[5] Easby-Grave also states that "it is possible . . . that the higher spans obtained from older children and adults are the result merely of grouping or organization, and not a true span."[6] Memory span, according to Humpstone,[7] is a test of associability which refers to the "number of discrete perceptions associated in a single act of attention, and the combination of the associated component parts of a single perception."[8]

Problem. The writer became interested in a comparison of the spans of 'attention' and 'memory' while working on the problem of the range for visual attention and the audito-graphic memory span for digits.[9] An inspection of the early results indicated the definite grouping of digits during the reproduction. This grouping had been observed and discussed many times in group experiments, but had never been studied individually. It was considered worthwhile to follow the suggestion of Guilford and Dallenbach to learn if there would be differences, and the nature of these differences, between the limens for the ungrouped memory span or 'attention span,' and the grouped 'memory span' as the test is generally known and used.

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[1] J. P. Guilford and K. M. Dallenbach, The determination of memory span by the method of constant stimuli, this JOURNAL, 36, 1925, 621-628.

[2] The stimulus value for which correct judgments are given in 50% of the cases.

[3] R. A. Brotemarkle, Some memory span problems, *Psychol. Clin.*, 15, 1924, 251.

[4] K. G. Miller, The competency of fifty college students, *Univ. of Penn. Exper. Studies in Psychol. and Ped.*, 8, 1922, 19.

[5] Alice M. Jones, An analytic diagnosis of one hundred twenty superior children, *Psychol. Clin.*, 16, 1925, 44-45.

[6] Charlotte Easby-Grave, Tests and norms at the six year old performance level, *ibid.*, 15, 1924, 279.

[7] H. J. Humpstone, Some aspects of the memory span: a study in associability, *Univ. of Penna. Exper. Studies in Psychol. and Ped.*, 8, 1917, 1-31.

[8] Lightner Witmer, *Analytical Psychology*, 1902, 251.

[9] H. S. Oberly, The range for visual attention, cognition and apprehension, this JOURNAL, 35, 1924, 332-352; Further results in the 'range for visual attention, cognition and apprehension' experiment, *ibid.*, 37, 1926, 132-138.

METHOD AND PROCEDURE

Material. One hundred and thirty series of digits were prepared. There were 10 series for every stimulus value from 2 to 14 digits inclusive. Brotemarkle warns that "the memory span test is of little value except when used by the examiner who has concisely arranged the stimulus with the intention of removing all inherent dangers from the span itself and from the manner of presentation."[10] The different series were arranged so that there would be eliminated, as far as possible, all successive or reverse order of digits as well as number associations; *i.e.* additions, subtractions, familiar dates, etc. An analysis indicates that the number 1 was the first of the series in 3% of the cases; 2, 17%; 3, 8%; 4, 13%; 5, 16%; 6, 23%; 7, 7%; 8, 14%, and 9, 9%.

Procedure. Successive series did not begin with the same digit. *E* read the series in regular order from the first to the last at the rate of approximately 1 per sec., and repeated the procedure until 50 observations had been obtained for every stimulus value. *E* read in a monotone, except for the last digit in the series which was accented as a signal for the end of the series. Inasmuch as the *Ss* could complete a few more than the prepared number of series in a single session, every session began with a different series.

Two methods of presentation, A and B, were used. In Method A the stimulus series of the various lengths were presented in haphazard order—care, however, being taken to see that series of the same length did not follow one another. In Method B the stimulus series were presented in regular or successive order, *i.e.* several series of 5 digits were presented, then several of 6, of 7, etc., in regular order up to and including the series of 12 digits. Method A was used with all our *Ss*; Method B with but one.

There were 5 experimental sessions with all the *Ss* assembled together, of 1 hr. each; two of these sessions were held at 5:00 P. M.; 2 at noon; and 1 at 8:00 A. M. A short rest was permitted the *S* at the end of half an hour's work.

An introspective report was obtained from every *S* after every experimental session. All the *Ss* worked without knowledge of the problem, and, in Method A, without knowledge of the length of the stimulus series.

Subjects. Seven members of the staff of the Department of Psychology acted as *Ss* in this experiment. Messrs. Sanderson (Sa), Humes (H), and Murphy (M), had the greatest amount of training in the introspective work. Messrs. E. M. Twitmyer (T), Altmaier (A), Irwin (Ir), and Gillespie (G) had had no previous experience in this type of work. Mr. Murphy (M) served as *S* in both methods, A and B, of presentation.

Instructions. The following instructions were read to the *Ss* at the beginning of every experimental session. "I am going to say some numbers to you and immediately after saying the last number, when I shall lower my voice, write the series of numbers on the paper which I have placed before you and cover it with the second sheet. Wherever possible, indicate the grouping of digits by means of brackets or spaces. The signal 'ready' will precede the reading of each series of digits."

RESULTS

The observed relative frequencies of the correct reports for the 'attention span' appear in Table I. These results were obtained without any indication of grouping on the part of the *Ss*. The 'memory span' results, which consist of the correct ungrouped as well as grouped reports, appear in Table II.

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[10] *Op. cit.*, 251.

TABLE I

ATTENTION SPAN

Showing for every *S* the percent of correct reports for every value of stimulus

S	Number of digits in stimulus series								
	2	3	4	5	6	7	8	9	10
Sa	100	100	98	16	6	0	0	2	0
H	100	100	88	22	10	4	2	0	0
M	100	100	100	96	98	88	50	10	0
T	100	100	100	20	12	6	2	2	0
A	100	100	100	26	4	2	4	0	0
Ir	100	100	100	34	16	20	8	4	8
G	100	98	98	84	50	16	2	0	0

TABLE II

MEMORY SPAN

Showing for every *S* the percent of correct reports for every value of stimulus

S	Number of digits in stimulus series												
	2	3	4	5	6	7	8	9	10	11	12	13	14
Sa	100	100	98	100	96	78	66	42	16	8	8	0	2
H	100	100	96	70	46	24	14	6	0	0	0	0	0
M	100	100	100	100	100	100	100	90	92	72	58	40	36
T	100	100	100	94	88	70	74	42	22	6	2	2	0
A	100	100	100	100	100	100	94	76	46	24	12	8	10
Ir	100	100	100	100	100	98	96	78	56	52	30	26	18
G	100	100	100	98	92	68	44	40	42	26	8	6	0

In Figs. 1-7 will be found the curves which have been constructed from the results appearing in Tables I and II. The solid line represents the correct judgments for 'attention span' while the broken line indicates the 'memory span' results. The curves for M, T, and G show a slight decrease for an odd number series with a subsequent increase for the next even series. This may be due to the method used in the 'memory span' test.

The values for L (limen) and h (precision) will be found in Table III. The correlation between the L and h values for 'attention span' when placed in rank order is striking, for M and G are the only *S*s to vary. For 4 of the *S*s, the value of h is greater for the 'attention span' than for the 'memory span.' When considering rank order for 'memory span,' the *S* with the lowest limen has the greatest value for precision. The highest limen is accompanied by the next to the lowest precision.

TABLE III

Showing for every *S* the spans of attention and memory

S	Attention Span		Memory Span	
	L	h	L	h
Sa	4.093	0.4039	8.640	0.3413
H	4.693	0.5822	6.032	0.4196
M	7.915	0.5902	13.666	0.2701
T	2.555	0.2433	8.466	0.3474
A	2.816	0.2835	10.161	0.3387
Ir	2.625	0.1592	11.071	0.2754
G	5.954	0.6346	8.701	0.2606

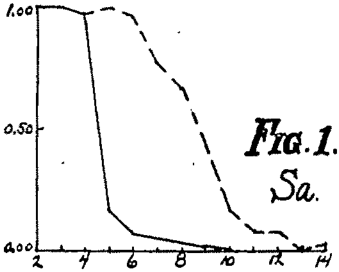


FIG. 1. Sa.

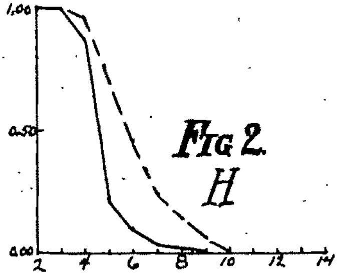
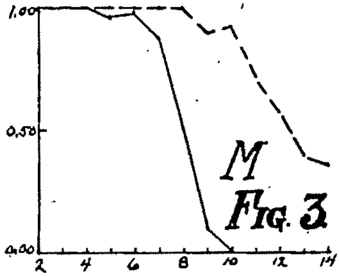


FIG. 2. H



M FIG. 3

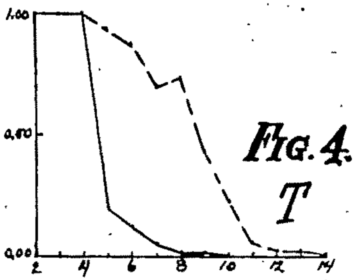


FIG. 4. T

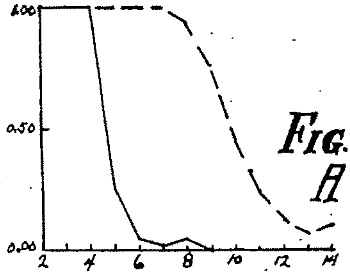


FIG. 5. A

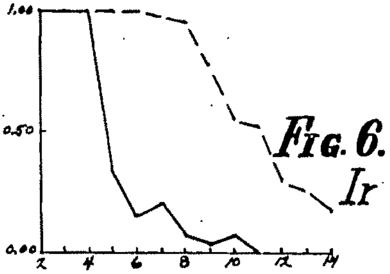


FIG. 6. Ir

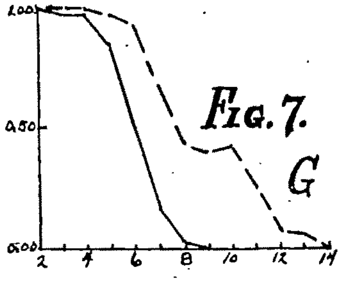


FIG. 7. G

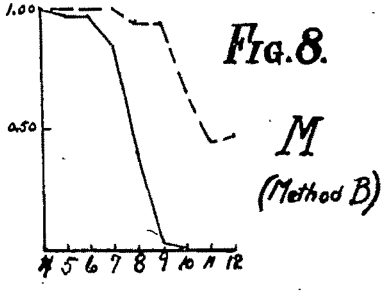


FIG. 8. M (Method B)

'Attention' ——— 'Memory' - - - - - -

The introspections, when considered individually, would indicate that the *E* read the stimulus series with a rhythm and accent. For example, at the end of the first session, Sa reports, "natural grouping of 3s seemed to be facilitated by the *E*'s reading." M reports, for the same session, "series up to and including 8 digits are reproduced at times without grouping in the reproduction." H states that he "made an effort to group" the digits. *S* A noted "decided grouping in practically every series." For the same session Ir however, states, "I generally group in 4s." G also grouped in 4s, but in a long series he "would often forget the first four digits." At the end of another session, Sa reports, "It seemed to me that the material was read in groups of 3s." A says, "Numbers seemed to be given in a rhythmical manner in 3s, probably due to my own grouping of the numbers." Ir writes, "*E*'s presentation seemed quite without rhythm."

When the introspections are considered collectively, they indicate that the rhythm was not of *E*'s making, that it was subjective, and they thus corroborate the conclusion of Gill and Dallenbach that "grouping is not dependent upon the objective arrangements of the stimuli as much as it is upon the subjective disposition of the *O*."[11] Gill and Dallenbach worked in the visual field, but their conclusion seems to hold also for the auditory field.

Several of the *S*s refer to their lack of preparedness for the series as they are given. Heretofore all had been familiar with the memory span test in which the number of digits is increased with each succeeding or second trial. T remarked on the first day that he "found it hard to get short series because I was prepared for more digits, and when they were not given there was a tendency—not confusion—toward a complete dropping out of consciousness of the numbers." Ir writes, "I sometimes missed 9 because I was expecting 12." G agrees that "if I am set for a long series and the series given is short, the abruptness throws me off and I forget the series."

In order to test whether the method of presenting the stimulus series affected the results a supplementary series of experiments was conducted with one *S* (M) with Method B. In this method, the usual one used in determining the memory span, the *S* could anticipate the number of digits to be given. The following results were obtained. M's 'attention' span with Method A was 7.915, with Method B was 7.566. His 'memory' span with Method A was 13.666, with Method B was 11.381. We do not consider these results final, for the range of the stimulus series in Method B was not as great as in Method A, but whatever advantage there is lies in favor of Method A and not of Method B, as our *S*s had supposed.

In order to obtain further information regarding the two methods, a group experiment was conducted with 100 *S*s. Stimulus series varying from 4-12 digits in length were presented to the group by the two methods and the percent of correct reports were determined for each. These data are given in Table IV. The group limen with Method A is 8.974; with Method B it is 8.929. The difference is slight, but it lies in the direction of Method A, and corroborates the results with M. An inspection of the data obtained from the group shows that 30 *S*s made the same score in the two methods, that 35 *S*s had larger spans with Method A, and that 35 *S*s had larger spans with Method B. We feel therefore that our method of procedure did not materially affect our results.

The degree of assurance was mentioned by 6 of the 7 *S*s. Sa remarks that "assurance was low when my attention was far from maximum." H stated that he had more confidence at the end of any series. M felt "sure of some series of 12 and 14. If I were able to command the maximum attention many more series could be reproduced correctly. My failures are often due to lack of attention." A noticed that he had "more assurance due to the building up of associations." At the end of the last session, Ir wrote, "I have felt increasingly confident with each hour of work." G noted the low degree of assurance toward the end of one session when he tried out a new method of grouping. On the whole, the assurance increased with the progress of the experiment, and fatigue which was mentioned in the first session was disregarded. H mentioned fatigue but once and only "after a decided effort to *avoid* grouping." All of the *S*s mention the fact that little attention was required for the shorter series of digits.

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[11] N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 256.

The grouping, that has been suggested by the investigators referred to above, is brought out rather definitely in an examination of the methods used by the *S*s. Sa mentions an effort to group in 3s (and sometimes 4s) in the first session. In the next session he notes that it was hard to group in 4s. Sa reports further, "without the grouping method I could work easily when the series was short. Any grouping but 3 is an effort, and often it is a distraction rather than an aid. My grouping represents a conscious effort to group the digits in preparation for the reproduction." One might observe that the results for Sa are not indicative of his attention span because of this effort on his part, but we note that he groups in 3s rather than 4s. His threshold for memory span (8.640) would indicate the probable attention span of three groups of three digits each.

TABLE IV

Showing the percent of correct reports for every value of stimulus for the haphazard (A) and successive (B) methods of presentation

Method	Number of digits in stimulus series									L	h
	4	5	6	7	8	9	10	11	12		
A	100	100	98	84	65	44	26	20	10	8.974	0.3392
B	100	100	96	90	69	45	25	12	5	8.929	0.4130

H repeatedly refers to his "conscious effort to group, but in no particular way. I tried grouping in 2s, but would frequently find other groupings apparently arranging themselves as by 4s or 5s." "This morning without apparent effort, groups of 3 began to appear in consciousness . . . . throughout the session the 3 grouping prevailed."

M, who has the highest limen for both the attention and memory spans, states that "it is comparatively easy to reproduce 5 digits without grouping" after he mentions that "in order to make certain in case a series should be long, one naturally groups." He further states that "some series are obtained by memorizing the first group of 4 digits." "It is easier to make associations on the basis of mathematical processes; *e.g.* the series 8-5-1-7 comes to me as one fifth of 85 is 17." "7 and 8 at the time of reproduction exist in consciousness as discrete units. Higher series are grouped." One should compare the observations and results of Sa and M in order to see clearly the different spans which we are presenting.

T had had no experience in this method, and his reports are meager. He "tried to group in 2s and 3s but lost out on the long series. On the short series I started with 4s, but when it came to recall the natural group was 3." He alternates between 3s and 4s throughout the experiment.

A writes, "I tried to group in 4s and I found myself referring to the series as dates, *i.e.* 1685, 1862, etc." Later in the experiment we find another effort to use dates in order to reproduce the series. In a series of 12 digits—3-7-1-5-2-8-1-6-4-9-5-7—he states that "37 is more than twice 15 and 28 is less than twice 16." Here we have a scheme which is similar to that suggested by M.

Ir starts out by grouping in 4s. "Grouping by 4s seemed to me to be much more efficient than by 3s." Toward the end of the experiment he writes: "I have now built up a number of association forms, into one of which I can press almost every series, thereby making it more probable that I can reproduce it. I attempt to memorize the first 4 digits in some way and keep the last ones by straight grouping." Later on he states that "I have become quite proficient in memorizing the first 4 digits of each series."

G reports: "I usually tried to group the first 4 numbers in groups of 2 and to use association for the others. Once I tried to interpret the digits as notes on the musical scale and tried to hum the tune of the digits: *i.e.* 1 = C, 2 = D, 3 = E, etc. The association worked for 3 digits, but I could not transpose fast enough to follow the presentation. At another time he had new associations. "The numbers of my apartment are 81, 82, 83 and 84. When one of these numbers appeared together, I associate them with the room." He varied the method from two groups of 2 each to three groups of 2. He observed a definite effort to group into 2s, 3s or 4s.

We find that this grouping is a definite mental process in this experiment. An inspection of the limens for 'attention span' for each of the *S*s in Table I will show the results obtained by the various methods described by the *S*s. M is the only one who reports that he finds it rather easy to get the digits up to 8. Although this may seem a rather large number, we must keep in mind the fact that his threshold for 'memory span' is a value of 2.6 digits above the next highest span of the *S*s. All of our observations indicate that M has the highest range of any of the *S*s selected in this investigation.

Humpstone states that "the subjects must have enough image-ability to get perceptions of the stimuli," and that "the images must be retained for the brief period of reproduction."[12] The elapse of time is just sufficient for writing the series, so that there is slight opportunity for a repetition of the series. Various types of imagery are reported by the *S*s. Sa states that it is chiefly auditovocomotor and that visual imagery was of little aid to him. H reports, "at times I tried to visualize the digits as written on the sheet of paper used as a shield, but this was finally discarded as impractical." M writes, "I tried using visual imagery but without success. All reproduction is based on auditory or vocomotor perseveration." T says, "Some series are given back by use of visual imagery for the first part of the series and auditory imagery for the latter part. The last two digits are obtained frequently by means of an auditory hangover." A first mentions imagery in the third hour; he says, "I seemed to visualize the series to a greater extent this time." At the end of the last session he writes, "I relied on my auditory imagery more than visual. This seems easier." Ir states that "generally when taking memory span tests I go very much by visual imagery, but this seemed lacking almost completely and I had to use auditory." Later on he mentions that his visual imagery was much clearer. G reports auditory imagery which is described above.

Former investigators have generally agreed that the memory span score that is obtained is not a true measure of the number of separate and discrete units which may be grasped and reproduced in a given moment of attention. In a previous study of the range for visual attention the writer concluded that "attention may be characterized by an immediate process in which all the sensory elements have an equal and high degree of clearness."[13] This was the result of simultaneous presentation of 'separate units.' The present investigation follows the method of successive presentation of separate units.[14] The limens for the 6 *Ss* in the range for visual 'attention' varied in that study from approximately 3 to 5 dots, and we find that the attention span in this study ranges from approximately 2.5 to 6 digits for 6 of our 7 *Ss*. We may expect that the highest *S* for memory span, will have a higher range than any of the other *Ss*, when working with visual stimuli, if the preliminary series can be considered as indicative of future progress.

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[12] *Op. cit.*, 15.

[13] Oberly, *op. cit.*, 353.

CONCLUSIONS

(1) Individual methods of grouping are used in memory span tests for the longer series.

(2) 'Attention span' is a term which satisfactorily describes the immediate reproduction of separate discrete units.

(3) A brief inspection indicates little difference in test results whether digits are presented in haphazard (Method A) or serial order (Method B).

(4) Visual imagery is of little aid in reproducing longer series of digits.

(5) 'Attention span,' for most individuals, is short enough to cause grouping of units after the number five has been passed.

(6) The 'attention span' limen ranges from 2.5 to 8, while the 'memory span' limen ranges from 6 to 13.5.

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[14] *Cf.* Gill and Dallenbach, *op. cit.*, 247.

THE CREATIVE YEARS

By Helen Nelson[1]

Goethe at seventy-nine once said of Napoleon, "But the real focus of his lustre belongs to his youth. That one of obscure origin should so have distinguished himself as to become in his seven-and-twentieth year the idol of a nation of thirty millions,—yes, yes, my good friend, one must be young to do great things! And Napoleon is not the only one!"[2]

"One must be young to do great things!" It was a sentence impulsively spoken, and not without its profound truth. Keats dying at twenty-six, Schubert before he was thirty-two, Alexander, with the world conquered, at thirty-three, Christ at thirty-three also, crucified yet with his incalculable influence upon the centuries, disclose the extraordinary power which may be released to youth. Yet we do not commonly think of early deaths otherwise than as frustrations. "Losing the spring out of the year" —Pericles' touching figure for the loss to the State of its youth in war—is after all losing its fruit. And Goethe as a matter of fact, in subsequent conversation and elsewhere, modified materially that one swift sentence quoted.

Yet the unmodified thought has currency, and Professor Woodworth of Columbia University in his *Psychology* has given it matter-of-fact expression. Speaking, in his chapter on *Imagination*, of the favorableness of youth to invention, "Seldom," Professor Woodworth says, "does a very old person get outside the limits of his previous habits. Few great inventions, artistic or practical, have emanated from really old persons, and comparatively few even from the middle-aged. The period from twenty years up to forty seems to be the most favorable to inventiveness."

That the years between twenty and forty are productive ones for genius there can be no doubt. But that the manifestations of genius are mainly confined to this score of years, genius itself disproves. A list of the great poets of Europe, for instance, might conceivably include the following nine great names: Homer, Aeschylus, Virgil, Dante, Chaucer, Shakespeare, Milton, Molière, Goethe. Though Longinus has an interesting passage in which he declares that "Homer grown old" wrote the *Odyssey*, "Homer great as ever, like the great setting sun, but without his fervent heat," we may best pass over Homer. The life of Aeschylus offers less for conjecture. Aeschylus winning his first victory at forty, and his thirteenth

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[1] The author of this paper, said of it, in two letters received some time after it had been accepted for publication, that "It grew out of reading of my own and was a steady two-months' labor of research and writing. At the same time the paper was by no means my own entirely, for my intelligence at every point was working with some other intelligence which seemed reënforcing mine. I mean that there was a constant transference of thought during the research for this paper and the writing of it. But whose intelligence it was directing mine I have no idea. The same condition has been true in the writing of one other paper of an entirely different nature, the editing of a book [actually published], as well as in very many experiences of ordinary life. This whole manner of living, which is fundamentally evil, for it means a secret power by person or persons unknown over body and mind and is made possible through hypnotism, animal magnetism, electricity or something very akin to these, first became distinct to my consciousness when by such person or persons I was made insane. I have reason to believe now, however, that these influences were active long before my recognition of them. Latterly it has been suggested to me by these same invisible, unwanted partners that my experience may be far from unique." The author has requested that the paper should appear under a fictitious name. The foregoing statement adds to the interest of the paper, which was accepted on its intrinsic merits. No editorial corrections have been necessary either in its admirable style or in the data which it presents. —M. F. W.

[2] *Conversations with Eckermann*, March 11, 1828.

and last and greatest victory at sixty-seven, but in his epitaph recalling only the proud serving of his youth in war, may cast just a passing light upon the sentence of Goethe's already quoted. Forty was a landmark for Virgil also, for then he began the *Aeneid*. Dying at fifty-one, with the poem as we have it now, he wished the manuscript burned, and wrote this into his will, because his work must lack the careful revision he had planned of three more years. Just when Dante began and finished his *Divine Comedy* is not known, but when he was about forty-nine he was working on the last cantos of its central portion, the *Purgatory*. Professor Skeat places the central period of the *Canterbury Tales* in Chaucer's late forties. Careful scholarship assigns to Shakespeare's forties *Othello*, *Lear*, *Macbeth*, *Anthony and Cleopatra*, *Coriolanus*, *Cymbeline*, *The Tempest*, *The Winter's Tale*. Milton began his *Paradise Lost* at fifty. Molière, dying at fifty-one, did not come into his genius until he was forty. "Nothing," suggests Mr. Saintsbury, "is more instructive than to compare *Les Précieuses Ridicules*, which is almost his first play, with *Les Femmes Savantes*, which is almost his last (the former produced at thirty-seven years, the latter at fifty). They are so closely connected in subject that the later play has sometimes been called an expanded recast of the earlier. But the improvement in treatment is immense. Amusing as *Les Précieuses Ridicules* is, it is not much more than farce of the very best sort. *Les Femmes Savantes* is comedy of the highest kind, the result of exact observation of life informed by intimate knowledge of character, and clothed with the most accomplished phrase."[3] As for Goethe, *Hermann und Dorothea* was written at forty-seven and forty-eight years, the first part of *Faust* was finished when he was fifty-seven, its second part when he was eighty-two. So that in the case of these great names at least, invention of the highest order, far from being in decay at forty, seems at very prime or just ready to begin.

Suppose we list some of the great tales of the world. Such a list would undoubtedly include *Don Quixote*, the first part of which was sent to press when the author was fifty-seven, the second part—"no book more signally contradicting the maxim that 'No second part was ever good' "[4]—when he was sixty-eight. If we include La Fontaine's *Fables*, it is the second collection, published when its author was fifty-seven, not the collection of a decade earlier, which called out Madame de Sévigné's "Divine!" The first part of the *Pilgrim's Progress* appeared when Bunyan was forty-eight, it being now generally supposed to have been written in the year immediately preceding; while the second part, taking up the tale of Christian's wife and their children, did not appear till Bunyan was fifty-six. *Robinson Crusoe* had a youthful author of sixty or almost sixty. *Gulliver's Travels*, said to have been well on its way toward completion when Swift was fifty-three, was not published until he was fifty-nine, though there seems to have been a period of private circulation; the first hint of *Gulliver* came to Swift when he was forty-seven. Sir Walter Scott did not enter upon the period of his novel-writing until he was forty-three, *Ivanhoe* appearing at forty-nine, while, as it happened, it was almost the latest of his novels, *The Fair Maid of Perth*, published when he was fifty-seven, which Goethe praised to Eckermann with such lively and intimate enthusiasm.[5] *Les Miserables* met an eager world when Victor Hugo was sixty years old; when *Quatre-Vingt-Treize*, so highly praised by Swinburne, was published its author was two-and-seventy.

Or suppose we were asked to name the great orators of the world. Probably the names of Demosthenes, Cicero, Burke would be first on our lips. But two of Demosthenes' greatest speeches, one of them the third

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[3] G. Saintsbury, Molière, *Chambers 'Encyc.*, 1896.

[4] J. F. Kelly, Cervantes, *Encyc. Brit.*, 11th ed.

[5] *Conversations with Eckermann*, Oct. 3, 1828.

Philippic, were delivered when he was forty-two or three years old; while eleven years later, his whole public life under attack, he gained "an overwhelming victory for himself and for the honor of Athens" (they are Professor Jebb's words) "in the most finished, the most splendid and the most pathetic work of ancient eloquence—the immortal oration *On the Crown*."[6] As for Cicero, he was forty-three in the year of his four famous speeches against Catiline, and over sixty when he delivered against Antony those *Philippics* which cost him his own life. While Burke made, at forty-six, his great plea for the conciliation of America, and opened the famous trial of Warren Hastings with a masterpiece of English eloquence at fifty-nine. Nor can we pass over the Gettysburg address, spoken when Abraham Lincoln was fifty-four.

Of the great composers of music, many met early death, Schubert before he was thirty-two, Mozart at thirty-five, Mendelssohn at thirty-eight, Chopin at forty, Schumann after several years of broken health at forty-six. But the *Messiah*, by which we are wont to remember Händel, was written when he was fifty-six; and we have forgotten his earlier forty-odd operas. Bach's *Passion according to St. Matthew*, to name but a single celebrated work, was written at forty-four. At sixty-three and onward Haydn was writing "the last and finest" of his chamber music, and at sixty-seven the *Creation*, surviving to-day alone of his oratorios. "At the beginning of 1827," writes Mr. Tovey (in 1827, the year of his death, Beethoven was fifty-seven years old), "Beethoven had projects for a tenth symphony, music to Goethe's *Faust*, and, under the stimulus of his newly acquired collection of Händel's works, any amount of choral music, compared to which all his previous compositions would have seemed but a prelude."[7] The four great symphonies of Brahms were of the period lying between forty-four and fifty-three years. Wagner was forty-six when he finished *Tristan und Isolde; Das Rheingold* and *Die Walküre* were produced when he was fifty-six and seven; *Siegfried* and *Götterdämmerung* followed when he was sixty-three, and *Parsifal* when he was sixty-nine years of age. As for Verdi, he was fifty-eight years old when *Aïda* was produced; but *Otello* produced at seventy-four and *Falstaff* at eighty years of age are, in the opinion of his biographer, Streatfield, writing in the *Encyclopedia Britannica*, "an immense advance on anything Verdi had previously written," while his settings, at eighty-five, of the *Ave Maria*, *Laudi alla Virgine*, the *Stabat Mater* and the *Te Deum*, to recur again to Streatfield, "in imaginative power and musical beauty yield to none of Verdi's works."

Of the great artists, Giorgione died at thirty-four, Raphael at thirty-seven, Correggio at forty, Van Dyck at forty-two; and Holbein at about forty-six, leaving, as work of his forties, a number of portraits "very perffight," "verye lyvelye." But many of the great artists have lived to sixty years or over, productive long after the brief score of years allowed them by Professor Woodworth is past. Giotto, solemnly appointed three years before his death master of the works of the cathedral of Santa Maria del Fiore and official architect of the city walls of Florence and of the towns within her territory, was able to plan two architectural works of importance and superintend their beginning before his death at seventy (or possibly it was sixty-one) years. Leonardo da Vinci's *Last Supper*, "for all Christendom the typical representation of the scene,"[8] was the work of the four years from forty-two to forty-six; the famous *Mona Lisa* the work of another four ending at fifty-four years. It was in his fifties also that his great cartoon of *The Battle of the Standard* was worked out and exhibited—the goad and inspiration of contemporary artists, but, aside from studies, surviving today only in a tiny partial sketch of Raphael's preserved at Oxford, and in some copies of cherished copies, one of these by Rubens. It was from fifty-nine to sixty-six that Michelangelo painted *The Last Judgment*, "probably the most famous single picture in the world,"[9] and from sixty-seven until his death at eighty-nine that he was chief architect of St. Peter's Church at Rome, with other vast architectural schemes also claiming his attention in the later years. The date of Titian's birth has been questioned, and the very last of his work is said to show failing power; but at least he had not lost the cunning of hand and eye in the year 1562, when at seventy-two or three, as some say, or at eighty-five as he himself professed, he painted the fine portrait of himself cited by William Michael Rossetti as among the highest examples of his work in this kind. Tintoretto began at fifty-nine the memorable series of paintings for the Scuola di San Rocco concluded seventeen years later only with his death; painted at sixty "four extraordinary masterpieces" in the Sala dell' Anticollegio; and at seventy began the vast *Paradise* (seventy-four feet by thirty) which, before the commission was awarded he prayed God he might paint, if so he might himself come at last into Paradise. It was the crowning work of his life. Rubens dying at sixty-three, Velasquez at sixty-one, passed with undimmed powers. As for Frans Hals, some of his most striking work was produced in the long bitter poverty of those later years when, it is thought, he could not afford the more expensive warmer pigments but must perforce use black and white; and in the two canvases of *The Regents and Regentesses of the Oudemannenhuis* (at Haarlem), he created "masterpieces of color" in all but monochrome at over eighty. Rembrandt at fifty painted some of his greatest pictures, including the famous *Portrait of Jan Six*, and at about fifty-seven produced, in the *Jewish Bride* of the Ryks Museum and the *Family Group* of Brunswick, "the last and perhaps the most brilliant works of his life."[10] "The most splendid period of Murillo's career" was that of his commission for the Almhouse of St. Jorge, for which, at fifty-seven, he received for his eight greatest pictures there about £800. Of English artists, Gainsborough did not come into his style until he was over forty; Sir Joshua Reynolds finished at sixty-one *Mrs. Siddons as the Tragic Muse*, "undoubtedly his greatest portrait;"[11] William-Blake's *Illustrations to the Book of Job*, holding first place among his artistic work, were published when he was sixty-nine. As for Turner, his "central and best period," according to the Scottish painter, Sir George Reid,[12] began at fifty-four when he exhibited *Ulysses Deriding Polyphemus; The Fighting Téméraire* and *The Slave Ship* were exhibited at sixty-four and five; while Ruskin in the first volume of *Modern Painters* cites a number of pictures falling between the years sixty-five and sixty-nine as Turner's most surpassingly perfect work. It may be of interest also to refer in passing to the fourth volume of *Modern Painters* and to the plates there reproduced of Turner's earliest *Nottingham*, published at twenty when he was "still working in a very childish way," and his latest *Nottingham* published when he was fifty-eight. As for Corot, and Corot must close a list already overlong, not until about forty-four did he develop "that style which marks the works of his maturity," and his medals at the Salon were won at thirty-seven, at fifty-two, at fifty-nine and at seventy-one years.

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[6] G. C. Jebb, Demosthenes, *Encyc. Brit.*, 11th ed.

[7] D. F. Tovey, Beethoven, *Encyc. Brit.*, 11th ed.

[8] Colvin, Leonardo de Vinci, *Encyc. Brit.*, 11th ed.

In the field of theoretic and practical science also, the later years, with the earlier, play their part. The groundwork of the Copernican theory, laid by its author between the years thirty-three and thirty-nine, tested with means scanty enough through the following years, was not finally embodied in the *De Revolutionibus Orbium Coelestium* until Copernicus was fifty-seven years old. Nor was the "Little Commentary" or abstract of his thesis written until that year. The latter part of Galileo's life was full of invention. He was forty-five when some rumor of a telescope newly invented by an obscure Dutch optician set him pondering one night on the principles of refraction. Six months later he had an instrument, made with his own hands—he was able to make hundreds of them subsequently for the requests poured in from every part of Europe—by means of which, and his own genius at discernment and expositión, were laid the foundations of modern descriptive astronomy. The first of Galileo's telescopic discoveries took form in the *Sidereus Nuncius* when he was forty-six; his last, that of the moon's diurnal and monthly librations, was made only a few months before he became hopelessly blind, at seventy-three. Even then, remembering the oscillations of a bronze lamp which at nineteen he had seen in the cathedral of Pisa, he thought out the application of the pendulum to clockwork, and shortly before his death (at seventy-eight) discovered the mathematical demonstration of the "not very obvious principle" which earlier it had been necessary for him to take for granted, that the velocities acquired in descending, from the same height, planes of every inclination are equal. While at seventy-two perhaps his most valuable and lasting treatise was completed, the result of his early experiments and mature deliberations on the principles of mechanics—the dialogue of *The New Sciences.* Kepler at thirty-eight gave to the world his "First and Second Laws," at forty-eight his "Third Law," at fifty-six the *Rudolphine Tables,* which for a century retained their authority and in principle have never been superseded; while his *Mysterium* published at twenty-five proved an error. It is supposed that Sir Isaac Newton was in his twenty-fourth year when, according to Voltaire who had the famous story from Newton's niece, the apple fell, directing the young mathematician's attention to the subject of gravity. But his calculations resulting only in serious discrepancies, he "laid aside at that time any further thoughts of this matter." Apparently it was not until he was forty that he recurred to the subject, a more exact figure for the diameter of the earth having in the meantime been formulated, and not until he was forty-two that he wrote *De Motu,* which contained in twenty-four octavo pages the germ of the *Principia.* The *Principia* itself was written in the two years, memorable for science, when Newton was forty-three and four. Darwin, beginning at twenty-eight the patient collecting of fact and testing of hypothesis, did not put his notes in order and publish the *Origin of the Species* until he was forty-nine and fifty years old; and of the series of studies which followed, *The Formation of Vegetable Mould through the Action of Worms* (the completion of a much earlier paper) appeared, at seventy-two, in the year before his death. Jenner, after more than a score of years of study and waiting, made at forty-seven his first inoculation for smallpox. Pasteur was fifty-eight when he set himself to study hydrophobia and sixty-three when he began for this disease the practice of inoculation. Virchow, "father of modern pathology," in the last thirty years of his life (he died in 1902, eighty-one years old) was conspicuously instrumental in effecting for Berlin that great reform in sanitation which left the city, in the beginning one of the unhealthiest in the world, one of the healthiest.

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[9] *Idem.*, Michelangelo, *op. cit.*

[10] J. F. White and P. G. Konody, Rembrandt, *Encyc. Brit.*, 11th ed.

[11] Artide on Reynolds, *Chambers' Encyc.*

[12] Article on Turner, *Encyc. Brit.*, 11th ed.

Among the very many investigators who have contributed to our knowledge of electricity or to its practical application, the following names are among those, perhaps, best known: Gilbert, who in 1600, at sixty, in his *De magnete, magneticisque corporibus, et de magno magnete tellure,* laid "the foundations of modern electric and magnetic science;" Benjamin Franklin, whose memorable experiments filled an interval of his forties and fifties between the business from which he had retired and the important diplomatic services of his later life; Cavendish, whose unpublished electrical researches in 1771 to 1781, when he was forty to fifty years old, were thought of sufficient value, a century later, to be edited by James Clerk Maxwell and published; Volta, whose Voltaic pile, "a fresh implement of great power" to the chemist, was devised when he was about fifty-four; Oersted, whose discovery at forty-two of the effects of a current of electricity on the magnetic needle was "epoch-making in the degree to which it stimulated other research;" Ampère, whose first notable memoir, the direct outcome of Oersted's discovery, was communicated to the Academy at forty-five; Faraday, whose brilliant and unprecedented work, the direct stimulation of untold scientific and technical activity, filled a score of years between forty and sixty; Morse, who at forty-six exhibited his telegraph to a few friends in the University of the City of New York, and at fifty-one was making experiments with telegraphy by a submarine cable; Lord Kelvin, among whose multitudinous achievements were the siphon recorder, designed at forty-three for receiving messages over the Atlantic cable, and (as d'Arsonval's galvanometer) now in common use in electrical laboratories, and a mariner's compass adding greatly to the safety of navigation and generally adopted by the navies of the world, patented in 1878 when he was fifty-two; Röntgen, who at fifty discovered the X-rays, now so indispensable to surgery and, it seems, to art; Edison, Marconi, whose wizardry somehow we are not accustomed to regard as entirely of a past tense.

A handful of inventors in various fields must close a protracted tale. John Harrison at sixty-eight, after a life-time of patient labor, claimed and finally received a reward of £20,000 offered by the British Government forty-eight years previously for a ship's chronometer which would determine longitude with an error not greater than thirty minutes. Rewards of £10,000 and £15,000 had at the same time been offered for chronometers determining the longitude within sixty and forty minutes. The perfecting of Harrison's delicate instrument, outstripping all hope for it determined longitude within eighteen minutes, was the work of twenty-six years after his first chronometer was submitted and found distinctly promising, at forty-two. Although James Watt's first patent for his steam engine was taken out when he was thirty-three, nearly four years as a matter of fact after the inventions covered by it had been made, other important patents followed; the fourth of which, covering the well-known "parallel motion," taken out when he was forty-eight, Watt considered "one of the most ingenious simple pieces of mechanism" he had contrived. Nor was this ingenuity lacking in the very last work of his life—the invention of two machines for reproducing sculpture, one for smaller likenesses, one for likenesses of the same size—with which, not many months before his death, he was making copies of busts and presenting them to his friends as the work "of a young artist just entering on his eighty-third year." George Stephenson's first locomotive, "My Lord," the work of his thirties, was not a particularly efficient machine; his engine used on the first passenger and freight railway to be opened (in 1825 when he was forty-four years old) had a speed of nearly sixteen miles an hour; his "Rocket," which, when he was forty-eight years old, bore off the prize of £500 offered by the Liverpool and Manchester Railway then nearing completion, had a speed of thirty miles an hour. "Now," exclaimed one of the directors, "has George Stephenson at last delivered himself." Photography began in 1814 with Niepce, who at forty-nine produced the first permanent sun-pictures; and with his subsequent associate, Daguerre, who a quarter of a century later, at fifty, announced the discovery of the process, the first of the sort of any practical value, which is associated with his name. Roebling at forty-five began the erection at Niagara Falls of the long-span wire suspension bridge which eminent engineers of the time regarded as foredoomed to failure, completed at sixty-one the still more remarkable suspension bridge at Cincinnati over the Ohio, and died at sixty-three from accidental injury before finishing his great Brooklyn Bridge over the East River. Nobel patented dynamite in 1867 at thirty-four, blasting gelatine, a more powerful explosive, at forty-three, and produced ballistite, one of the earliest nitroglycerin smokeless powders, at fifty-six.

The inventors are only a handful, chosen from out the baffling silences of our encyclopedists. But they suggest that inventors are not of different race from genius in other fields (nor from humanity for that matter, to whom continued effort means naturally increasing excellence in accomplishment). They suggest, too, that we scan a little more closely the dates commonly associated with inventions in this field. The date of the final perfecting of John Harrison's chronometer, for instance, represents a date preceded by creative activity; yet a date of a type rarely receiving emphasis in accounts of mechanical invention. We crown invention with its earliest date, forgetting that improvement may follow improvement through a lifetime, perhaps, as the relatively crude first contrivance grows into the perfected machine. Such a method of dating is no index to the creative years. Nor is it strictly comparable with the method of dating in theoretic science and the arts, where datings mark normally the finished masterpiece, after the last stroke of the laboring file. Certainly this is a matter to be searched into, in any further study of the creative years. In the meantime we may suggest it as possibly accounting for the barrier of forty which we found raised.

But surely we may suggest that this barrier should be taken down! And to the writer at least, it seems an obstruction best not raised at any subsequent milepost. For here differences in the ageing of individuals must play their subtle part, and health and the possession of sight and hearing contribute, at times a little mockingly; for though a blind Sir Joshua Reynolds cannot paint, yet a deaf Beethoven can compose music, and delicate health, as in the case of Darwin, need not interfere with late creative activity. Then, too, personal relationships, with their stimulus, have also their fortuity, Michelangelo's sonnets, for instance, for the most part falling within a period of five years just before and after he was seventy, when his lonely life was sweetened with the friendship of the accomplished Vittoria Colonna. Debt, as for Sir Walter Scott, or poverty, as for Frans Hals, may conceivably constitute another determinant. The personal ideal must be allowed its definite shaping power also; as with Racine, retiring and silent for over a dozen years before producing when about fifty, at the solicitation of Madame de Maintenon for her girls to act at Saint-Cyr, *Esther* and *Athalie*, two of his finest plays. Destiny, too, has its force. It caught Milton at the Civil War, only his early poems written, into seventeen long years of polemic and diplomatic English and Latin prose; it caught Benjamin Franklin into diplomacy, and George Stephenson into the problems of railway administration. Again, at times one may suspect the possibility of genius solving its problem before exhausting its powers; and observe the new lease of life accorded those who, like Michelangelo, can turn from sculpture and painting to poetry and architecture, or like William Morris, from poetry to the beautifying of living through fine artisanship, as in the latest achievement of his life, the Kelmscott *Chaucer*. At least it is not age alone which halts genius; or turns it from invention in its narrower sense into other, and sometimes wider, thoroughfares.

Nor for Professor Woodworth is age the fundamental factor, but age because bound by habit. "Seldom," says Professor Woodworth in the passage already quoted, "does a very old person get outside the limits of his previous habits." But is it, after all, habit which is the deterrent of genius? Genius, so wary, so impatient of the habitual, so instinctively seizing upon the disparate elements of new and right combinations avoiding habit, can genius be said to become enchained by its habits of youth? Its habits of youth are habits toward freedom. Its whole approach to life is individual, unique; and its successive approaches to life retain this vital quality. Even the style of genius, its "habitual" expression, grows with its growth, painters and musicians having their successive "manners," Shakespeare writing at different periods a distinguishable music into his lines. The very definition of genius seems to involve something of the protean kind. When genius grows sluggish, as it did early in Wordsworth's life,[13] or breaks down as it did late in Turner's and Titian's, may we not suspect the presence of some subtle disintegrative force among the bodily forces which prevents the operation of habit, or which is even actually dispersing it, rather than the fettering quality of a distinguished creative past? Indeed, it was just as Turner's sight began to fail at seventy, with the health of body and mind, that his work failed also. As for Titian, who continued to paint up to his death at ninety-nine (or was it at eighty-six or seven?), it scarcely seems unnatural to attribute Titian's failing power to some breaking of the delicate bodily habits which for so long had served him. Even with Wordsworth it seems more reasonable to suppose some volatile element in the creative habits of his best decade, lacking which his later work fell to ground, than that he was bound hard and fast by these best ten years. For he continued to compose verse: why did it not continue to be poetry? The later life of Emerson affords still another example. A book had been promised, but it was found that Emerson could not put it together from his own earlier notes. Scarcely could one say that, because he could read those notes but could not fuse them, he was bound by his previous habits of reading. Emerson had behind him the habits of a lifetime of creative composition; it was because these habits were broken with broken health that he was helpless. No, *genius is stopped, in youth or age, when the habits of its personality break down.* While they function, the past but gives enablement, the very munitions of war, for present inspiration. This is only a natural law of growth.

On a December evening after Goethe was eighty years old, he read a scene from the second part of *Faust* to Eckermann, and after reading said, "I have had it in my mind for fifty years. But perhaps it is well that I have not written it down till now, when my knowledge of the world is so much clearer. For I am as one who had in youth many small pieces of copper and silver money, who constantly exchanging them for better, beholds now the property of his youth in pieces of pure gold."[14] Emerson turns us an obverse side of the picture of which Goethe too had knowledge. "We must prize our own youth," he tells us in that volume which he himself was unable to marshal. "Later, we want heat to execute our plans. The good-will, the knowledge, the whole armory of means are all present, but a certain heat that once used not to fail, refuses its office; and all is vain until this capricious fuel is supplied."[15] The pure gold of maturity, the heat of youth—these together make the living masterpiece. Happily, with most men of genius, youth does not die with the years of youth. Continually we have found genius equalling or surpassing its youthful work; while instances are not rare of youth's proving only a latent or apprenticeship period. "Other people are young but once," Goethe explained to Eckermann, "but if the soul is of a powerful kind as is the case with all men of natural genius, then its animating penetration of the body tends to confer the privilege of a perpetual youth. So that we constantly perceive in men of superior endowments, even in their old age, fresh periods of singular productiveness; they seem to grow young again for a time, feel a new youth."[16] We can begin to understand therefore, why masterpieces of the first rank are so often produced in the middle and later years. Youth holds over; and maturity with its wealth of winnowed and delicately winnowing habits has been won. To natural endowment is added the gathered wisdom of an intensely critical experience, so that the favorableness of youth to great invention seems more than matched by the favorableness of age. Certainly forty lays no staying, withering hand upon the creative powers. Certainly forty and the years after are years most precious for earth's inventive, as secondarily for her uninventive, children.

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[13] "Wordsworth composed verses during a space of some sixty years; and it is no exaggeration to say that within one single decade of those years, between 1798 and 1808, almost all his really first-rate work was produced." Matthew Arnold: *Wordsworth*, in his *Essays in Criticism*, 2nd series.

[14] *Conversations with Eckermann*, Dec. 6, 1829.

[15] Emerson, Inspiration, in *Letters and Social Aims*.

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[16] *Conversations with Eckermann*, March 11, 1828. The quotation is somewhat condensed.

APPARATUS

THE VICTOR STROBOSCOPIC DISK FOR VISUAL EXPERIMENTS

The psychologic and world-classic trade-mark "His Master's Voice" shows our canine friend not only listening but looking. The Victor Talking Machine Company in their 60-*Cycle Stroboscope Disk* have provided us with an important item not to listen to but to look at. This paper disk, 5 in. in diam., is for testing the speed of phonograph turntables. On its face 92 radiating black spokes are printed. The black spokes are 2° wide and 1 7/8 in. long. The white interspaces are approximately 1° 45′ wide and there is a clear white space in the middle of the disk 1 1/4 in. in diam. The edges of the spokes are sharp and straight and the entire design seems to be very accurately made. The paper disk is placed on top of a regular record the outer portion of which is then played. The illumination must be from an electric lamp on 60-cycle current and then the speed regulation of the turntable is varied until the dark radial lines appear and are stationary, when the rate will be 78 revolutions per minute. (A 60-cycle current gives 120 light pulses per second or 7200 per minute; 78 revolutions of 92 spokes gives 7176 passing a given point per minute.) The bars appear of about the same intensity as if one looks at the stationary disk through two thicknesses of 16-pound white English bond paper. The method is of course simple and quite exact. Whereas, the ordinary stroboscopic disk[1] provides for making determinations over a range of several frequencies, the present disk provides for just the one rate. Several years ago Lorenz[2] pointed out the applicability of such disks for measuring alternating-current frequency. Quoting him further he said, "stroboscopic methods of measuring frequency and speed of rotation possess certain advantages, and doubtless the reason they have not been more generally used has lain in the lack of a convenient and reliable source of light. Since that time the alternating 60-cycle current for household uses has become so relatively steady that the stroboscopic method can now be used in every household and the Victor Company is to be congratulated for having worked out this simple scheme.

This 5-in. disk considered as a *stationary visual stimulus* is the most *live* object of the kind I have ever seen. If one fixates the center as steadily as he can, he observes phantom objects rippling and revolving in a most extraordinary manner. It resists being converted into a first rate negative after-image except from very brief fixation. There are faint transitory concentric color bands seen near the central portion. The thin black wedges near the center show strikingly the effect known as Bergmann's Experiment.[3] The wedge-shaped spokes are very black and straight at the periphery of the disk, but as they approach the center they seem to become prominently beaded, at times appearing twisted like a thin tapering screw.

If the surface of the disk is viewed almost edgewise, color mixing by irradiation can be observed on both sides while straight across the middle the black spokes show with exceeding prominence. According to the fixation position these prominent spokes show in their true wedge-shaped character as straight lines or as curved lines.

Viewed from a distance of about 15 ft., the disk serves admirably as a test chart for measuring astigmatism and probably does it better than other charts devised for the purpose by reason of the fact that the sector units are so small.

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[1] E. W. Scripture, Elementary course in psychological measurements, *Studies from the Yale Psychol. Lab.*, 4, 1896, 136, Fig. 19; also C. E. Seashore, A voice torroscope, *Univ. of Iowa Studies in Psychol.*, 3, 1902, 18-28.

[2] C. F. Lorenz, A stroboscopic frequency-meter. *Electrical Review and Western Electrician*, May 17, 1913.

[3] Helmholtz, *Physiological Optics*, trans. 1924, II, 34-35; and Edmund C. Sanford, *A Course in Experimental Psychology*, 1895, pt. i, 107 (experiment 118).

Used as a *moving visual stimulus*, the disk exhibits some remarkable qualities. A very slow rate of rotation provides perfect color mixing (a middle gray) due to the fact that the sectors are only 2° in width. Moved fairly rapidly up and down over a distance of 2 in. while the center is being fixated, brings out a striking illusion of motion. When the disk moves upward, the sectors directly at the bottom seem to expand and grow farther apart. While those at the top execute the opposite motion of squeezing in and becoming nearer together. When the disk is moved downward sharply, the opposite of this accordion-like effect takes place; likewise the effect may be observed if the disk is moved along any other linear axis.

If the disk is moved as on an eccentric without being actually revolved on it's own center, grayish phantoms appear near the central portion and exhibit prominent polar projections which appear to revolve; sometimes in one direction, sometimes in another, almost as a reversible windmill illusion.[4] The disk may well serve as an alternate to the black and white spiral disk[5] for showing colors in connection with flicker, Purkinje's 'shadow figure,' and the apparent arrest of movement due to eye-movements. The Victor disk gives all of these phenomena more prominently. Probably this is due to the tapering shape of each unit and to the radiating character of the whole arrangement. The disk, inexpensive and readily available as it is, should become one of the most useful objects for visual demonstration. The phenomena are so prominent and so bizarre that people as a rule object to looking at it for any but very brief intervals.

Stanford University WALTER MILES

A CHILD'S STYLUS MAZE

The ordinary slot maze[6] is ill-adapted to certain work with children because the *S*s can 'peek' at the maze or are disturbed if blindfolded. Carr[7] designs a metal maze with invisible stops which avoids these difficulties. The writer has found, however, that Carr's maze is more easily and cheaply constructed out of vulcanized fiber-board, and wishes therefore to make the recommendation. In addition, the use of fiber makes it a simple matter to employ a brass stylus to give electric contacts with the stops, so as to work a signal at the goal, or even to record all errors except reversals in the correct path.

The maze is constructed by inserting smaller pieces of fiber between the base and the blocks which form the surface of the maze, and attaching each block to the base by two bolts.[8] This construction leaves inverted T-shaped grooves which may be obstructed at appropriate points by pairs of bolts screwed through the base into the lateral (hidden) portions of the groove.[9]

Yale University JOHN M. MCGINNIS

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[4] Since preparing this note the article by Carmichael and Schlosberg has appeared (this JOURNAL, 40, 1928, 128.)

[5] Helmholtz, *ibid.*, 256 (Fig. 53).

[6] The 'Foster' maze is an example. cf. W. S. Foster, *Experiments in Psychology*, 1923, 17-18.

[7] A. H. Carr, The influence of visual guidance in maze learning, *J. Exper. Psychol.*, 4, 1921, 398 ff. Another form of this maze was demonstrated by Miss Frances Bodkin in connection with a report given at the December meeting of the American Psychological Association at Philadelphia in 1927.

[8] The author suggests the use of 3/16 in. stove bolts with a No. 24 twist drill, which makes it unnecessary to tap the holes.

[9] From the Psychology Laboratory, University of Maine.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Meaning of Psychology. By C. K. OGDEN. New York and London, Harper Brothers, 1926. Pp. 326.

Dr. Ogden's book applies a consistent principle to the field of psychology, especially to problems now under discussion. The style is informal without loss of technical exactness. An admirable clarity, restraint and common sense marks the whole work. Critical comment is frequent but always subordinated to the main issue. The author shows in high degree a power of holding to the essentials of any matter with which he deals.

The nature of his objective leads at the outset to a discussion of the mind-body relation. Seven theories are stated and the defects of each briefly noted. The criticism of these hypotheses seems grounded, in some instances, upon their metaphysical implications rather than any psychological insufficiency. Holders of the Double Aspect Theory, for example did not substitute a speculative thing-in-itself for the psychological objective, though the Double Language view which the author adopts phrases the essential situation better. It is one and the same life which psychology and neurology study.

The author has no quarrel with Behaviorism for making the most of its principle. Its insistence upon objective methods and controlled observation has been altogether healthy. It has compelled Psychology to set its house in order. The study of response patterns and their conditioning has been undertaken by opponents as well as by representatives. Its endeavor to dispense with 'mind' and 'experience' is laudable in view of the unverifiable speculations of the past. This exclusion, "a bias as extraordinary as that which it was designed to avoid," is the fundamental defect of a strict Behaviorism. Psychology depends upon two methods, neurological and introspective, each of which supplements the other. Behaviorism will soon forget its theatrical gesture in disowning experience, just as it will correct its own over-emphasis of movement and recognize the significance of the total psychological fact which 'reaction' includes. What the Behaviorist overlooks is that it is the postulation of satisfaction and dissatisfaction, impulses, aims and valuations which defines the limits of his field and gives him his *point d'appui* as psychologist.

The program of the book is a synthesis of neural conceptions which shall serve as a schematism, objectively for the reaction field and subjectively for attitudes in all their complications. Beginning with the simplest path taken by a nervous discharge its first problem is found in the inhibitions which result from a conflict of impulses. The treatment is based on Lapicque's theory of chronaxies, which starts from a comparison of the times required to produce a response in different nerves and muscles, and of the influence upon these time relations of variations in stimulus-intensity, fatigue, drugs, modifications of glandular secretion, etc. A muscle and its motor nerve are isochronous, but each muscle and each nerve has its own chronaxy. When conduction takes place within any plexus, the chronaxies involved are either identical or have simple numerical relations. If the chronaxy of any element is accelerated or retarded without a corresponding modification in the rest of the field it is thrown out of tune with the system and conduction does not occur. This conception he applies to the selective and organizing functions of the nervous system.

Of the innumerable impulses coming in from the sense organs few reach the final common path of motor neurones, but very many play a part by barring out or modifying concurrent impulses. Lower level receptive centers react to specific patterns as a lock to its key. Associated with these, on the next higher level, are sensory coördinating centers so related that the patterns getting through one center will depend on what patterns have been picked out by others. The key-patterns obviously can neither be simple nor unvarying, and the schematism must from the outset be conceived as very complex. The interdependence of the coördinating centers points to a still higher control mechanism which is found in the associative areas of the brain. The set of the whole system thus determines the history of any impulse coming in, which may illustrate the influence of expectation on perception or that of a general conception on the particular instances which are brought under it.

Pavlov's work on conditioned reflexes affords occasion for the fullest development of this concept. The author finds McDougall's drainage theory and that of lowered resistance alike inadequate. Differences in potential and reduction at the synapses do not account for certain inhibition phenomena which these conditioned reflexes present. What has not been sufficiently considered is the end-state of the whole process. The impulses do not come to an indifferent brain but one tuned to them by hunger, and the total conditioning of the reflex includes its satisfaction. It is the nature of this final state which maintains or disturbs the synchronizing of the neurone chronaxies in which the establishment of the conditioned reflex consists.

In the remainder of this part of the work the conception of need and end-state is applied to such problems as recognition, interest and foresight. The term 'interest' marks our characteristic selection of patterns—which again records the plan of persistent accords in our nerve centers—an activity which depends finally upon the satisfaction which their picking out affords us as integral individuals, their consonance with conscious or unconscious purposes.

Now purposiveness is a psychological characteristic due to retention. What we call foresight is the influence upon any action of the effects of what happened on previous occasions under similar situations. Its essential features appear in the conditioned reflex and involve no non-physical concept, such as a psychic urge or libido.

The general scheme of all these activities is to be found in a few primary needs and the great array of derivative needs associated with them in the case of man. An interest may thus be defined as an activity set going and maintained by a need. The process demands, of course, a nervous organization capable of translating the need into activity, including both excitability in the centers and delicacy and plasticity in their tuning.

This schematism is clearly applicable to the phenomena of selection and exclusion in the field of perception, and serves to re-state Koffka's contribution. The *Gestalt* is an end-state system and new *Gestalt*-patterns will appear wherever fresh end-states or novel systems arise in the satisfaction of needs. The application of the principle to learning is obvious and leads the author through a review of Köhler's experiments with apes to mental growth in man.

Development begins with sensory experience but its 'perception bridges' between need and satisfaction are soon supplemented by images which arise as substitutes for the missing situations and constitute the matrix out of which memory develops. The conflict between fantasy and reality, together with the processes concerned in their integration, is finely drawn.

From this the author turns to look inward upon experience and outward upon the world. The mind is a system of mobile equilibria among tensions, stresses and impulses grounded in needs and moving toward their satisfaction. This character determines the range of awareness as well as the focus of attention, and affective quality is affiliated with the flow or hindrance of movement toward these end-states. The nature of our outward look has already been indicated, for example in the discussion of interests, and the author turns to the psychology of scientific fictions and the significance of the human equation in all accounts of the external world.

The flow and conditioning of thought reflects in detail this pragmatic principle. It is a process of experimentation with the mental representatives of things, including emotions and moods as well as images. The spread and direction of association are determined by interest and attitude. Remembering and forgetting have a similar dependence, as Freud pointed out. The hierarchy of concepts corresponds to the system of coordination levels for handling impulses. Judgments and beliefs are idea-structures formed in the service of our interests.

Affectively toned experience and emotional attitudes as well as trains of ideas are to be construed as preparations for action and probably constitute the matrix out of which the latter have arisen. The emotional disturbance, however, does not mark situations which allow of a prompt and free response but those which involve a conflict between rival tendencies. This interplay, with its subtle and far-reaching effects, has been the chief object of psychoanalysis in its study of repressions, complexes and defence systems.

This brings the author to his final problem—how the mind goes wrong. In general it may be described as due to the mutual obstruction of interests, resulting in blocking, displacement and loss of integrity in the system as a whole. Many needs which cannot be expressed because activities grounded in other needs are in possession of the appropriate paths find satisfaction either in rational compromise and development or in fantasy and regressive organization. It is the nature of the compromise which distinguishes the normal action from the abnormal. Artificial fixations appear, mechanisms arise which are out of adjustment with more general requirements, the movement toward satisfying end-states is thwarted, the activity of the higher coördination centers is weakened and regression to an infantile condition takes place with widespread disorder as its result.

The book is a valuable contribution to current discussion and the reader will find few topics upon which some interesting light is not thrown.

New York University ROBERT MACDOUGALL

The Journal of General Psychology. Edited by E. B. TITCHENER and CARL MURCHISON. Worcester, Mass., Clark Univ. Press. Quarterly. Vol. I, no. 1, Jan., 1928. Pp. 192.

The appearance of a new psychological journal is always an event of importance to psychologists. *The Journal of General Psychology* is to be a quarterly devoted to "experimental, theoretical, clinical, and historical psychology." This first number includes eleven articles, of which six are experimental, four theoretical, and one on the border-line between the two. There are in it no clinical or historical articles, and the Board is not chosen so as to emphasize the clinical aspect.

The first number is extremely interesting because of the high average quality, the distinction of many of the authors, and the variety of the subject-matter. Six of the Editorial Board—Bartlett, Dunlap, Hollingworth, Miles, Spearman, and Wheeler—contribute articles. Of these articles the only one that can be called 'experimental' is Bartlett's, a fact that verifies a growing suspicion that the correlation between publication of experimental work and distinction of author is negative. There are three articles from Cornell and its periphery, and two from Stanford, but the other six are distributed in a most catholic manner among Clark, London, Cambridge, Hopkins, Columbia, and Kansas.

The journal was 'co-founded' and this number 'co-edited' by Titchener and Murchison. There is no doubt of Titchener's great interest in the project and the general form of the journal bears the stamp of his views. It is most regretable that at the moment of its birth the new undertaking should be handicapped by his death.

The editors are supported by a Board of twenty-four psychologists: sixteen from the United States, three from Great Britain, and one each from Canada, Australia, Germany, Austria, and Italy. Presumably the journal will be primarily American and secondarily Anglo-American.

There is a department of Notes, under Titchener's editorship, and a department of Book Reviews, under Nafe's editorship. In this number the four notes and the seven reviews all come from Cornell or Clark, except one note from Wittenberg. Two of the notes are by Titchener, his last publications.

The journal does the world the service of printing a photographic reproduction of the Brauner portrait of Titchener, an excellent reproduction. It promises later an article on Titchener by "a friend and fellow-psychologist."

One hesitates to make any criticism at all of a new editorial venture, embarrassed before it went to press by the death of its senior editor; but friendly suggestions are always in order. In general the proof-reading is as excellent as the contents. The present reviewer is bothered by the absence of authors' names from the page-headings; the reader presumably knows that there is a *Journal of General Psychology* or he would not be reading it, and he does not need to be reminded of the fact on every even-numbered page; what he wants in a scientific journal is to be able to find quickly an article by a given author without having to use the index or to translate the title into its author. Institutions and their addresses are put in prominent italics at the end of each article: it seems odd to separate the author so far from his address or to give the address such prominence. In general, though not always, quotation marks have been made to follow the laws of logic rather than those of typographical convention; that is to say, punctuation is put within the marks when it is quoted and without when it is not. If this change is a reform, well and good—even though it bother the eye.

Questions of consistency are the bane of the editor's life. When should the author have his way and when not? There is no general answer, and yet consistency seems desirable when it does not interfere with meaning. The reviewer believes that the editors of the new journal should seek conformity in the following matters. Some rule for quotation marks should be adopted and rigidly followed. Common words should be spelled alike: Husband says "thru" and "tho," but no one else adopts the radical orthography. If the rule is to put the laboratory origin of the article in a foot-note and the author's institution and address at the end, then a different usage should not have been adopted for Bartlett. Titchener invented *O* and *S* as translations for the German "*Vpn.*" (and its variants), but the Cornell and Clark articles use "*O*" and the others write out "subject." Most of the references are complete, but Spearman omits page references. Two articles cite the literature by a "Bibliography" at the end and a parenthetical reference to it by number; all the other articles, when they cite literature at all, use foot-notes. Nevertheless consistency is but a minor matter for editors to worry over. *The Journal of General Psychology* will stand or fall by its content of psychology, whatever its typographical uniformity.

E. G. B.

Mental Tests in Clinical Practice. By F. L. WELLS. Yonkers-on-Hudson, World Book Co., 1927. Pp. x, 315.

This book is one of those rare and delightful demonstrations of complete mastery of the art of scientific authorship. The author moves with masterly precision through a welter of literature carefully classifying arranging and evaluating so that those who follow in his foot-steps may profit most by his experience and genius. He is quick to appreciate and slow to condemn, is gracious even when he disagrees and is enviably clear in expression. The result is so thorough a summary of the literature of mental testing that hereafter no psychologists will care to work long without having Wells within easy reach.

The reviewer doubts if one can easily over-emphasize the serviceableness of this book. The reference lists are extensive and are arranged in perfect bibliographic form. There is a general reference list at the end of the volume of 159 titles and in addition there are supplementary lists with most of the chapters which bring the total number of titles well above four hundred. At the end of nearly every chapter there is a presentation of typical cases which taken together number fifty-five. These alone would be a valuable contribution. They show quite clearly how much, and at times how little, the psychometric scores contribute to a complete understanding of particular case problems. The practical tester and the clinical psychologist will also find the tables include, especially those in the appendix, matter of much every-day working value.

Of course there is a chapter on general methods and another on the Stanford Scale, but there is also a full discussion of the Kuhlmann Scale. There is also a summary of the controversy over the possible merits of the point-scale method. The problems and findings of group testing appear in one chapter. The so-called performance tests occupy another. The author's own method for memory examination is reproduced in one chapter, the literature on the free association experiment is digested in another, and then there are two final chapters on personality tests and the vocational uses of tests supplemented by a very pratical presentation of office methods for the clinical psychologist which is inclusive to the most minute detail. In all of this the author is not much inclined to express his own opinion. He keeps himself well in the background, so well in fact that one sees the current trend of discussion and practice in each chapter almost forgetting that he is seeing it all through the critically evaluating eyes of the author. Occasionally, however, the author's own preferences or opinions appear. He defines a mental test as being a psychological experiment which has been standardized. Concerning the much discussed constancy of the I.Q. he admits that there are occasional changes of rating but thinks that in the hands of competent examiners such variations may be safely discounted. In the controversy over the average age of adult intelligence the author thinks that 14-year performance most nearly expresses that of the average adult. On the significance of scatter the author is cautious. He says that he has found scatter so common in normal children that it cannot be safely stressed as a pathological sign; but when adults scatter over five or more year levels he thinks it proper to raise the question of organic or toxic involvement. He is careful to add, however, that a regular performance cannot be interpreted as excluding such involvement. But interesting and important as all this is the merit of the book lies not so much in the author's own opinions and preferences as in the survey which he gives of the general trend in the psychological world of mental tests and the almost innumerable aids which he supplies to the wise use of psychometric methods.

University of Oregon EDMUND S. CONKLIN

L'Intelligence. By BOURDON. Paris, Libraire Félix Alcan, 1926. Pp. 338.

The volume is devoted to a study of higher processes which the author distinguishes from sensory phenomena because of the fact that the latter are the primordial phenomena from which the higher processes are derived, in the sense that they presuppose sensory data. Due to the fact of experimental accessibility, the sensory processes are comparatively well understood. He notes the difficulties involved in the method of introspection, especially the difficulty of separating what has been observed from that which has been inferred. Nevertheless, he does not propose to abandon introspection, but promises to make a serious effort to separate the phenomenal from the inferred. Despite the fact that our knowledge of the higher processes is incomplete, what we do know about them is today far beyond the knowledge contained in old treatises of psychology.

He notes the rudimentary state of our knowledge regarding the cerebral accompaniment of thought. He pleads this fact as an excuse for his relative neglect of neural conditions. He charges those who discuss intelligence in terms of neural processes with merely accumulating hypotheses—to the reviewer's mind, a well-grounded objection to many writers. Surely we cannot dispel our ignorance concerning the mystery of thought by data ('synapses,' etc.) regarding which we know even less. The author observes that to explain thought processes in neural terms generally amounts to substituting new designations for familiar terms, without otherwise 'helping' the situation.

The mental contents he examines (as shown by the succeeding chapters) are immediate images (immediate memory); representations, types of imagery in which he includes Ribot's affective type. There is a chapter on *Sentiments Intellectuels* in which he considers imageless thought, the *Bewusstseinslagen* of the Germans, and the problem of *meaning* as handled by English and American psychologists. There are chapters on perception, association, judgment in general, and a special chapter on judgments of resemblance, chapters on illusion and hallucination—in fact it is, with the exception of chapters on sensory phenomena, a complete textbook. However, emphasis is placed on higher processes and he does bring in impartially data from many foreign sources. His data has the virtue of being genuinely psychological data—he neither tries to substitute neural mechanics for mind, nor to substitute chapters from a logic text for psychology of the thought processes. Both of these errors are rampant within our shores.

Wells College C. O. WEBER

Two Souls in One Body? By H. H. GODDARD. New York, Dodd, Mead and Co., 1927. Pp. ix, 242.

Readers of this journal have doubtless been aware for a long time that Dr. Goddard was working on a genuine case of mutiple personality and will therefore welcome this publication of his study. The story is the now familiar one of a girl in delicate health subjected to excessive tension and emotional distress culminating in later adolescent years in a thoroughgoing breakdown. Two personalities were clearly manifest, one that of a child and the other of a girl in later adolescence. Under mental tests one gave a mental age of four years and the other of sixteen years. Another state appeared a few times which some less cautious investigator might have exploited as a third personality. The interpretation offered is in terms of Janet's theory of a weak synthesis dissociated by emotional shock. The dichotomy of character traits and memories is well established.

There is an ample and interesting presentation of the behavior of the two personalities and of the process of changing from one into the other. The history prior to the break is exceptionally well summarized.

The case is in many respects much more simple than the famous Beauchamp case and is more concisely presented. It is illuminated by some instructive photographs of the patient in different states and also of her hand-writing. The book will therefore be of great service, especially as a type case for student reading. It was a happy thought also to include in the appendix a reprinting of the long famous Mary Reynolds case, thus making that oft quoted report much more accessible.

But the design of the author was not exclusively that of producing a clean-cut report of an instructive case for scientific uses. His aim was avowedly that of using the case as the basis also for an appeal to the general public for a better understanding of the problems of child health and of the public responsibility for its care. He also hoped that he might deal popular superstitions an effective blow. These two supplementary aims have quite naturally influenced both the style of the book and the selection of the material. Here and there the author drops into an hortatory vein which is sometimes almost impassioned. There are also included in the body of the text letters from ignorant cranks, aroused doubtless by the newspaper publicity which the case has received, with many more in the appendix, as well as a curious discussion of the problem of "two souls" which is reflected in the title selected for the book. While no one would question the desirability of awakening the general public, if possible, to a clearer consciousness of its responsibility in such cases, many scientific readers may question the wisdom of attempting to present so valuable a case as this in a fashion to appeal at once both to the tyro and to the technician.

University of Oregon EDMUND S. CONKLIN

Die Persönlichkeit des dreijährigen Kindes. By ELSA KÖHLER. Leipzig, S. Hirzel, 1926. Pp. ix, 240.

The author designates Karl Bühler's developmental theory as the one which is central to the present study. In Bühler's *Geistigen Entwickelung des Kindes* (Jena, 1924) occurs an analysis of the one-year-old child, and Charlotte Bühler has extended his basic principles to the study of the period of puberty. The present study is offered as a connecting link between the two above studies. The author maintains that the third year presents us with remarkable strides in development. The 'third year' in this book is defined as the age lying between 2 years and 6 months and 3 years and 6 months. As regards method, this study is an acceptable product of observation, experimentation, and theoretical interpretation. She ignores mass data, and her empirical findings are drawn almost entirely from her intensive study of one case (*Annschen H.*), a little girl of normal antecedents and character.

The research portions of the book are three in number. The first portion (covering the period from 2.5 until 2.7) deals with the manner in which this child evolves and represents her 'world-view' (*Weltbildes*). However, this involves very concrete observations regarding the child's sense perception in general, color discrimination, space and time perception, and her conception of number. This includes also the child's conceptual world —her notions of law and causality. Also her language mechanisms, vocabulary, grammar, imagery. Part II is concerned with finding a synthetic view of the convergence of external impressions upon inner development. This study covers the period 2.8 to 2.9 inclusively. The author then discontinued her study until A was 3.0 years old. The third study (3.0 until

3.6) is devoted to research regarding A's recollections of the earlier periods, her new attitudes, etc. The chief method employed throughout is that of carefully questioning her subject.

Out of the vast detail of personal data which the study displays regarding this child, we may indicate the following as the most general findings. The first period is dominantly phenomenological. A's senses are not fully developed, she lives in a 'present' world since the past and future are extremely dim notions to her, she has a vague notion of time, the mystery of number is wholly unsolved. Feeling and willing are barely distinguishable. Development is very rapid, however, and in the second period she has already developed a sense of personal identity, which appears *pari passau* with the clear separation she makes between herself and objects, her separation of present from past time, her recognition of relationship to her parents, etc. Her willing and feeling center around this new self-awareness. In the third period, the self is well established, and interests (volitional and affective) turn towards the external world. The impulse *to know* this world—to put it in a scheme—becomes dominant. She holds a criterion of value; the valuable is that which can be understood, which can be grasped as an element in a schema.

Wells College C. O. Weber

Psychotherapie. Ein Lehrbuch für Studierende und Ärzte. By Max Isserlin. Berlin, J. Springer, 1926. Pp. 205.

This psychotherapy represents in the main a collection of "practically usable therapeutic methods and techniques." The book consists of a 'general' and a 'special' psychotherapy. In the first part, hypnotherapy, educational therapy, and psychoanalysis are considered; in the second part the therapy of different symptoms and the problem of 'abnormal constitutions' are discussed.

In contrast to Liébault, Bernheim, and Charcot, Isserlin distinguishes only two degrees or stages of hypnosis. Of considerable importance for therapy is his description of abnormal phenomena in the hypnotic state. A special effort is made to clarify the nature of suggestion. The psychologist will welcome the detailed account of the techniques of hypnotization. As regards educational therapy, the method of persuasion as used by Dubois and Déjérine receives due consideration. Volitional gymnastics, inhibition gymnastics, association therapy, psychomotor discipline, and occupational therapy are discussed. A good account of the development of the Freudian views and a critical analysis of these views are given. The conclusion is drawn that the method of association, the exploration of dreams and the exploration in the hypnotic state are practically usable methods.

This book is of special value to the physician who has to cope with problems of psychotherapy, and to anyone requiring information about modern psychotherapeutic procedures. The psychologist who considers an analysis of normal functions as basic for a therapy of abnormal and pathological phenomena will be somewhat disappointed. Isserlin starts from the techniques available. A book such as Kronfeld's psychotherapy is of greater interest to the psychologist since its point of departure is "the structure of personality, character, and disease picture." Although Isserlin does not present even the fundamentals of his medical psychology, his book contains a large number of psychologically interesting observations.

It is gratifying to note that the author does not overestimate the possibilities of psychotherapy since he considers the majority of mental disorders in this field as 'constitutional' diseases. Complete cure, therefore, can be effected in a limited number of cases.

Columbia University Heinrich Klüver

The Ways of Knowing or The Methods of Philosophy. By WM. PEPPERELL MONTAGUE. New York, The Macmillan Co., 1925. Pp. 427.

This is an essay in reconciliation, inspired as to method by Sidgwick's well-known *Methods of Ethics*, and as to content by the conviction that if "the methodological inquiry into the Ways of Knowing" were isolated from "the metaphysical inquiry into the Ways of Being" with which it has been confused in much of modern philosophy, and the extent of the latent agreement between rival views made clear, much time would be saved for the more serious business of philosophy (whose "primary interest is in the ways of things rather than in the ways of knowing them"), *i.e.* for metaphysics and theory of value.

Montague divided Methodology into Logic and Epistemology and his book into two corresponding parts: Part I, Ways of Attaining Knowledge—The Six Methods of Logic (Authoritarianism, Mysticism, Rationalism, Empiricism, Pragmatism, and Scepticism). Part II, Ways of Interpreting Knowledge—The Three Methods of Epistemology (Objectivism, Epistemological Dualism, and Subjectivism). He concludes each Part with a conciliatory chapter, attempting to show in the case of Part I that "The methods of logic, despite their differences, are capable of being combined in a federation in which each performs a more or less distinct and more or less valid and valuable function in the many-sided enterprise of attaining knowledge;" and in the case of Part II that "the three methods of epistemology, when reinterpreted in the manner explained in this chapter, are each of them true and each of them consistent with and supplementary of the others."

The last hundred pages of the book are occupied by a dialogue entitled "The Knower and the Known," one of the adroitest things of its kind in recent philosophical literature, in which Montague's essential argument is restated in simpler terms. Hylanous, a Realist, has no difficulty in showing Partridge, an Objectivist, Bryce, a Subjectivist, and Lovelace, a Dualist, that the truth of their respective epistemological positions is contained in his own very catholic one, and that "in the light of his eirenicon such concepts as truth and error can be translated without loss of meaning from any epistemology into any other."

Philosophers will quarrel with Professor Montague's stretching of the term 'logic,' reject his reinterpretations, and remain unconvinced by his reconcilations; but psychologists wishing to keep abreast of development in epistemology will find here a fairly complete marshalling of current views, and in the case of several of them expositions which have not been surpassed within the same compass.

Cornell University M. H. FISCH

Psychology in Business Relations. By A. J. SNOW. Chicago, A. W Shaw Co., 1925. Pp. 554.

Snow attempts, as he states in his preface, to combine in one book the study of all the branches of business psychology for the benefit of the business man and the student of business. All discussion of the technicalities of psychology has been omitted. It is only fair to the author that this objective be kept in mind in surveying the book. One should remember, too, that writers of business books have a tendency to see the business man as a "tired business man" whose mental food must be palatable and not too heavy. Judging from business reviews the work has 'gone over' with its audience; and that is the measure of its success.

The author has chosen from the numerous sytems that one which best suited his purposes. The reader is introduced to this system in Chapter I, "The Fundamental Nature of Man." Just what the system is that has been chosen, the reviewer cannot say. He thought at first it was a clear-cut behaviorism, but the frequent use of such concepts as feelings, desires, volitional, and rational actions, shows that such is not the case. The fact is that the most important concept that can be played upon in business psychology is 'motivation' of behavior, and for practical purposes it makes no difference whether one adopts the term instinct, desire, reflex chain, or any other as its mechanism. Snow does introduce the business man to the instinct controversy, and tells him that 'instinct' is a foolish notion, whereupon the acquaintance ceases.

The book is divided into five parts which deal with consumer, marketing, advertising, selling, and employment problems. The treatment of each topic is necessarily limited, and the material offers nothing of particular interest to the psychologist. Numerous references to psychological and business literature following each chapter would enable the business reader to study his subject more thoroughly if he should choose to do so.

Columbia University A. T. POFFENBERGER

A Manual of Individual Mental Tests and Testing. By AUGUSTA F. BRONNER, WILLIAM HEALY, GLADYS M. LOWE, MYRA S. SHIMBERG. Boston, Little, Brown & Co., 1927. Pp. x, 287.

This volume is classed as number four of the Judge Baker Foundation Publications. The manual had its growth in mental testing for the purpose of guidance in educational, personality, and conduct problems. The authors seek "to combat the uncritical acceptance of very narrow and simple measurements as offering adequate criteria for judgment of mental capacities," and urge the "great urgency for utilization of a much wider range of tests that the human individual may be better known in regard to his various mental capacities, in order to relate his abilities to his possible social, educational, vocational adjustments and achievements." Detailed account is given of those tests only that are not members of a scale. Mere mention is accorded 'omnibus tests,' educational, vocational, industrial, temperament tests and scales. Part Two is stated to include "every adequately standardized individual mental test." Full information is presented for the giving, scoring, and numerical evaluation of a large number of tests. The bibliography contains 319 titles.

The manual shows much diligence and is a handy volume which will be appreciated by anyone who gives it careful consideration.

University of Vermont PAUL C. SQUIRES

Problems of Philosophy. An Introductory Survey. By G. WATTS CUNNINGHAM. New York, Henry Holt & Co, 1924. Pp. xvii, 453.

The Principles and Problems of Philosophy. By ROY WOOD SELLARS. New York, The Macmillan Co., 1926. Pp. xiv, 517.

A little philosophy is a dangerous thing, especially if got in the usual 'introductory' course; and it may well be questioned whether the college undergraduate who has not time for the more leisurely and thorough introduction by way of logic, ethics, and the history of philosophy, might not be better off without the hasty and necessarily misleading survey which has become so popular in recent years. But if the promiscuous samplers who crowd our universities and colleges must continue to be given their nibble of philosophy along with all the other nibbles, there are probably no better textbooks for the purpose than the two before me. What is more to the point of their being noticed here, I know of none which would be more useful to the advanced student or younger teacher of psychology who has had no technical training in philosophy. They are of equal competence; the differences are in point of view and distribution of emphasis, and cannot be adequately indicated here. Labels are of doubtful value, but those which the authors would find least objectionable for their respective points of view are, I take it, 'objective idealism' in the case of Professor Cunningham, 'evolutionary naturalism' in that of Professor Sellars. *The Principles and Problems of Philosophy* is "partly a revision, partly a supplementation," of Professor Sellars' earlier book, *The Essentials of Philosophy*, and, like it, follows the epistemological approach, tending to make central the problems presented by the natural sciences. Professor Cunningham's approach is more logical (in the Hegelian sense) and his emphasis more humanistic. But there is no dodging of important issues on the part of either. Of special interest to psychologists are Professor Sellars' chapter on "Psychology as a Natural Science" and Professor Cunningham's critical survey of theories of mind. Both books are equipped with abundant references to the literature of philosophy, and good indices.

Cornell University M. M. Fisch

Shell Shock and Its Aftermath. By Norman Fenton, with an introduction by Thomas W. Salmon. St. Louis, C. V. Mosby Co., 1926. Pp. 173.

Whoever wants information on any aspect of shell shock will do well to turn first to this volume. The available information on about every aspect of the problem is here systematically and pains-takingly summarized, with the addition of much new and valuable data. Statistical studies are reported on a very large number of topics including the proportions of officers and of men afflicted, the proportion of drafted *versus* volunteer soldiers involved, relation to different branches of the service, age distributions, personal and family histories, social and economic background, and relation to intelligence distribution. Within this small volume there are thirty-seven statistical tables, twelve graphs, and, as indicative of the literature examined, there are one hundred and seventy four footnote references. A notable addition to our knowledge is the author's extensive study of shell shock cases since the war, of their health and of what has has been done for them.

All this might lead the reader to expect some final statements concerning the nature of shell shock. But on this the author is very cautious. He doubts the sufficiency of the interpretations extant, and urges more consideration of shell shock in terms of the psychology of habit—especially because of the ease with which shell shock habits once established are found to be rearoused by any intensely emotional experience. He also urges more investigation of the shell shock by the technique of physiological chemistry before much faith is placed in any of the interpretation offered.

The reviewer turns from this volume with the wish that it were not quite so highly condensed, that it did not have quite so much the appearance of a doctoral dissertation. With so much knowledge at his command the author could have produced a more extended presentation which would have served a larger circle of readers than any existing volume on the subject, and which would in all probability have done more to stimulate the studies of psychoneurosis in industrial life which Dr. Salmon, who introduces the volume, properly hopes may be the consequence of the author's work.

University of Oregon Edmund S. Conklin

Mental Tests, their History, Principles and Applications. By Frank N. Freeman. Riverside Textbooks in Education, 1926. Pp. ix, 503.

The aim of this book is "to give an account of all the important types of mental tests." There is an historical organization of the general descriptive part of the text. The hope is expressed that the book may be of interest to laymen as well as to students of psychology and education.

The first part of the book is devoted to a description of tests, together with their valuation. Applications of mental tests are considered in the latter part of the work. The most significant applications are those in the field of scholastic problems, while next in importance are the applications of tests to vocational guidance and selection. The relation of mental tests to delinquency problems is considered. There is a discussion concerning the interpretation of differences between residential, occupational and racial groups, with the result that a 'middle-ground' is assumed by Professor Freeman.

Considerable attention is given to Spearman's theory. The closing chapter treats of the nature of intelligence. The author maintains that the Spearman 'G' is "unduly complex and rather difficult to conceive in its application." He holds that the hypothesis of the 'G' fails to fit any "known facts concerning the evolution of the intellect." Freeman proposes an hypothesis that is considered by him more adequately to fulfill the demands of the psychological description of intelligence and of the evolution of the brain. The position is taken that "the statistical evidence, so far as it goes, favors the notion that intelligence rests upon some more or less unitary factor." This book recommends itself to consideration as an introductory volume.

University of Vermont PAUL C. SQUIRES

The Psychology of Time. By MARY STURT. New York, Harcourt, Brace & Co., 1925. Pp. 150.

We find in this book a somewhat different approach to the study of time from that which we have learned to expect in experimental psychology. The author is chiefly concerned with the common-sense conception and with its development. She gives us a discussion of the evolution of the experience in the animal series, in the race, and in the individual. The growth of the concept in the child is examined with the aid of numerous test experiments.

The book will be disappointing to one who goes to it for a systematic review and critique of the experimental researches on the subject, or to one who seeks a discussion of modern metaphysical notions of time. The experimental work of Benussi in Germany and of Curtis in this country is neglected.

The chief merit of the book lies in the fact that it shows the importance of the genetic problem and that it suggests a new method of approach in the study of the time experience of the child. A book which summarizes and evaluates the experimental work has still to be written.

University of Kansas J. P. GUILFORD

Problems in Educational Administration. By GEORGE D. STRAYER, N. L. ENGELHARDT, and others. New York, Bureau of Publ., Teachers College, Columbia Univ., 1925. Pp. xvii, 755.

This volume applies to the field of educational administration, the so-called "case" method of instruction. Each problem describes a situation (usually an actual one), giving pertinent data and suggesting the specific questions involved. In this way the student is encouraged to begin his thinking in terms of actual conditions to be met, and from there to come, presumably, to the development of governing principles and a philosophy of administration. The chief danger in having a textbook devoted largely to a statement of problems is, obviously, that it may tend to crystallize the nature of the problems considered by the student.

The one hundred and sixteen problems in the book cover the important phases of school administration—national and state responsibility, local administration, financing schools, business administration, school publicity, buildings and equipment, classification and progress of children, personnel management, and the like. While administrative techniques are emphasized, the implications of these in terms of a philosophy of administration are not overlooked. The various bibliographies give perhaps the best set of selected references on general administrative problems that has yet been published. No alert worker in school administration can afford to neglect this volume.

Cornell University JULIAN E. BUTTERWORTH

A Short Outline of Comparative Psychology. By C. J. WARDEN. New York, W. W. Norton & Co., 1927. Pp. 96

Beginning with ancient lore, Dr. Warden, who writes from the animal laboratory of Columbia University, traces down to the present the history of the interpretation of animal behavior. He points out that pictures still extant suggest observation of animals as early as the Cro-Magnon period.

Dr. Warden's survey should provide profitable reading not only for biologists and psychologists but for all who wish to be well informed. The book, in addition to furnishing a background of fact, arouses curiosity concerning the investigations mentioned. References, however, are not given in the book. They may be obtained from the publishers who upon request will supply a general bibliography on the subject and a selected bibliography which corresponds to the five chapters of the book. These bibliographies, the reviewer feels, should be included in the book itself as they not only give the reader the source of the material, but add the weight of authority to the book.

A disproportionate amount of space is given to the work done at Columbia. The recent work of European comparative psychologists is ignored. The work of Köhler, for instance, is not mentioned. Except for the unexplanable omissions, this volume, the historical part of a forthcoming textbook, augers well for the complete book.

Cornell University FLORENCE WOOLSEY HAZZARD

Religion in the Making. A. N. WHITEHEAD. New York, The Macmillan Co., 1926. Pp. 160.

This small book, made up of four Lowell Lectures for 1926, entitled respetively Religion in History, Religion and Dogma, Body and Spirit, Truth and Criticism, does not call for extensive review in a psychological journal. Its purpose is "to give a concise analysis of the various factors in human nature which go to form religion, to exhibit the inevitable transformation of religion with the transformation of knowledge, and more especially to direct attention to the foundation of religion on our apprehension of those permanent elements by reason of which there is a stable order in the world, permanent elements apart from which there could be no changing world."

A few of the many phrases by which Whitehead describes 'religion' may serve to give some idea of the direction taken by his thoughts: "religion is force of belief cleansing the inward parts;" "religion is the art and the theory of the internal life of man so far as it depends on the man himself and on what is permanent in the natures of things;" "religion is what man does with his solitariness;" "religion is world loyalty;" "God is the one systematic, complete fact, which is the antecedent ground conditioning every creative act."

Bryn Mawr College JAMES H. LEUBA

Measurement in Secondary Education. By P. M. SYMONDS. New York, The Macmillan Co., 1927. Pp. xvii, 588.

As Symonds states it, the purpose of this volume is to bring together the material used in and derived from measurement in secondary schools, as well as to discuss the techniques employed. Inasmuch as this material and technique are spread through many periodicals, the volume makes them much more easily available for school men and students for whom the text is intended. The book describes in elementary fashion the standardized tests available in high school subjects, and for guidance in the clerical and mechanical vocations. Prediction of success in high school and college, ability grouping, tests of personality traits, and unreliability of marks all come in for some discussion.

The chapters on norms and standards, and how to handle the results of testing suffer from the defect common to many other discussions of similar topics: statistical terms such as coefficient of correlation, probable error, standard deviation, are arbitrarily used; but their significance and limitations are not clearly indicated. The danger here lies in the fact that misinterpretations and misuses might follow—a not infrequent occurrence in the past.

The author, however, has assembled valuable material which will be useful to the critical student.

Cornell University FRANK S. FREEMAN

Psychologie et Sociologie. By DANIEL ESSERTIER. Paris, Félix Alcan, 1927. Pp. 234.

This ambitious but well accomplished undertaking furnishes a guide to the vast literature that deals with the relation of psychology to sociology. The literature has been classified under five general headings. In the first are included periodicals, general treatises, text books and source books, all sociological in nature. The second contains a list of books illustrating the origin and evolution of the problem of the relation between the two sciences. The third enumerates books and articles dealing with questions of social psychology. In the fourth section, entitled "psychological sociology," are listed references to the topics of sociological interpretation of sentiments, intellectual functions and personality, while the fifth section is concerned with works on psychopathology, the rôle of instinct, and the psychology of value in relation to sociology.

The value of the bibliography is enhanced by short comments added to practically each one of the 622 books and articles listed, which either indicate the general position maintained by the author or briefly state the content. The importance of the book for sociologists lies in the fact that most of the listed writings approach the problem from the point of view of the methodological procedure in sociology. It should also be of interest to the psychologist, to whom it may offer a general orientation in the scope of psychological theory in the social sciences.

Essertier stresses the point throughout that the collaboration between psychology and sociology is not only useful and necessary but indispensable.

University of Illinois T. F. ABEL

The Seven Seals of Science. By JOSEPH MAYER. New Yo n-tury Co., 1927. Pp. xiv, 444.

This is a well-written, popular book on the history of science, where the lack of books makes this one welcome. The 'seven sea mathematics, astronomy, physics, chemistry, geology, biology, and chology, and the author's thesis is that the development of the scienc in some such order is the result of the unsealing of each by advances in those others that are necessarily its precursors. The author also hints that psychology may break an eighth seal for social science.

Dr. Mayer has written carefully and the result is very readable. He still speaks of the dark ages of science, though he shows some knowledge that Sarton has broken an historical seal, letting much light in upon and out from this hitherto obscure period.

It is interesting, of course, to find psychology admitted to the sisterhood of sciences. The forty-one pages on "modern psychology" are not, however, for the psychologist to read; the picture is too simple and the perspective too disputable. From James, according to the author, "the modern period in psychology may be said to have taken form," even though "Wundt is rightly called the father of modern psychology." The first section is almost entirely devoted to James. The second on experimental psychology, centers about Wundt. The third, on abnormal psychology, deals with Pinel, Mesmer, Coué, Charcot, Morton Prince, Janet and Freud. The fourth is on comparative psychology in the broad sense, and the last is on theory.

A psychologist might have written with more accurate perspective, but the fact remains that most psychologists do not write at this level and not many seem able to write so well.

E. G. B.

Thought and the Brain. By H. Piéron. English translation by C. K. Ogden. New York, Harcourt, Brace & Co., 1927. Pp. 255.

Piéron, in *Thought and the Brain,* puts in perspective the facts relevant to the most fundamental problems of the relation of mind and body. The book is divided into four sections.

The first part deals with the general conception of neural-mental functioning. The problem of the localization of cerebral function does not seem as well understood as the function of reflex organization. Piéron shows that neither the position that cerebral localization can be determined by the lesion of well-defined partial areas nor its opposite of general disturbance is comparable with the new facts, particularly those of war pathology. The function of the cortex is differentiated as (1) incito-associative receptor centers; (2) incito-motor centers; (3) receptive coördination centers; (4) incito-motor coördination centers. The fallacy of attempting to localize intelligence is shown by the multiplicity of possible pathways. The localization of faculties must give place to dynamic conceptions involving privileged synapses and variations within the sensory nuclei themselves.

The second part of the book deals especially with the incito-motor function of the brain. The elementary modes of sensory-motor thought are not confined to the cerebral cortex, but sub-cortical responses do not involve association. The principle of explaining physical processes by the idea of amoebial movements of the neuronic elements must give way to one that takes account of the functional individuality of the neurones themselves. The specific character of responses is explained by a non-technical exposition of the Chronaxy theory of Lapicque.

The third part concerns the cerebral mechanism of language and verbal thought. These involve a high degree of integration and include both cortical and sub-cortical mechanisms. It is the predominance of these centers in certain areas of the cerebral that explains the appearance of asphasic disturbances as resulting from limited lesions of the brain. Verbal localization is thus limited somewhat vaguely in the areas through which the great association pathways pass.

The fourth and final part of the book deals with the rôle and mechanism of affective regulation in mental life. The study of cerebral associative mechanisms, on which thought and perception depend, does not exhaust the problems of mental activity. Mental activation is a function of an expenditure of nervous energy, the liberation of which is termed 'interest.' This varies with the predominant affective like tendency, which functions to partially canalize it. The facts now show that thalamic activation suffices to govern the general behavior of the organism, acting as a primitive mode of response to a situation.

In his treatment, Piéron gives much that is new, both factually and theoretically. Without bias in favor of any particular theory, the author confines himself to the examination of questions which can be stated with some precision in view of the scientific knowledge now at our disposal. The book is a fairly complete synthesis of European and American research, with the contributions of the war lesions to neurology and the latest work on asphasia receiving the major amount of attention. It is in keeping with the present dynamic trend of biological and psychological explanation, and will prove helpful to those interested in the correlation of mind and body.

Cornell University G. L. Freeman

Tests and Measurements in High School Instruction. By G. M. Rich and George D. Stoddard. New York and Chicago, World Book Co., 1927. Pp. xix, 381.

This book is essentially a resumé of existing tests and test methods in the field of high school instruction. The volume consists of four parts. The first part includes such topics as the historical background of measurement in secondary schools, the uses and limitations of various types of tests in the high school, and the criteria utilized in the selection of educational tests. The second part considers tests for such subjects as mathematics, the languages, science, and history. There is also included a discussion of mechanical and trade tests, commercial and music tests, survey tests, general intelligence tests and Junior high school tests. The third part deals with the informal objective examination methods, while the final part treats of the construction of educational and mental tests. Samples of topics in the fourth part are as follows: test validation, breaking a test into comparable forms, derivation of norms, determination of reliability, and perfecting the administration of a test.

This volume strikes a fair balance between theory and application. It is a most timely contribution in its special field. One of the attractive aspects of the work is to be found in those chapters that set forth principles of test construction. These chapters are of considerable assistance to the teacher, or whomever the person interested may be, in obtaining some insight into the principles involved in test construction. It may be said of this book that it is well adapted to serve as a stimulant toward the further solution of specific educational problems.

University of Vermont Paul C. Squires

Vorsatz, Wille, und Bedürfnis. Mit Vorbemerkungen über die psychischen Kräfte und Energien und die Struktur der Seele. By Kurt Lewin. Berlin, J. Springer, 1926. Pp. 92.

Lewin's study is a contribution to the psychology of affect and action. The first part brings some general remarks on the nature of psychic processes and on the problems involved in psychological experimentation; the second part deals with the facts and the theory of "intention, will, and need." The author advocates a sharp separation of "phenomenological" and "causal-dynamic" concepts. In other words: the results reached by objective observation and introspection, by the description of overt and implicit behavior, are not necessarily important in causal respect. Phenotypically, similar or identical situations or processes are not always equivalent from a causal point of view. It is only the recourse to the conditioning factors which makes a phenotype scientifically meaningful. Stated succinctly, "introspectively" or "objectively" ascertained relations are not necessarily causal relations. This may be a commonplace to *Wissenschaftstheoretiker*, but by overlooking this fact a large number of controversies in psychology arise.

In considering the facts bearing on voluntary action, the author brings to light the "conditional-genetic" importance of the "*Quasibedürfnis*." "*Quasibedürfnis*" and "situation" definitely determine a concrete psychological process in phenotypical respect. It is questionable whether a more detailed psychological analysis of *Bedürfnis* upon which the *Quasibedürfnis* is dependent would justify dismissing the question of value as Lewin does.

Columbia University HEINRICH KLÜVER

Le Peyotl: La plante qui fait les yeux émerveillés. ALEXANDRE ROUHIER. Paris, G. Doin, 1927. Pp. xii, 371.

In this monograph, written by a Doctor of Pharmacy, will be found the most comprehensive study of peyote (mescal buttons, *Anhalonium Lewinii*) yet published. Not a great portion of the monograph is of general interest to psychologists but all of it would be invaluable to anyone working with the drug and its effects. Working from a pharmaceutical point of view, the author differentiates two subspecies of the plant. He advocates its therapeutic use as tone-sedative and as a stimulant of the nervous system.

Part I (pp. 3–86) is concerned with the geographical and botanical origins of Peyote. Part II (871–76) gives an outline of the history and ethnology of the drug. It is this and the next parts which are of most interest for psychologists. A good deal of space is given to descriptions of the peyote cults among different tribes of Mexican and American Indians. Although this material is not new, it is well summarized and is in a convenient form which should be of great interest to the social psychologist interested in religion and its development.

The last and longest part of the monograph (177–371) discusses the chemistry, pharmacology, and therapeutic possibilities of peyote. In this section, the author describes in some detail the 'visions' of some of the previous workers along with some new observations of his own. In these new observations, four of which are given in great detail, the author falls into the common error of over-emphasizing the visual reactions obtained with the use of the drug. Although it may be interesting to know what Havelock Ellis, Weir Mitchell, and now Rouhier see in the way of visions produced by ingesting peyote, such descriptions do not materially advance our knowledge of the effects of the drug. Certain physiological effects are also discussed. The reviewer regrets that these are not described with the amount of detail given to the 'visions.' Such a contribution would have been of great value.

The book contains a good many illustrations and a bibliography of 136 titles—which although large is far from complete.

University of Pennsylvania SAMUEL W. FERNBERGER

Elements of Educational Psychology. By L. A. AVERILL. Riverside Textbooks in Education. New York, Houghton Mifflin Co., 1924. Pp. xii, 425.

This volume is arranged in 17 lessons, designed as a rather detailed and intensive work for teachers-in-training. Class experiments are introduced at the beginning of ten of the lessons, and lists of selected references and questions for discussion are appended to the different sections. The presentation is clear and informal, but occasionally it seems a bit wordy.

Cornell University H. D. POWERS

NOTES AND DISCUSSIONS

Certain Points Concerning the Reliabilities of Experiments in Psychology

The purpose of this paper is (*1*) to give a short review of a controversy concerning the reliability of experiments in psychology which has injected itself into the literature during the last four years; and (*2*) against this background (*a*) to supplement the author's monograph on proofreader's illusions[1] with the employment of methods of calculation more accurate than certain of those which were published in 1924; (*b*) to demonstrate that our experiment as a whole was so reliable as to more than meet the statistical precepts of both Hunter and Kelley in the matter of differences between the various conditions of reading proof with all *S*s treated as a single group; (*c*) to restrict certain conclusions in the monograph to the few cases of *S*s involved; and (*d*) to offer the suggestion that, if our results were characterized by high reliability, that is, if one proofsheet can test an *S* as adequately as can five sheets, or if one series of sheets can satisfactorily replace four series, the method as employed and the sheets which were used give some hopeful promise of being useful in future vocational testing programs in the typography and journalism fields.

(*1*) Hunter and his students—Heron,[2] and Randolph[3]—and Davis and Tolman,[4] have demonstrated that the records of white rats in alternate runs in a maze do not give coefficients of high correlation and that the records of their running in two or more mazes show low coefficients of correlation. The correlations under consideration are employed as indices of the reliability of the experiment, and if high are thought to prove that the same ability is being tested. The values obtained by the above four investigators induced Hunter to suggest at the meeting of the A.P.A. in Washington, December, 1924, that all such maze studies are too unreliable for the measurement of individual differences, and of group differences as well. Carr, a pioneer in maze-investigations, took exception to Hunter's position, stating that, although his "hat was not satisfactory for eating, it still served a useful purpose" in covering his head, and that "maze-studies will continue at the University of Chicago." The discussion was continued in the literature by Carr[5] and Hunter,[6] the former saying that Hunter's criticisms of animal maze-studies are just as applicable to all other experiments in psychology as to the maze alone, and that the matter of 'unreliability' is a question of statistics, not of experimental technique. To this Hunter replied with a quotation from Kelley, to the effect that it is unwise to attempt, with a population as small as 30 *S*s, a study of group differences unless a reliability is demonstrated to be as high as 0.4, and that with the same population individual differences should not be attempted unless the reliability is proven to be as high as 0.9. In the meantime, Murchison,[7] taking umbrage at the criticisms of Boring,[8] launched an attack upon the various papers of the latter, his chief criticism being that Boring had employed too few cases as the basis for the rather general conclusions at which he had arrived. In the literature one meets the assumption frequently that the mere increase of the number of *Ss* tested will remove some of the unreliability of the experimental or testing technique. It is perhaps true that too few published experiments have been checked in order to show the reliability of the data, as a necessary step for determining whether or not the technique used was satisfactory or whether a much larger number of *Ss* should have been employed. Since the discussion has gained headway, Tryon[9] has shown that a difference which is empirically demonstrated between groups of *Ss* becomes all the more significant in proportion to the unreliability of the instrument or technique of measurement. Crum[10] and Holzinger[11] believe that they have demonstrated that adding more items to a test does not increase reliability to the degree which the Brown-Spearman prophecy formula would lead one to expect. And Lanier[12] has proved that the mere augmenting of the number of *Ss* engaged in an investigation by no means guarantees greater reliability, for, if the apparatus or technique is at fault, leading to variable results, multiplying these variations or errors by an increased number of *Ss* will in nowise raise the reliability of the experiment.

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[1] H. R. Crosland, An investigation of proofreaders' illusions, *Univ. of Oregon Publ.*, 2, 1924, no. 6, 1-168.

[2] W. T. Heron and W. S. Hunter, Studies of the reliability of the problem box and the maze with human and animal subjects, *Comp. Psychol. Monog.*, 1, 1922, 1-56.

[3] W. S. Hunter and V. Randolph, Further studies on the reliability of the maze with rats and humans, *J. Comp. Psychol.*, 4, 1924, 431-442.

[4] F. C. Davis and E. C. Tolman, A note on the correlation of two mazes, *ibid.*, 4, 1924, 125-136.

[5] H. A. Carr, The reliability of the maze experiment, *ibid.*, 6, 1926, 85-94.

[6] W. S. Hunter, A reply to Professor Carr on the 'reliability of the maze experiment,' *ibid.*, 6, 1926, 393-398.

[7] Carl Murchison, An answer to Dr. Boring, this Journal, 37, 1926, 459-461.

[8] E. G. Boring, Scientific induction and statistics, *ibid.*, 303-307.

(*2*) (*a*). On pages 60-64 of our monograph we stated that a Pearson product-moment r was calculated for 5 samples, each considered by itself, of proofreading for each of 30 *Ss*, making in all 150 samples or items as a basis for the computation. Inasmuch as only 30 *Ss* instead of 150 took part in the experiment, it was bad statistical method to employ 5 samples from each person. The usual procedure would require that the mean of the 5 for each, 30 cases in all, be employed—a number of cases too small to suit the above formula. Since the publication of the monograph, we have employed in the three instances of Series A, B, and C the Pearson rank difference formula, obtaining correlation coefficients between mean accuracy and speed records as follows: Series A -0.55 ±0.09; Series B -0.43 ± 0.11; and Series C -0.42 ±0.11. It will be seen at a glance that these values do not vary greatly from those published in the monograph: -0.471 ±0.048; -0.457 ±0.046; and -0.478 ±0.045, respectively (pp. 61-63).

On pages 77-88 of our monograph the inaccuracy of reading proof was shown to increase progressively from the left to the right of the galley sheet, culminating in greatest error in the v-4 quarter column. The differences in the mean inaccuracy scores between v-1 and v-4, and v-3 and v-4, in all four series (A, B, C, and D) pooled together, for 26 *Ss*, have now been ascertained; these differences have been submitted to the D/σ diff. formula for the probability of the differences exceeding zero in many subsequent testings, were these repeated 99 times. The difference between the means for v-4 and v-1, throughout the series, is as significant as is shown by 98 chances in 100. That between the means for v-4 and v-3 is shown to be as significant as the probability of exceeding zero in 90 chances in 100.

Likewise, the difference in mean inaccuracy scores in Series A, B, and C, for the upper and lower halves of the proofsheets, 27 subjects, is found to be as significant as is indicated by 93 chances in 100. We have shown, furthermore (on pages 64-72 in the monograph), that the kind of error in the proofsheet has an influence on the inaccuracy with which the proof is read and corrected. Superfluous line spacing (SLS) and out-see-copy errors (OSC) were the kinds of mistakes which led to the most inaccurate proofreading. Omitted inter-word spacing (OS), superfluous spacing (SS), and superfluous capitals (SC) were the errors least frequently left uncorrected by the proofreaders. That the difference between the most frequent and least frequent kinds of errors is significant, we have proven by the standard statistical procedure for that purpose; the difference between the scores for superfluous line spacing errors and superfluous capital errors is found to be as significant as to be expected to exceed zero in 100 cases out of 100 times. The smallest difference is that which exists between scores of misspellings and omitted paragraph spaces. This difference is found to be as significant as is indicated by 58 chances in 100, where in the formula we have included r, a correlation of +.43, between the two sets of scores.

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[9] R. Tryon, Effect of the unreliability of measurement on the difference between groups, *J. Comp. Psychol.*, 6, 1926, 449-453.

[10] W. L. Crum, *Amer. Math. Mo.*, 30, 1923, 296-301.

[11] K. J. Holzinger, Note on the use of Spearman's prophecy formula for reliability, *J. Educ. Psychol.*, 14, 1923, 302-305; cf. also K. J. Holzinger and Blythe Clayton, Further experiments on the applicability of Spearman's prophecy formula, *ibid.*, 16, 1925, 289-300.

[12] L. H. Lanier, *J. Exper. Psychol.*, 10, 1927, 69-113.

(*b*) The reliability of the results which we obtained from our proofreaders, 30 in one series, 27 in two series, and 26 in the remaining series, we have now measured in three ways: (1) by comparing the scores on one sheet with the scores on another sheet in the same series to see if a high or low coefficient of correlation is obtained; in other words, to ascertain whether high or low variability was present among the *S*s in the reading of sheets which were greatly alike and read under greatly similar conditions of purpose; (2) we have computed the reliability of the results between series, that is, by calculating the degree of correlation between the accuracy of reading a sheet in one series and the accuracy of reading proof on some sheet in another series; and (3) we have computed the interseries correlation between the mean for five sheets of a series with the mean of five sheets of each of the other series, to ascertain whether or not a score earned in one series can prophesy the probable score in any other series. Such reliability calculations as we have mentioned above have been applied to speed scores as well as to inaccuracy scores. The correlation coefficients which were obtained under these three conditions are as follows.

(1) Intra-series correlation coefficients by individual sheets:[12]

INACCURACY SCORES

By percentages (30, 27, 27, and 26 *S*s)

Series A, sheets 1, 2 = +0.919±0.02
Series B, sheets 1, 2 = +0.920±0.02
Series C, sheets 1, 2 = +0.929±0.02
Series D, sheets 1, 2 = +0.606±0.08

By absolute (Series D) scores (26*S*s)

Sheets 1, 2 = +0.679±0.08
Sheets 1, 3 = +0.515±0.10
Sheets 1, 4 = +0.486±0.10
Sheets 1, 5 = +0.440±0.11
Sheets 2, 3 = +0.711±0.07
Sheets 2, 4 = +0.750±0.06
Sheets 2, 5 = +0.730±0.06
Sheets 3, 4 = +0.617±0.08
Sheets 3, 5 = +0.720±0.06
Sheets 4, 5 = +0.901±0.03

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[12] In every case, r, was determined by Huffaker's method (*J. Educ. Psychol.*, 16, 1925, 265).

in which $r = \dfrac{\sigma_1 + \sigma_2 + (M_1 - M_2)^2 \dfrac{-\Sigma(X_1 - X_2)^2}{N}}{2\sigma_1 . \sigma_2}$, when σ_1 and σ_2 are standard deviations of the two sets of scores; M_1 and M_2 are the means of the two sets of scores; x_1 and x_2 are the differences between the two sets, item by item in each set; and N is the number of cases (*S*s).

SPEED SCORES (divided by 100)
By absolute time records in sec. (30, 27, and 27 *S*s)
Series A, sheets 1, 2 = $+0.947 \pm 0.01$
Series B, sheets 1, 2 = $+0.888 \pm 0.03$
Series C, sheets 1, 2 = $+0.950 \pm 0.01$

(2) Cross-series correlation coefficients by individual sheets.

INACCURACY SCORES
By percentages (27 *S*s)
Series A-1, B-1 = $+0.860 \pm 0.04$
Series A-1, C-1 = $+0.610 \pm 0.08$
Series B-1, C-1 = $+0.910 \pm 0.02$

SPEED SCORES (divided by 100)
By absolute time records in sec. (27 *S*s)
Series A-1, B-1 = -0.250 ± 0.12
Series A-1, C-1 = $+0.210 \pm 0.12$
Series B-1, C-1 = $+0.680 \pm 0.07$

(3) Cross-series correlation coefficients by means of series.

INACCURACY SCORES
By absolute scores (27 *S*s)
Series A, B = $+0.863 \pm 0.03$
Series A, C = $+0.939 \pm 0.02$
Series A, D = $+0.853 \pm 0.04$
Series B, C = $+0.904 \pm 0.03$
Series B, D = $+0.882 \pm 0.04$
Series C, D = $+0.881 \pm 0.03$

SPEED SCORES (divided by 100)
By absolute time records in sec. (27 *S*s)
Series A, B = -0.54 ± 0.09
Series A, C = $+0.17 \pm 0.13$
Series B, C = $+0.69 \pm 0.07$

It is to be observed from the coefficients of correlation listed above that all values save three (those for A-1, B-1 and A-1, C-1, where speed r was -0.25 and $+0.21$ respectively, and for the A, C means, where speed r = $+0.17$) exceed the minimum of 0.40 which Kelley has set as a reliable basis for the study of group differences. Except for these three values, all coefficients exceed Kelley's minimum by large amounts. Had they been much smaller, such a fact would not have militated against the reliability of the experiment, as the original purpose proposed to discover real differences between the various series because of the divergent aims with which they were read and the differences existing between Series D and all other sheets. Moreover, had the above intra- or cross-series rs been much smaller, their smallness could then have been the basis for concluding that, inasmuch as the proofreading scores varied so widely in accuracy from sheet to sheet and from series to series, any adequate test of the abilities requires more than one sheet from each series and requires the entire four if not more series. The high rs shown above indicate, however, that no matter under what conditions reading is done, practically the same ability or abilities are being tested in the experiment. Perhaps this sweeping statement should be restricted to exclude sheets such as those which composed Series D, in which the greatest variations from sheet to sheet occurred. It will be noted above, moreover, that nine of the coefficients equal or exceed the minimum r of 0.90 which Kelley has proposed for an investigation of individual differences, while it must be remembered the minimum population for him is 30 *Ss*. It is true that in Series A we had employed the 30 necessary *Ss*, but in B, C, and D, only 27, 27, and 26 *Ss*, respectively, were used. Such individual differences as we have pointed out in the monograph (pp. 128-138) have been based upon still fewer cases, as for example the *Ss* composing the journalism, typography, psychology, and two student groups, comprising 5, 5, 5, 6, and 9 *Ss* respectively, with trained and untrained readers included among these *Ss*. The use of such small numbers of cases as the populations from which to draw conclusions concerning individual differences is obviously an insufficient basis for sweeping generalizations: this paper is written partly to limit our conclusions to the actual cases in hand. Nevertheless, practically all of the above coefficients of correlation indicate that the measuring device or technique employed was surprisingly accurate and reliable; such high reliability would seem to promise that future testing can with profit proceed along the lines which we have described in the monograph, and can have the added advantage of curtailing the 20 sheets to one or at most two test sheets, inasmuch as practically the same results can be prophesied because of the high r's exhibited in the foregoing table. To obtain results which have general applicability with respect to individual groups of *Ss* would require a great increase in the number of readers in each group or class.

Even the coefficients of correlation between the time scores, save in three instances, exceed the minimum of 0.40, a most surprising result, because one would imagine that the speed of reading proof would be far more variable than the accuracy of reading. That a negative relationship should occur between Series A and B was not unexpected; some indication of this fact was presented on pp. 34-42 and on p. 129 of the monograph. But that this r should be as large as −0.54 was a surprise. At several places in the monograph it was also stated that the reading of Series C resembled, in speed, Series B more than it did Series A; this fact has been confirmed by the above table of coefficients. The large value of r between B and C was unexpected.

If we arrange the above coefficients of correlation between mean accuracy scores for the series in descending order of size, we obtain the following sequence: A-C, B-C, C-D, A-B, A-D, and B-D. In this sequence it is obvious that, except for the A-B comparison, Series D is present in the comparisons which gave the lowest r's, that is, D correlated less highly with each of the other series than they correlated among themselves. The low correlation of D is probably to be explained by referring to four characteristics of the sheets of D which were absent from the other series. First, each sheet of D contained far fewer errors than the sheets of any other series, and it has been amply proven that the sheet containing the fewer errors is relatively more difficult to proofread accurately. Second, only two sheets of D contained the same number of errors, and the number of errors increased to the middle of the series; then followed a sheet of fewer errors and then one of a greater number of errors; in the three other series all sheets had approximately an equal number of objectively placed errors. Third, in D the reader was permitted to read and to correct proof in his own natural fashion, while in A, B, and C he was restrained by predetermined instructions, for accuracy, for speed, and for understanding, respectively. Fourth, the reading period in D was set at exactly 5 min., which fact led to unfinished and reread sheets, while no rereading and unlimited time were allowed in each of the other series. If one inspects the intra-series coefficients for Series D, above, it will be seen that high coefficients obtained when the compared sheets were greatly similar in number of objective errors, provided the sheets were closely adjacent or consecutive in order of presentation; see for example, the coefficients the *Ss* a function of expectation which, if gratified, raised the values of the correlations but, if ungratified, lowered the values, presumably because such unfulfilled expectation brings out great variations among *Ss* in the efficiency of reading, some *Ss* being more affected than others. It also happens that the correlations between D-2 and D-4 and between D-4 and D-5 are the highest in Series D. Since the others are low, these high, and since D shows low correlation values with each of the other series, it appears to follow that the sheets of D are more adequate for the measurement of all aspects of proofreading ability than are the sheets of the other series. Therefore, if one cared to curtail our technique and to repeat the experiment with a saving of time and energy, sheet D-4 could conceivably serve the purpose. In fact, in a supplementary investigation, which is to be published elsewhere at an early date, we have employed D-4 as a means of testing all-around proofreading ability, along with measurements of the range of visual perceptual apprehension. Where we have defined proofreading ability as being the product of reading time and reading accuracy, we have obtained a +0.84 correlation with range of apprehension scores, this value being higher than many of the values which are used in predicting college success from intelligence or high school record scores.

(*c*) In view of Murchison's criticisms of Boring and in view of Kelley's precepts for the study of individual differences, certain conclusions appearing on pp. 128-138 of the monograph should be restricted to the *Ss* who served in the investigation. These conclusions are as follows: (1) Several readers untrained in proofreading were more accurate than the trained *Ss*. (2) Speedy reading was, for the most part, characteristic of the untrained persons. (3) Slow reading did not proportionately increase the accuracy of the trained readers. (4) Individual differences existed in our *Ss* with respect to susceptibility to the number of errors objectively present in the sheets. (5) Slight individual differences in susceptibility to various kinds of errors were indicated. (6) The trained and untrained *Ss* differed as regards the effects of different purposes in reading on both time and accuracy scores. (7) Several statements were made concerning differences between groups of readers, for example, between journalists and psychologists, between both of these and students, between job-shop typographers and news typographers, etc. At the time of the preparation of the monograph it was realized that too few cases were included in each group so that sweeping generalizations were out of order. All conclusions other than those listed above were based upon data characteristic of true group differences—the group having not fewer than 26 *Ss* at any one time; and thus are in accord with the exacting standards of Kelley, Hunter, and others.

(*d*) If the results of the investigation of proofreaders' illusions possess the high reliability which is indicated by this paper, meeting so well the standards of statisticians by revealing only very small variations in accuracy scores from sheet to sheet and from series to series, so that one can feel that real proofreading ability has been measured, any program of vocational testing in the fields of journalism and typography cannot well ignore and may adopt with advantage the technique which we have worked out and have described at great length. Much of our labor can be avoided, it appears, by the curtailing of the number of sheets employed and by using only one general condition of purpose for the reading that is required of each person tested. Moreover, if it should turn out,[14] as has been tentatively suggested by both Downey[15] and Sterzinger,[16] that a high correlation obtains between proofreading ability and general intelligence, a foreshortened set of proofsheets, much like our own, conceivably could be introduced with advantage into an intelligence measurement program.

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[14] We are planning an investigation of this question which we hope will be settled unequivocally.

[15] J. E. Downey, The proof-reader's illusion and general intelligence, *J. Phil., Psychol., & Sci. Method*, 15, 1918, 44-47.

[16] Othmar Sterzinger, Zur Prüfung und Untersuchung der abstrakten Aufmerksamkeit, *Zsch. f. angew. Psychol.*, 23, 1924, 121-161.

Summary. This paper may be taken as a full and honest confession that the author is no more than a novice at statistical procedure, a fact, however which does not prevent him from sympathising greatly with Boring in the belief that it is possible to test with statistical correctness large numbers of *S*s and obtain differences whose probability of being real differences is great, while such differences may be ridiculously untrue; with Lanier in the belief that the mere possession of a population large enough to be statistically acceptable is not necessarily a guarantee of results more reliable than those obtained from fewer cases; with Boring and Carr in the belief that 'reliability' is more a matter of statistical precept than it is of the validity of data or of the effectiveness of technique; with Carr, perhaps, that not every investigation undertaken by statistical novices can be proven, *post hoc*, to have been so unreliable as not to warrant notice and acceptance; and with Tryon that differences arising from a clumsy or unreliable technique are, by reason of that very fact, all the more surprising and significant. When the author planned the investigation of proofreading he knew nothing of statistical precepts and methodology; now, due to the help of colleagues, he knows a little more, although he is still a novice. The computations above have been undertaken through curiosity as to what would be revealed if acceptable statistical methods were employed and through the desire to know if the investigation must necessarily be unacceptable merely because the investigator had proceeded without the employment of statistics '*à la* Hoyle.' The author has satisfied himself; it is hoped that the reader is equally satisfied.

University of Oregon H. R. CROSLAND

DR. WEVER ON ATTENTION AND CLEARNESS

In the January number of the JOURNAL Dr. Wever[1] reports a study in which he criticizes the concept of attributive clearness in general and my work on *Attributive vs. Cognitive Clearness*[2] in particular. Dr. Wever's study was seriously undertaken and it therefore deserves a serious reply. My one regret, however, is that Dr. Wever did not make as earnest an attempt to understand as he did to criticize.

Dr. Wever's difficulties are for the most part of his own making. Before criticizing one should understand, and this obviously Dr. Wever has failed to do. In fairness to Dr. Wever, however, it must be said that the Titchenerian doctrine of attention is difficult, and that it has been subject since its inception to much misunderstanding and misinterpretation[3]—so he is

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[1] E. G. Wever, Attention and clearness in the perception of figure and ground, this JOURNAL, 40, 1928, 51-74.

[2] K. M. Dallenbach, *op. cit., J. Exper. Psychol.*, 3, 1920, 183-230.

[3] I list here the criticisms that have been made of the Titchenerian doctrine and of the experimental work that was done under it. I give also the replies that have been made in rebuttal. The criticisms, as the replies show, rest upon misconceptions of the doctrine.

H. R. Marshall, Clearness, intensity, and attention, *J. Phil., Psychol., & Sci. Methods*, 6, 1909, 287-290.

M. W. Calkins, The abandonment of sensationalism in psychology, this JOURNAL, 20, 1909, 287.

R. S. Woodworth, Review of Titchener's 'Text-book of psychology,' *J. Phil., Psychol., & Sci. Methods*, 6, 190,692 ff.

Reply: E. B. Titchener, Attention as sensory clearness, *ibid.*, 7, 1910, 180-182.

F. Hillebrand, Review of Titchener's 'Lectures on the elementary psychology of feeling and attention,' *Zsch. f. Psychol.*, 58, 1910, 141-149.

C. Rahn, The relation of sensation to other categories in contemporary psychology, *Psychol. Monog.*, 16, 1913, no. 67, 29 ff.

(*Footnote 3 continued on page 338*)

in good company. The difficulties that many have with the Titchenerian doctrine are, I firmly believe, due in a large part to a terminological confusion occasioned by Titchener's unfortunate use of the term 'clearness.'[4]

Titchener found in his examination of the works of various writers on attention, "that, wherever you look you find some form of reference to clearness; clearness is, so to say the first thing that men lay hands on, when they begin to speak about attention."[5] Clearness, or some form of reference to clearness is the common denominator of the accounts of attention. It was this fact that led Titchener to the consideration of 'clearness' as an attribute of sensation, and to his doctrine of attention. He says, upon this point, "it seems to me beyond question that the problem of attention centers in the fact of sensible clearness."[6] Titchener found that 'clearness' fulfilled the qualifications of an attribute,[7] and, after some hesitation between the terms 'vividness' and 'clearness,'[8] he chose the latter to designate that aspect of experience by which the focal processes are distinguished from the marginal. 'Clearness' was an unfortunate choice, as Titchener himself later realized, for he changed the name of this aspect to 'vividness' in 1915,[9] and again to 'attensity' in 1924. The word 'attensity' was coined, Titchener says, "to replace what is ordinarily called sensory or attributive clearness or vividness, in the hope that . . . its freedom from associations may help to clear up the confusion of cognitive and attributive clearness."[10]

Had Titchener employed the term 'vividness' (or better yet the term 'attensity') when he formulated his theory we should, I believe, have been spared a deal of controversy and confusion, for 'clearness' has proved to be a very slippery term, particularly when applied to visual experience—the modality with which Dr. Wever was concerned. Four kinds of clearnesses may be involved in a visual experience: (1) attributive clearness, *i.e.* the

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Reply: Titchener, Sensation and system, this JOURNAL, 26, 1915, 258-267.

C. A. Britz, *Eine theoretische und experimentelle Untersuchung über den psychologischen Begriff der Klarheit*, 1913.

Reply: Titchener, The psychological concept of clearness, *Psychol. Rev.*, 24, 1917, 43-61; K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 183-230.

K. Koffka, Review of Dallenbach's 'The Measurement of Attention,' *Zsch. f. Psychol.*, 73, 1915, 281 f.

Reply: Dallenbach, Note of the experimental study of attention, this JOURNAL, 31, 1920, 215.

J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 1916, no. 35, 1 ff.

Reply: Dallenbach, Dr. Morgan on the measurement of attention, this JOURNAL, 29, 1918, 122 f.

H. Henning, Review of Dallenbach's The measurement of attention in the field of cutaneous sensation,' *Zsch. f. Psychol.*, 82, 1919, 245.

Reply: Dallenbach, *op. cit.*, this JOURNAL, 29, 1918, 122 f.

H. M. Johnson, Reaction-time measurements, *Psychol. Bull.*, 20, 1923, 577-580.

Reply: Dallenbach, Dr. Johnson on reaction-time experiments, this JOURNAL, 35, 1924, 305-307.

H. S. Oberly, The range for visual attention, cognition, and apprehension, this JOURNAL, 35, 1924, 332-352.

Reply: Dallenbach, Dr. Oberly on 'The range for visual attention, cognition and apprehension,' *ibid.*, 36, 1925, 154-156.

H. M. Johnson, Definition and measurement of attention, *ibid.*, 36, 1925, 601-615.

Reply: Dallenbach, Dr. Johnson on the measurement of attention, *ibid.*, 37, 1926, 149-154.

S. W. Fernberger, The 'range of attention' experiment, *ibid.*, 38, 1927, 478-479.

Reply: Dallenbach, Dr. Fernberger, on the 'range of attention' experiment, *ibid.*, 38, 1927, 479-481.

[4] E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 171-317.

[5] *Ibid.*, 186.

[6] *Ibid.*, 182.

[7] *Ibid.*, 211-220; cf. also The psychological concept of clearness, *Psychol. Rev.*, 24, 1917, 53 f.

[8] *Ibid.*, *Psychol. Rev.*, 24, 1917, 55.

[9] E. B. Titchener, *A Beginner's Psychology*, 66, 92 ff.

[10] E. B. Titchener, The term 'attensity,' this JOURNAL, 35, 1924, 156.

visual experiences may occupy the focus or the margin of attention; (2) cognitive clearness,[11] *i.e.* the visual experiences may or may not be cognized; (3) physiological clearness, *i.e.* the visual experiences, due to the difference in sensitivity of the retina, may be focal and distinct, or peripheral and at varying degrees of indistinctness; (4) physical clearness, *i.e.* the visual image, due to spherical aberration of the crystalline lens, may be sharp and definite, or diffuse and vague.

These 'clearnesses' may vary concomitantly or—except for physical and physiological 'clearness'—independently. I do not know for certain, but I think that physical and physiological clearness always vary concomitantly, *i.e.* they are tied by the structure of the lens and retina. However that may be, it is certain that attributive and cognitive clearness may vary independently of them and of each other. That attributive and cognitive clearness may vary independently of physical and physiological clearness is attested by the fact that an impression in the periphery of vision may be cognized and attended to. That they may vary independently of each other was shown by the results of my study in which it was discovered that "an impression may be attributively clear and cognitively clear; attributively clear and cognitively unclear; attributively unclear and cognitively clear; attributively unclear and cognitively unclear."[12] These findings have recently been confirmed by Cooper who found that "with attributive clearness high or low may be combined either high or low cognitive clearness."[13]

All of these 'clearnesses,' with the possible exception of attributive, are easy to comprehend. Attributive clearness is more difficult than the others simply because it is an ultimate. Titchener, in his reply to Britz's criticism, writes that very little can be said about the actual nature of attributive clearness: "You cannot say much about a thing you regard as ultimate to your science; any attempt at a definition runs over, by force of circumstances, into what Wundt would call *tautologische Umschreibung*. I have had recourse to a number of these periphrases; but I have tried above all, to exhibit the thing itself, to state conditions under which it may be experienced and identified in experience. Quality and intensity are here in the same box with clearness. You can exhibit qualities or intensities, as you can exhibit clearnesses; but when you attempt to define them, you find yourself talking round them. If Britz had performed Geissler's simple experiment[14] with the two metronomes, equated for quality and intensity of sound, he would have discovered at first hand what I mean by sensory or attributive clearness."[15]

Instead of performing the experiments that exhibit attributive clearness, the critics have jumped at conclusions. They have thought that they understood but they have, without exception, mistaken one or more of these other kinds of 'clearnesses' (most frequently cognitive clearness) for the phenomenon in question. Now my point is simply this: had the term 'attensity'—or any other term besides 'clearness'—been used by Titchener, confusion would have been less likely. Not that it would have made the critics sagacious, but that it would have had a tendency to keep them from thinking that they understood what was meant when, as their criticisms show, they did not. That alone would have been a gain.

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[11] For a differentiation of attributive and cognitive clearness see Titchener, *Feeling and Attention*, 238 f.; *Experimental Psychology of the Thought Processes*, 1909, 17; S. S. Colvin, *The Learning Process*, 1911, 255 f.; and Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 199-225.

[12] K. M. Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 229.

[13] S. F. Cooper, The effect of brightness in the range of attention experiment, this JOURNAL, 40, 1928, 268.

[14] L. R. Geissler, The measurement of attention, *ibid.*, 20, 1909, 510; cf. also K. M. Dallenbach, The measurement of attention, *ibid.*, 24, 1913, 467.

[15] E. B. Titchener, The psychological concept of clearness, *Psychol. Rev.*, 24, 1917, 55 f.

In the visual experiences of every day life all four of the 'clearnesses' may exist together, and even a practiced observer may find it difficult to differentiate between them. Vision therefore is a particularly troublesome sense department in which to work. The fields of audition or of cutaneous sensitivity are much simpler. But if one must work in vision one should at least make the introspective task as simple as possible by restricting the sensory field—as Britz and I did with the telescope[16]—so that visual experience is limited to the foveal regions of the retina, and physical and physiological clearnesses are eliminated. In any case, whether one works with the restricted visual field, with audition, or with touch, the investigation of clearness—since one is dealing with an ultimate—should be held to the sensory level. Dr. Wever agrees in theory that "the problem should be reduced to its simplest terms,"[17] but he works in vision—the most complex of the sense departments; he does nothing to restrict his visual field—all four 'clearnesses' are effective in his experiments; and he presents his observers a perceptual task! It is small wonder therefore that he obtained nothing but an undifferentiated clearness. Dr. Wever was ill advised; he 'does not know his clearnesses.'

So much by way of general reply; I now turn to the particulars of Dr. Wever's criticism. To the charge of misunderstanding must now be added the charges of misreading, misinterpreting, and misquoting. I give instances.

(1) Dr. Wever says, "Titchener's distinction of sensory and cognitive clearness was given experimental confirmation in a study by Dallenbach, a study *expressly devised* to *substantiate* the concept of sensory clearness in the face of a *severe* attack by Britz."[18] Dr. Wever should know that experiments are undertaken to test hypotheses not to "substantiate" them. But in any case my experiments had a different purpose. They were undertaken to test Britz's results—as even a superficial reading of my study should have shown him, for I say in the introduction, "the present paper reports the results of a series of experiments carried out so far as possible under the conditions laid down by Britz."[19] I can assure Dr. Wever that the results of my study would have been reported whatever their outcome. The experiments, moreover, were of Britz's 'devising' not mine, as Dr. Wever should have learned from my paper even though he had not read Britz. "Severe," furthermore, is hardly the word that one, who has critically read Britz or Titchener's reply, would use to characterize the "attack." But as incredible as it may seem, we have no evidence that Dr. Wever has read either.[20]

(2) The reader of Dr. Wever's paper who has not read mine would hardly guess that his principal criticisms[21] against my experiments were practically quotations from my paper. I devoted a section to the discussions of the "complexity of the problem;"[22] I pointed out "that neither the method nor the instruction was conducive to good psychological description. The observers were required to note meaning and process and to give a full report of both. An extremely difficult instruction!"[23] I well recog-

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[16] K. M. Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 186.

[17] E. G. Wever, *op. cit.*, 54.

[18] *Op. cit.*, 53. Italics mine.

[19] *Op. cit.*, 184.

[20] Dr. Wever gives the page and sectional references to every article that he cites in footnote with the single exception of Britz's study—and Britz devotes only a part of his paper to the Titchenerian concept. The first part of the paper is devoted to Wundt and the last part to the report of the experiments. Dr. Wever, furthermore, makes no reference to Titchener's reply to Britz. Dr. Wever should have known of it for it is the first reference that I give in my study.

[21] *Op. cit.*, 53, 68 ff. [22] *Op. cit.*, 196-198. [23] *Op. cit.*, 196.

nized the defects of the method but I had no choice in the matter. I was repeating Britz and any change in the method or procedure would have invalidated the comparison of my results with his. With all its faults, however, Britz's method and procedure were superior to Dr. Wever's. Britz at least eliminated physical and physiological clearness from his experiments, and he thought to work upon the sensory level. Indeed, if Dr. Wever had attentively read Britz he would have noted his strictures against working upon this problem at the perceptual level.

(3) Dr. Wever's quotations from my paper are, furthermore, misleading to a reader who does not have my report clearly in mind. He says, for example, that one of my observers "maintained that 'the report is so complicated that it seems foolish to talk about attributive clearness.' "[24] What I said was, "W says in one of the early reports: 'The report is so complicated that it seems foolish to talk about attributive clearness.' "[25] The implications of Dr. Wever's quotation is very different from my statement. The qualification "in one of the early reports" is not without significance.

(4) I am said by Dr. Wever to rely in my discussion upon two premises: "the first is that the *O* is able to report upon the clearness of a given part-content accordingly as it stands out to his grasp in the total experimental content; and the second is the clearness of conscious processes determines the fullness of report upon these processes."[26] I nowhere formulate these premises; they are of Dr. Wever's own making. I relied entirely upon the reports of my observers. I am willing, however, if I understand them correctly, to accept the first—I believe that an observer can tell which processes he is attending to and which he is attending from; and I accept the second if Dr. Wever's "clearness" means cognitive clearness, but I must emphatically deny it if Dr. Wever's "clearness" means attributive clearness for the results of my study and Cooper's[27] (made independently and in a different laboratory so 'atmosphere' had nothing to do with his results) stand in direct opposition to it. My results show (1) that attributive and cognitive clearness each has its own conditions, and (2) that attributive clearness is one of the conditions of cognitive clearness, but it is only one of 12 conditions that obtained in my experiments.[28] It is not a compulsory condition, it operates for cognitive clearness in variation with the other 11 as, for example, the factors of chance disposition act for or against the perception of duality in the psychophysical experiment on the two-point limen. Impressions are cognized when the conditions are 'adequate,' and the degree of cognition varies with the 'grade of chance disposition.'

(5) Dr. Wever shows from my data that "there exists a high correlation between 'levels of attributive clearness' and 'degrees of cognition.' "[29] Since attributive clearness is one of the conditions of cognition we should expect that relation to exist. He will also find a high correlation between every one of the other 11 conditions of cognitive clearness and degrees of cognition if he will but take the trouble to look. The important point in this connection, however, is not that there is a high correlation between the two clearnesses, but that there is not an invariable correlation—which we should expect if there were but one kind of clearness. "But why," Dr. Wever asks, "should the fact that these two things do not give a one-to-one correlation be any proof that the observer has had within his consciousness two types of clearness?"[30] Well, the observers reported the two kinds of clearness within a given consciousness. When competent observers report that the impressions of a given moment are attributively clear and cognitively unclear, and repeatedly give such reports through the course of 240 experiments I am inclined to accept them.

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[24] *Op. cit.*, 68.

[25] *Op. cit.*, 198.

[26] *Op. cit.*, 69.

[27] *Op. cit.*

[28] *Op. cit.*, 213-215.

[29] *Op. cit.*, 70.

[30] *Op. cit.*, 69.

(6) Dr. Wever's method is as, I have already pointed out, inadequate to the problem. Still it is possible even in his method, as bad as it is, to differentiate in the excerpts that he gives of his observers introspections, the various kinds of clearnesses that may be integrated in the perception of a total visual field. I give examples: B (p. 66) "An unclear field is 'fuzzy.' An unclear field is on the fringe of the picture" [physical and physiological clearness]. Z (p. 67) "A thing is clear in proportion as it can be reported upon" [cognitive clearness]. Z (p. 67) "In the experimental field one part of the field will be distinct, and will seem to stand out more, is more *eindringlich*, like the focus as opposed to the margin" [attributive clearness, I cannot of course be certain because this excerpt appears out of its context].

(7) Since I regard Dr. Wever's method as inadequate and his "undifferentiated clearness" as a composite, it is perhaps unnecessary for me to say that I consider his discussion of the law of two-levels as irrelevant. It is, however, a very significant fact that the investigators who thought they had demonstrated the multi-level type were all working in vision, with an unrestricted field where physical and physiological clearness had an opportunity to be effective.

Cornell University KARL M. DALLENBACH

THE ACCURACY OF MÜLLER-URBAN'S METHOD OF CONSTANT STIMULI

In view of Dr. Lufkin's note,[1] let me try to clear what remained obscure in my reply[2] to Dr. Urban.[3] Being purely a *pièce d'occasion* and by no means a review of the whole problem, my first paper had but one aim: to demonstrate beyond reach of cavil that Urban's formula (probable error of the constant-stimulus limen) and his own figures are in error; theory of necessity therefore made way to factual proof. We may now enquire in more detail how c and h are related.

Consider the equations

$$c - hJ_1 + \gamma_1 = \rho_1, \text{ of weight } P_1;$$
$$c - hJ_2 + \gamma_2 = \rho_2, \text{ of weight } P_2;$$
$$\dots\dots\dots\dots\dots\dots\dots\dots$$
$$c - hJ_s + \gamma_s = \rho_s, \text{ of weight } P_s;$$

wherein h is the 'precision;' c equals ah (a being the limen); J_k is the stimulus judicandus to be compared with a standard S; ρ_k is a residual (amount by which left member of its equation departs from zero); P_k is relative weight of the observation-equation k in deriving the normal equations for c and h. The Müller-Urban technique, as we know, is a device for minimizing the sum of these residua when squared ($\rho^2_1 + \rho^2_2 + \dots + \rho^2_s$) and duly weighted; it thereby enables us to lay through the observed percentages an ideal curve, $\Phi(\gamma)$, whose course will deviate as little as possible from the empirical figures.

The question is: in what conditions and to what degree will these *minimizing* values of h and c correlate with each other? Suppose we make many determinations of the limen and thereby locate n points (c_1h_1, c_2h_2, ..., c_nh_n) where $\Sigma\rho^2$ is least; these n minima, instead of scattering uniformly, will cluster in some part of the c-h plane. The cluster reveals, by its form and orientation to the coördinate-axes, in what measure c and h are related. A correlation-surface is commonly made up by erecting, in each square of the double-entry field (x, y), a pillar whose height equals the frequency of that combination (x_k, y_k); let us build our model instead by summing $\Sigma\rho^2$ for all pairs lying in a given square of the c-h plane and then raising an ordinate equal to this total. At $h = 0.100$, $c = 11.0$ are, let us say, 15 of our n cases; instead of placing as usual a column of length 15, we sum their $\Sigma\rho^2$ ($\Sigma\rho^2_1 + \Sigma\rho^2_2 + \ldots + \Sigma\rho^2_{15}$) and embody this total in the ordinate. Our solid model thus incorporates, not n cases all of unit value, but n $\Sigma\rho^2$, unequal in magnitude but alike in being minimal each for its own series. Every ordinate now symbolizes an equation of the form[4] (after P is multiplied in)

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[1] H. M. Lufkin, Accuracy of the method of constant stimuli, this JOURNAL, 38, 1927, 666-7.

[2] Elmer Culler, Accuracy of the method of constant stimuli: a reply to Dr. Urban, this JOURNAL, 38, 1927, 307-12.

[3] F. M. Urban, Accuracy of the method of constant stimuli, *ibid.*, 38, 1927, 252-56.

$$[P\rho^2] = [PJ^2]h^2 - 2[PJ]hc + c^2 - 2[PJ\gamma]h + 2[P\gamma]c + [P\gamma^2].$$

By writing $[P\rho^2] = k$, we automatically pick those ordinates which are equal, as though by slicing the top off our model on a plane k units above the c-h base; the edge of this cut is a contour-line of our solid cluster. Being of second degree and of type

$$Ax^2 + 2Bxy + Cy^2 + 2Dx + 2Ey + F = 0,$$

this contour-equation stands for a conic section (ellipse, parabola, hyperbola, with their limiting and special forms).[5] Thomson says indeed that it "represents a family of ellipses."[6] This is not wholly true; the general equation always describes a conic; if we are to define it more precisely, other conditions need be known, as arrayed in the schedule below.

Condition[7]	Type of conic
I. $\Delta \neq 0$	
1. $B^2 > AC$	hyperbola
2. $B^2 = AC$	parabola
3. $B^2 < AC$	ellipse
a. $B = 0, A = C$	circle
II. $\Delta = 0$	two straight lines, real or imaginary

Until we know the value of Δ and of $(B^2 - AC)$, no more can be said about the contour of our model. In practice however it is almost sure to be elliptical (with a straight line as limiting form) or possibly hyperbolic. So long as the correlation-figure (ellipse) runs obliquely athwart the c- and h-axes, these variables are bound to correlate in some degree; only if major and minor axes parallel the coördinates will r_{ch} become nil. The model's obliquity, Θ (angle of major axis with x-coördinate) is measured by the relation

$$\tan 2\Theta = \frac{B}{A - 0}.$$

When $B = 0$, $\Theta = 0°$ or $90°$; in either case, $r = 0$; only if the term in xy vanish, will r itself disappear. Since B (coefficient of the xy-term) = [PJ], B depends directly on magnitude of J and site of the origin. The whole c-h model then, in form and orientation alike, may be completely altered by merely moving the point of reference; the size of r can be varied at will by simply changing J (or the limen a; that value of J where the proportion of comparative judgments—heavier, warmer—is 50% of the whole).

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[4] The square brackets are signs of summation.

[5] The propositions here stated are proved in analytical geometry; these books may be consulted:

A. C. Jones, *Introduction to Algebraical Geometry*, 1912, 226.
E. H. Askwith, *Analytical Geometry of the Conic Sections*, 1908, 120.
S. L. Loney, *Elements of Coördinate Geometry*, 1912, 330.
C. H. Ashton, *Analytic Geometry*, 1900, 166.

[6] The second term in his equation should be multiplied by two. Cf. G. H. Thomson, Accuracy of the ϕ (γ) process, *Brit. J. Psychol.*, 7, 1914, 54.

[7] The determinant, Δ, equals

$$\begin{vmatrix} A, & B, & D \\ B, & C, & E \\ D, & E, & F \end{vmatrix} = ACF + 2BDE - AE^2 - CD^2 - FB^2.$$

The following schema may do to show how 'tricky' is the h-c relation and how carefully it must be handled.

	h =	.10	.20	.30	.40	.50	
Origin at 0	a =	80	90	100	110	120	
	c =	8	18	30	44	60	$r_{ch} = .996$
Origin at 130	a =	−50	−40	−30	−20	−10	
	c =	−5	−8	−9	−8	−5	$r_{ch} = .000$

The limen, a, is taken once from zero, then from 130; this purely formal change reduces r from 0.996 to 0.000. The focus of trouble resides in the product c (=ah); any change in one of its factors disturbs the whole c-h correlation-surface. In correlating a product with one of its own factors (ah with h), we are dealing with a shifty relation.

We are now prepared to scan Dr. Lufkin's comment on my reply to Dr. Urban; two criticisms, if I read correctly, are implied in his note.

(1) By using ρ instead of r for measuring their concomitance, I exaggerated the amount of relation between h and c. In reply, I was concerned only with proving a high positive correlation of h and c in Urban's data; its exact magnitude for my purpose was wholly incidental. Ease and speed of computation were my only reasons for using ρ; neither then nor since have I computed r for those figures. We all know r to be a more precise measure than ρ; but with short series[8] no index of relation is highly reliable. Consider r and ρ in Dr. Lufkin's own figures;[9] the means run as follows:

	S_1		S_2	
	r	ρ	r	ρ
Lifted weights	.798	.732	.952	.875
Visual apprehension	.595	.750	.538	.700

In two cases r exceeds ρ, in two, ρ exceeds r; neither is uniformly higher than the other. The correlation of h and c in Urban's data is so high as to be unquestionably significant; this we believe no one will venture to deny; if then r be a few points higher or lower than ρ, what of it?

(2) If c be a direct function of h $[c = F(h)]$, then the method of least squares cannot be applied and the whole Φ (γ) procedure must be abandoned. In my former article, let me say, I sought to show that c and h are de facto dependent in Urban's data, as proved by their high covariance; *not* mathematically dependent, as though either were uniquely determined by the other. The equation, $c = F(h)$, whose implication Dr. Lufkin traces through, is far too simple; $c = F(h, \ldots, m, n, o, \ldots)$ is nearer the truth. The magnitude of both c and h is affected, in variable degree, by other determinants as well. In a word there is no consistent or inherent relation of c and h; their r may range from zero to unity; but even if the correlation be casual instead of necessary, it vitiates Urban's formula all the same. The phrase "functional dependence" perhaps gave a wrong impression of my view.[10] To Dr. Lufkin it may well have meant mathematically functional (any value of c immutably fixes the corresponding value of h). The term was here used instead to mean the kind of empirical dependence which is found in natural phenomena: and not the 'structural' or necessary relations of theory. This meaning of the word functional is surely implied in the qualification "virtually perfect;" such a phrase

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[8] n = 6 to 28; cf. Culler, *op. cit.*, 310.

[9] Lufkin, *op. cit.*, 666.

[10] Culler, *op. cit.*, 310.

would naturally characterize the variable correlations found in experience and not the fixed relations of theory. It would be curious to say, for example, that the dependence of a circle's area on its radius is "virtually perfect." In a mathematical universe, the dependence of function on argument is in general unique and complete; in a perceptual universe, no such dependencies exist. The phrase "virtually perfect" was then meant to imply that we are dealing with a natural correspondence which rises and falls from one group of data to another as do all experimental relations; but which, in Urban's data, proved to be almost unity.

Urban's major (though not sole) mistake lay in applying to quantities which may correlate in any amount from zero to unity a formula (for probable error of a quotient) which applies only when numerator and denominator are *not significantly correlated.* The reason is clear. Given $a = c/h$: when c and h rise and fall together, a will be well-nigh constant; when they vary independently (as theory for propagation of error assumes), a will fluctuate far more. The higher this covariance, the larger the error thereby introduced; because of the extremely high agreement of c and h in Urban's data, his p.e. work out from 10 to 30 times their true value. When r is small the error is less, but will always remain in some degree so long as c and h correlate at all. When they vary inversely (correlate negatively), the Urban-values are too small.[11]

Far from believing the $\Phi(\gamma)$ procedure invalid, I have adopted Müller's weighting-device in deriving a set of tables (soon to be issued) applying to biological data the chemical equation for monomolecular autocatalysis.

University of Illinois. ELMER CULLER

A NEW SYSTEM FOR THE CLASSIFICATION OF ODORS

E. C. Crocker and L. F. Henderson of Arthur D. Little, Inc., of Cambridge, Mass., have recently published some very interesting results of research on the analysis and classification of odors.[1] This problem has persistently continued to fall short of a final, acceptable solution. Henning's prism may be thought of as reducing Zwaardemaker's nine classes to six as well as making a statement of the relationships between the six. Experimental attempts to verify Henning seem always to result in but partial verification: the prism is neither wholly right nor wholly wrong.[2] The psychologist may welcome, therefore, a further simplification, if not in the belief that any simple statement can give the complete account of olfactory quality, at least in the hope that the eventual establishment of some gross fundamental classification may furnish a safe ground for further experimental differentiation.

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[11] The former note, written under great pressure for time, contains a few places which might have been phrased more suitably or clearly. Two typographical errors may be listed: change 'without' (p. 308, last line) to 'within' and 'thought' (p. 311, c, line 7) to 'though'.

In this connexion let me add that Dr. S. M. Newhall kindly informed me of an error in my Studies in psychometric theory (xi), *J. Exper. Psychol.*, 9, 1926, 180, Table IV: after P replace 2.30 by 2.32, 5.98 by 5.59, .800 by .758. These changes, happily, reënforce my original conclusions.

Let me signalize again Professor Thomson's article, above cited, on this problem; it will repay—and demand—careful study.

[1] E. C. Crocker and L. F. Henderson, Analysis and classification of odors, *Amer. Perfumer and Essential Oil Rev.*, 22, 1927, 325 ff. The relative unavailability of this publication to academic psychologists is my excuse for the first part of this note, but I can not of course reprint most of the data nor summarize the paper completely. Mr. Crocker has furnished me with a number of reprints, and I shall be glad to send these to interested psychologists as long as the supply lasts.

[2] Cf. F. L. Dimmick, this JOURNAL, 33, 1922, 423-425; M. K. Macdonald, *ibid.*, 33, 1922, 535-553; A. E. Findley, *ibid.*, 35, 1924, 436-445; Dimmick, *Psychol. Rev.*, 34, 1927, 321-335.

Crocker and Henderson are chemists engaged in research in connection with the manufacture of perfumery. They have both had wide experience with smells, and each had worked at the classification of odors before their collaboration. As a result of a great deal of systematic observational work they have concluded that for purposes of classification there are four principal qualities of odor and that any particular odor may be placed with respect to others by a statement of the numerical degree by which each of these 'components' enters into it. The four basal qualities are:

(*1*) *Fragrant*, which corresponds to Zwaardemaker's and Henning's Fragrant.

(*2*) *Acid* (or sour), a new class. The authors reject Henning's Fruity (and also Zwaardemaker's Ethereal) on the ground that a quality thus described depends in part upon the tactual effect of vapor in the nostrils. "Acid" is thought by them to be the true olfactory element that remains when the effects of vapor are abstracted from. They also warn against the confusion with smell of the experience of "pungency," which they find frequently associated with acid smells and which they disregard as sensory pain. That they have not fallen into the "stimulus error" is apparent when they note that it is not acids alone that have the acid smell.

(*3*) *Burnt*, which is Henning's Burnt and Zwaardemaker's Empyreumatic.

(*4*) *Caprylic* (or oenanthic), which is Zwaardemaker's Hircine.[3]

Crocker and Henderson have established by practice an absolute scale of judgment for each of these four 'components.' The scale runs from 0 to 8, that is to say, it is a scale of nine points. Each odor is characterised numerically with respect to each of the four *Hauptgerüche*, which they write conventionally in the order FABC. Thus, when they say that cineol is 5726, they mean that cineol is placed at 5 in the scale for Fragrant (F), at 7 for Acid (A), at 2 for Burnt (B), and at 6 for Caprylic (C).

In this way they have worked out the class numbers of a great many smells. In their Table I they give 'standards' for each principle class, *i.e.* for eight substances representing the degrees of F from 1 to 8, combined with varying other degrees of A, B, and C; and similarly for the other three principal 'components.'[4] Classifications of other odors are made by re-

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[3] One can not safely speak of relationships between the classes used by different investigators because of similarity or difference of name merely; the ultimate reference must be to actual stimuli. Nevertheless the experimental indications of the following relationships are pretty clear.

Zwaardemaker	*Henning*	*Crocker and Henderson*
1. Fragrant	1. Fragrant	1. Fragrant
2. Ethereal	2. Ethereal	
		2. Acid
3. Aromatic	3. Spicy	
	4. Resinous	
4. Ambrosiac		
5. Alliaceous		
6. Empyreumatic	5. Burnt	3. Burnt
7. Hircine		4. Caprylic
8. Foul	6. Putrid	
9. Nauseous		

[4] Since all this discussion has scientific meaning only in relation to the stimuli, I reprint here the table of standards, omitting only a few alternative standards. I regret that I do not have space for reprinting Crocker and Henderson's Table II.

FRAGRANT		ACID	
1517	Cyclohexanone	6123	Beta-naphthyl ethyl ether
2424	Toluene	4221	Cedrene
3336	Alpha-chlornaphthalene	5336	Resorcinol dimethyl ether
4344	Alpha-naphthyl methyl ether	2424	Toluene
5222	Phenylethyl benzoate	5523	Isobutyl phenylacetate
6434	Diphenyl ether	5636	Methyl phenylacetate
7343	Safrol	5726	Cineol
8445	Benzyl acetate	3803	Acetic acid (20%)

(*Footnote 4 continued on page 344*)

ferring to these standards. In Table II the authors give the formulae for 75 chemicals arranged according to chemical nature. In Table III they give ranges for certain types of odors; for example, a typical perfume is F 6-8, A 2-6, B 2-4, and C 2-4; greasy odors are F 3-5, A 6-7, B 5-7, C 7-8; and so on for nineteen other types.

They also give some curves for the variation of F, A, B, and C in the alcohols and aldehydes where the number of carbon atoms changes, but they realize that they are as far from a chemical theory of smell as other investigators. They even suggest that the ultimate theory may turn out to be physical rather than chemical, depending upon surface tensions, etc.

The value of this work as a ground for psychological understanding of smells and further research upon them seems to me to depend upon its reliability. The authors have no elaborate theory; nevertheless, if it were possible for anyone, with sufficient practice in the use of these scales and in abstraction from non-olfactory components, to identify without error an odor by a four-digit number, then we should be in the way of defining stimuli for research, and we should have a system in terms of which research on mixture, reciprocal adaptation, and other problems could be intelligently undertaken.

Reliability. Crocker and Henderson gave no data upon reliability, but at my suggestion they have set to work and have supplied me with data showing the judgments of 25 stimuli made independently by Crocker (*C*), by Henderson (*H*), and by each of two untrained observers (*A* and *B*). Each *O* was given these 25 stimuli and, for comparison, the 32 standards. The untrained *O*s worked through the samples one at a time, without rechecking. *C* and *H* arranged the samples in order for each 'component' separately, comparing constantly with the standards, and then comparing similar numbers together and with the standards. It required from 90 to 120 min. for each *O* to complete this task.

The problem thus becomes one of correlations among *O*s, but in this case coefficients of correlations are less interesting than the average deviation of one *O* from another. Accordingly I have determined for each of the four principal olfactory factors and for each pair of *O*s the differences. Since there are four *O*s, there are six pairs of *O*s; 6 pairs × 4 olfactory judgments × 25 stimuli = 600 differences. The following summary shows the frequency of large and small deviations of one *O* from another. The most usual deviation was 1; there was agreement oftener than a deviation of 2; large deviations were rare.

Deviation	6	5	4	3	2	1	0	Av. = 1.22
Frequency	3	5	11	42	130	256	150	Tot. = 600

These data are analysed further in the accompanying table, which shows average deviations for each pair of *O*s and for each class of judgment.

The table indicates in the first place that practice had less effect than would have been expected. *C-H* is the pair of experts (the authors), and experts might have been expected to deviate from each other less than untrained *O*s, and less than an expert deviates from a novice. It is true that average deviation of *C-H* (1.03) is the least of all, but the low value is largely determined by the small deviation for Caprylic Judgments

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Burnt	
5414	Normal propyl alcohol
6322	Phenyl propyl alcohol
5336	Resorcinol dimethyl ether
4344	Alpha-naphthyl methyl ether
5345	Veratrol
6665	Thujone
4376	Para-cresyl acetate
7584	Guaiacol

Caprylic	
5221	Santolol
5322	Isoamyl benzoate
5523	Isobutyl phenylacetate
4314	Cyclo-hexane
4445	Tetralin
5636	Methyl phenylacetate
2577	Anisol
3518	Diisoamyl

of C were peculiarly difficult to make, and here practice may have had its effect. The *C-H* pair does not give the minimal deviation for A and B, and for F the deviation with *C-H* is no less than with *A-C*. The psychologist starting out to duplicate these results ought not, then, to expect that a long period of training would be necessary.

It is, of course, only the psychologist, long baffled by the olfactory problem, who is surprised by this conclusion. Crocker and Henderson tell me that their experience shows that a new operator can begin immediately with fairly accurate evaluations, once he understands the instructions, and that familiarity with the system makes the judgments easier but not, in general, more accurate. Accuracy is, however, increased for both new and practiced operators by abstraction from three components and attention to only one at a time.

	Observer-pairs						
	A-B	*C-H*	*A-C*	*A-H*	*B-C*	*B-H*	Av.
Fragrant	0.92	0.76	0.76	0.88	1.12	1.16	0.93
Acid	1.12	1.32	1.24	1.44	1.72	1.48	1.39
Burnt	1.12	1.20	1.04	1.28	1.28	1.12	1.18
Caprylic	1.36	0.84	1.24	1.28	1.76	1.80	1.38
Average	1.13	1.03	1.07	1.22	1.47	1.39	1.22

The averages for the four 'components' indicate that F is by far the most accurately judged. This result is not surprising, for there has always been most to say and least disagreement about the fragrant odors. The greatest difficulty was experienced with the completely novel class, A, and the relatively novel class, C. B is intermediate.

Crocker and Henderson point out that their system provides the possibility of differentiating "9×9×9×9=6561 detectably different odors." However, the psychologist knows that accurate discrimination of nine degrees under these conditions is likely to be difficult to achieve; in fact it surprises me that the average deviation should be as small as 1.22. Nevertheless it is large enough materially to reduce the total number of accurate classifications with this method.

I have tried to get some impression as to the total number of accurate discriminations possible in the following way. The average deviation for F is 0.93 on a 9-point scale, 8 units long. Suppose that we increase the unit so that the points on the scale are separated by a difference equal to the probable error of the difference between them. In such a case we should just as often get perfect agreement as not, a way of dividing the scale into liminal steps. We can change 0.93 into a P. E. by multiplying by 0.845 (assuming normality of distribution); and, assuming that the same P. E. applies to two adjacent points, we can take the square root of twice its square for the P. E. of the difference and thus the new unit, which is for F 1.11. Similarly the unit for A is 1.66; for B, 1.41; and for C, 1.65. Now how many significant points can we get? The numbers of units in an 8-unit scale are as follows: F, 7.2; A, 4.8; B, 5.7; C, 4.8. Remembering that in a limited scale there is always one more point than there are units, we may say (roughly) that we might safely attempt 8 discriminations for F, 6 for A, 7 for B, and 6 for C. This result gives us 8×6×7×6=2016 supraliminally different odors.

If, instead of taking the averages of all pairs, we take the averages for the expert *C-H* pair, we come out by the same method with 3240 odors. The minimal average deviations, irrespective of the pairs that gave them, give 4410 odors. In general it seems that 2000, or possibly even 4000 odors can be distinguished from each other the majority of the time by this method and in terms of this system.

This result seems to me to be extremely promising. There is relatively great consistency for this difficult task. It is certainly worth the psychologist's while to see if he can do as well or better in the psychological laboratory, and, if he can, to repeat the attack upon some of the old problems of smell with this system behind him.

Harvard University
EDWIN G. BORING

A NOTE ON THE INTERCORRELATION OF INTELLIGENCE TESTS

The following data were derived from the testing of eighty-eight children, nine years of age, with Series II, Examination C, of the Dearborn Group Test of Intelligence. This examination is made up of four parts: (1) picture sequence; (2) word sequence; (3) form completion; and (4) sentence completion.

The correlations here presented will be of interest to those who are concerned with the intercorrelations of the parts within a test, and the correlations of the parts with the total score. It will be seen, of course, that this study bears directly on the issues raised by the Army test results[1] regarding the value of tests having high or low intercorrelations of parts and high or low correlation of parts with the total scores.

Now, inasmuch as the correlations between scores of parts with the total scores are in a degree self-correlations, it is necessary to equalize the possible influence of each on the total score. If each section contained the same number of items and had the same time allotment, no weighting would be necessary. But inasmuch as time and possible maximum score vary for the several parts, scores were weighted; and for these the following coefficients of correlation were found.

Parts	*r*
I with II	.35 ± .06
I with III	.35 ± .06
I with IV	.42 ± .05
II with III	.11 ± .07
II with IV	.50 ± .05
III with IV	.23 ± .07
Total with I	.69 ± .03
Total with II	.69 ± .03
Total with III	.57 ± .04
Total with IV	.77 ± .03

If these results are valid and typical, it does not seem at all necessary to hold with some investigators that in constructing tests we must choose between the mathematical desideratum of low intercorrelations but high coefficients with the totals (or criterion), on the one hand, and the so-called "psychological fact," on the other. This "psychological fact" has been phrased in the question, "Are there tests, as a matter of fact, which have a low intercorrelation and which correlate to a high degree with a criterion; in other words, can tests be found to meet this mathematical desideratum?"[2] From the foregoing coefficients, it appears that there is fairly good reason to answer this question in the affirmative.

In the six intercorrelations there is only one which is not significantly below the coefficients obtained for any of the parts with the whole. That is the coefficient of .50 for II and IV (word sequence and sentence completion). The reason for this intercorrelation, somewhat higher than the rest, lies in the fact that both II and IV involve language; yet .50 is sufficiently far removed from unity to enable one to say that these parts are not necessarily measures of the same fact. However, it will be noted that this correlation of .50 is well below those for parts II and IV with the total, .69 and .77 respectively.

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[1] Psychological Examining in the U. S. Army, *Memoirs National Academy*, 15, 1921, 316, 338.

[2] F. N. Freeman, *Mental Tests*, 1926, 238.

It must be remembered that the question of "psychological fact," quoted above, was raised in connection with the results obtained with Army tests. No one will deny that these tests were of great service in the solution of a pressing problem. But at the same time it should also be clear that the results were obtained under conditions which by no means warrant their acceptance as data out of which to build principles. If the problem of the intercorrelation of parts and correlation of parts with total scores or criteria is to be dealt with, data must be obtained under reliably controlled conditions, such as were impossible in the Army.

Cornell University FRANK S. FREEMAN

BLUFFING IN EXAMINATIONS

Fernberger's recent note[1] on the prevalence of bluffing in examinations suggests that the matter be investigated by means of objective type questions. The following three questions were accordingly inserted in the first hour-quiz given students in elementary psychology at Wesleyan University and at Antioch College. The topic covered in the preceding instruction period had been the measurement of individual differences. "Do not guess" instructions were given and it was implied that there was a true answer to each item. The quiz was taken by 46 students at Wesleyan and 84 at Antioch.

"(51) Psychoterminality is: (a) discussion of endless possibilities; (b) a neologism; (c) prophesy of maximum mental development; (d) result of feeding thyroid extract.

"(54) The trait of sexagenarianism is measured by: (a) one test in Army Alpha; (b) tests of sex differences; (c) a special test; (d) no kind of psychological test.

"(56) Tests of nevrility: (a) correlate highly with emotional balance; (b) have low correlation with intelligence; (c) are very reliable; (d) have not yet been made."

The following table summarizes the results. The three headings might be interpreted to read: knowledge, intelligent ignorance, bluffing.

Question	Group	Answers returned—in percentages		
		Right	Omissions	Wrong
51	Wesleyan	2	33	65
	Antioch	2.5	45	52
54	Wesleyan	37	43.5	20
	Antioch	36	52	12
56	Wesleyan	28	35	39
	Antioch	18	63	19
All	Wesleyan	22.3	37.2	41.3
	Antioch	18.8	53.3	27.6

These results do not cause one to rise in alarm; I am not sure that the showing is not better than I had thought possible. Call it 'luck' if you will, but only about a third fell into the trap digged for them.

But there is a moral. The students were at first inclined to be a bit indignant at being thus tricked. But the announcement that hereafter other such questions would be a matter of routine seemed to have a salutary effect in persuading them that it is necessary to know when you do not know.

Antioch College HORACE B. ENGLISH

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[1] S. W. Fernberger, On bluffing in examinations, this JOURNAL, 38, 1927, 155-156.

MEMORY COLOR AND TUBERCULOSIS

During a period of three and a half years as a patient in a hospital for tuberculosis I found that the phenomenon of memory color[1] was quite frequently experienced by patients who had had a hemorrhage of the lungs. Tuberculous patients invariably examine every expectoration because they soon learn to determine whether their sputum is positive or negative. If it is greenish-yellow in color it is generally positive. Ordinary catarrhal droppings and sinus secretions are more yellowish.

For many months following hemorrhage of the lungs, tubercular patients have fleeting impressions when they expectorate white or yellow matter that it is red in color. Over 95% of all hemorrhage cases report this illusion. The larynx is slightly congested while expectorating. This gives one the impression that the mucous comes from the lower passages. Any congestion in the larynx is likely to arouse a 'beware of hemorrhage concept' and the illusion. In the majority of cases the 'red' vanishes in a few seconds, but in some it persists for two or three hours. Color discrimination is normal for the patients except under these peculiar circumstances.

University of Chicago RAY MARS SIMPSON

NINTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

The Ninth International Congress of Psychology will convene at Yale University in New Haven, Connecticut, in the first week of September, 1929.

The officers of the Congress are as follows: President, J. McKeen Cattell; Vice-president, James R. Angell (Yale); Secretary, Edwin G. Boring (Harvard); Treasurer, R. S. Woodworth (Columbia); Foreign Secretary, Herbert S. Langfeld (Princeton); Executive Secretary, Walter S. Hunter (Clark); Chairman of the Program Committee, Raymond Dodge (Yale); Chairman of the Committee on Arrangements, R. P. Angier (Yale).

Besides these men the National Committee includes J. E. Anderson (Minnesota); Madison Bentley (Illinois); E. A. Bott (Toronto); H. A. Carr (Chicago); Knight Dunlap (Johns Hopkins); S. W. Fernberger (Pennsylvania); William McDougall (Duke); W. B. Pillsbury (Michigan); C. E. Seashore (Iowa); L. M. Terman (Stanford); E. L. Thorndike (Columbia); H. C. Warren (Princeton); M. F. Washburn (Vassar); R. M. Yerkes (Yale).

The first three officers and all but three members of the National Committee were elected by the American Psychological Association by mail ballot.

This is the first meeting of the Congress in America. The list of meetings with their presidents is as follows: Paris, 1889 (Charcot); London, 1892 (Sidgwick); Munich, 1896 (Stumpf and Lipps); Paris, 1900 (Ribot); Rome, 1905 (Sergi); Geneva, 1909 (Flournoy); Oxford, 1923 (Myers); Groningen, 1926 (Heymans); New Haven, 1929 (Cattell).

It is expected that the Congress in the United States will be truly international in character. The Americans hope that the appointment of some foreigners for lectures and lectureships can be arranged near the time of the Congress, so that foreign attendance can be increased and international solidarity within psychology furthered. Most appointments of this kind would have to apply to psychologists who speak English.

E. G. B.

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[1] Cf. G. K. Adams, An experimental study of memory color and related phenomena, this JOURNAL, 34, 1923, 359-407.

THE NEW YORK MEETING OF THE AMERICAN ORTHOPSYCHIATRIC ASSOCIATION

The fifth annual meeting of the American Orthopsychiatric Association was held at the Russell Sage Foundation, New York City, on February 24th and 25th, with an attendance of over 200 psychologists, psychiatrists and psychiatric social workers. Four sessions were held for the presentation of scientific papers. On the evening of the 24th the annual dinner was held, followed by the address of the president, Dr. K. A. Menninger, on "What is orthopsychiatry?"

One session was devoted primarily to papers concerned with measurements. Dr. L. N. Yepson reported on "Objective estimation of social behavior." He used a graded series of statements about specific responses of the subjects, and had each rater check the item under every trait which best described the subject's behavior. Dr. P. H. Furfey described "A scale for measuring mental age." Dr. G. J. Rich reported on "Body acidity as related to emotional excitability," and a paper by Dr. C. W. Darrow on "Some physiological aspects of repression" presented some interesting differences between the effects of various stimuli as shown by the psychogalvanic reflex and the blood pressure. The leading paper of this session, "The study of the criteria and definitions of different groups of abnormal personalities," by Dr. William Healy, outlined four principal types of disordered personality which could be clinically differentiated.

The remaining sessions were devoted mainly to topics concerned with behavior disorders in children. Several of the papers were of especial interest to psychologists. Dr. H. M. Adler and Mr. S. H. Tulchin discussed the "Intelligence of criminals." Data from over 10,000 individuals yielded results in harmony with those which have recently been published by other investigators. Dr. C. Shaw considered "The correlation between the rate of male juvenile delinquency and certain indices of community organization and disorganization." Two papers, by Drs. S. T. Orton and P. Blanchard, were concerned with reading disabilities, more especially their effect upon the child's behavior as a whole. Dr. J. Levy reported tabulations of the size of the family of children who are referred to a clinic as behavior problems and showed that, contrary to general belief, the only child is not especially liable to become a problem. Dr. S. T. Orgel reported "A genetic study of mongolism," in which he was unable to isolate any causative factors for the condition, but was able to show that many of the factors which have been postulated as causes cannot be effective.

The meeting is significant in that it forms one of the few common grounds where psychologists, psychiatrists and workers in the social sciences can come together to discuss problems which require for their solution the coöperative efforts of these several points of view and techniques. The need of such a meeting is shown by its rapid growth in a very few years.

Institute for Juvenile Research GILBERT J. RICH
Chicago, Illinois

THE BOOK REVIEW DEPARTMENT

Professor Joseph Peterson of the George Peabody College for Teachers, of Nashville, Tennessee, assumes, with this number of the JOURNAL, the editorship of the department of Book Reviews. This department will be greatly strengthened; more space will be devoted to it than has been given it in the past, and emphasis will be placed upon constructive and critical reviews rather than upon mere abstracts.

K. M. D.

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THE AMERICAN
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Vol. XL JULY 1928 No 3.

THE COLD RECEPTOR

By C. R. PENDLETON, University of Pennsylvania

The original object of this study was to apply histological tests to the Von Frey theory of receptors for the four modality sensitivity of the skin. The theory was propounded a number of years ago[1] and yet no direct histological demonstrations have been made. In fact it appears that there are no adequate studies of the nerve endings of the hairy parts of the skin. Ruffini[2] says that all histological studies of the skin of man and the higher vertebrates worth mentioning have been confined to the palms of the hands and fingers, the soles of the feet, the eye, the generative organs, the lips and similar hairless parts. Very little seems to have been done since Ruffini's time.[3]

Further, the theory seems to have an illogical coupling of one kind of receptor for the conjunctiva of the eye and another kind for the other parts of the skin. It calls for the Krause end-bulbs in the conjunctiva, and the Dogiel corpuscles in the skin, which latter are assumed to be the same as the Golgi-Màzzoni corpuscles of Ruffini.[4] Two different forms for the same function is not unknown; but here the Krause end-bulb of the eye, the supposed cold organ seems to be homologous to the Meissner corpuscle of the palms, the supposed tactile organ. Krause end-bulbs lie just below the conjunctiva in the papillae, while Meissner's corpuscles occupy a corresponding position in the hairless parts of the body, *i.e.* just below the epidermis,[5] which is

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[*] Accepted for publication May 9, 1927.

[1] M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. könig. säch. Gesell. d. Wiss., math.-phys. Kl.*, 47, 1895, 180.

[2] A. Ruffini, Les expansions nerveuses de la peau chez l'homme et quelques autres mammifères, *Revue General d' Histologie*, 1905, 425.

[3] Luigi Luciani, *Human Physiology*, IV, tr., 1917, 55.

[4] The Golgi-Màzzoni corpuscles of Ruffini would seem to be the forms indicated. The Dogiel corpuscles according to Ruffini's nomenclature, have been reported but once, by Dogiel, and are intermediate in form between the Meissner corpuscle and the free papillary nerve ending (A. Ruffini, *op. cit.*, 435-442).

[5] L. F. Barker, *The Nervous System*, 1899, 387, fig. 243. A. S. Dogiel, Die Nervenendkörperchen (Endkolben, W. Krause), in der Cornea und Conjunctiva Bulbi des Menschen, *Arch. f. mikr. Anat.*, 37, 1891, 605. A. S. Dogiel, Die Nervenendigungen im Lidrande und in der conjunctiva palpebr. des Menschen, *Arch. f. mikr. Anat.*, 44, 1894-5, 15-25, table III, fig. 1. G. A. Piersol, *Human Anatomy*, 1907, 1016.

homologous to the conjunctiva. Further, cats and dogs have Krause end-bulbs in the dermic papillae of the soles of the feet where men and apes have Meissner corpuscles.[6]

Opposed observations. A number of years ago Donaldson,[7] using gold chloride, failed to find any organs under the temperature spots. Häggqvist,[8] using hematoxylin and eosin, found smooth muscle bundles under the cold spots. Stetson,[9] working on the hair follicle, failed to find the tactile organ. Goldscheider,[10] working with chemicals, concludes that the end-organs of temperature are not stratified and lie at the boundary between the epidermis and the corium. Dallenbach,[11] using methylene blue, failed to find any bulbs under the temperature spots.

Confirmatory observations. It may be said that all these negative findings are meaningless, especially as histology is involved in all but one of them, while on the other hand Von Frey's co-workers and students have offered several confirmatory observations that cannot be ignored.

Strughold[12] found that Krause end-bulbs were present in all parts of the mouth sensitive to cold—but the converse was not established. Strughold and Karbe[13] found that the topography of the cold sense of the conjunctiva agreed with that of the Krause end-bulbs as reported in the literature. Strughold[14] found that the threshold for the cold spots was higher in regions where the conjunctiva was thicker. Strughold and Karbe[15] colored the nerves of the conjunctiva with methylene blue and found that Krause end-bulbs thus displayed were under cold spots, the positions of which were later determined.

Plan of the present study. Having in mind these contradictory findings, the writer tried to find some solution of the problem. This work was divided into five parts. I. The comparison of the patterns of the temperature spots with those of the nerve end-organs. II. The search for Krause end-bulbs and Ruffini cylinders in the hairy parts of the skin. III. The application of Bielchowsky's method of staining to the skin. IV. The determination of the most probable depth of the cold receptors. V. An histological examination of the skin. The first three problems gave negative results. The last two, however, gave results that we believe are significant. The work is reported as a whole, for the negative results, as well as the positive, may be of some assistance to other workers in this field.

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[6] A. Ruffini, *op. cit.*, 458-476.

[7] H. H. Donaldson, Research on the temperature-sense, *Mind*, O. S., 10, 1885, 408. f

[8] Gösta Haggqvist, Histophysiologische Studien über die Temperatursinne der Haut des Menschen, *Anat. Anz.*, 45, 1913, 46-63.

[9] R. H. Stetson, The hair follicle and the sense of pressure, *Psychol. Monog.*, 32, 1923, no. 145, 1-17.

[10] A. Goldscheider and H. Hahn, Untersuchungen über den Temperatursinn: II. Chemische Reizung und Lahmung der Temperaturnerven. Schichtungstheorie. Paradoxe Wärmeempfindung. Hitzeempfindung. *Arch. f. d. ges. Physiol.*, 206, 1924, 308-324.

[11] K. M. Dallenbach, The temperature spots and endorgans, this JOURNAL, 39, 1927, 424 ff.

[12] Hubert Strughold, Die Topographie des Kältesinnes in der Mundhöhle, *Zsch. f. Biologie*, 83, 1925, 515-534.

[13] Hubert Strughold and M. Karbe, Die Topographie des Kältesinnes auf Cornea und Conjunctiva, *Zsch. f. Biologie*, 83, 1925, 189-200.

[14] Hubert Strughold, Die Schwellen des Kältesinnes am Auge, *Zsch. f. Biologie*, 83, 1925, 201-206.

[15] Hubert Strughold and M. Karbe, Vitale Färbung des Auges und experimentelle Untersuchung des gefärbten Nervenelemente, *Zsch. f. Biologie*, 83, 1925, 297-308.

I. COMPARISON OF THE PATTERNS OF THE TEMPERATURE SPOTS AND OF THE NERVE END-ORGANS

In the first part of the work, emphasis was laid upon technique so as to avoid two possible sources of error observed in the earlier investigations. The first of these was the possible shifting of the organs in the excision and later manipulations. Although preliminary experiments seemed to show that the cold spots did not shift even when the skin was pulled to one side, or lifted, it seemed advisable to take out large pieces of skin with several spots to see if any pattern relation between the spots and the organs could be observed. The second was the staining. It is very well known that the special nerve-stains are capricious, staining only a part of the nerve elements, and often stopping short at some point, for no discoverable reason. We, therefore, decided to use ordinary histological stains and to depend on the organized tissues constituting the nerve end-bulbs for a means of identification. Nerve end-bulbs are often seen in good preparations and are large enough to be readily detected—the smallest being several times as large as the red blood corpuscle.[16] The only tissue with which they might be confused is a section of a nerve trunk; and in good preparations, nerve trunks in cross sections show numerous circular myelin sheaths, a structure lacking in all nerve end-organs so far observed.[17]

Observers: The observers were E. S. Rudisill and S. F. Cooper, with whom the work was carried through to completion; and S. Sanderson, D. E. Busby and C. R. Pendleton, with whom this part of the work was abandoned before completion.

Apparatus: The apparatus consisted of four parts: (1) An arm rest with a thin metal table above it, the height adjustable so that the metal would not lower the temperature of the skin; a square hole in the metal table allowed careful manipulation of a small exposed part of the forearm of the observer. (2) A marker.[18] (3) A double scale graduated along two perpendicular lines in ½-mm. steps, made of transparent celluloid, so that the coördinates of marks of cold or warm spots could be read with respect to two points previously tattooed in the skin. (4) The best histological apparatus obtainable.

Procedure. The procedure was to explore the skin (size of area 1 x 2 cm.), by continuous motion in one direction along parallel lines (¼-⅓ mm. apart), mark the spots, read the coördinates, enlarge ten times and transfer the coördinates of every observation to a single sheet of paper. Clusters of observations were assumed as the most probable location of the temperature organs. Pieces of skin over 1 x 2 cm., including both tattoo points, were then removed by the experimental dermatologist of the Medical School, care being taken to get all of the lower layers of the skin; then paraffin serial sections were made.[19] Some were stained with Mallory's aniline blue, others with Delafield's hematoxylin and Van Gieson's solution.

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[16] A. A. Böhm, M. von Davidoff, and G. C. Huber, *A Textbook of Histology*, 1926, 170.
[17] A. Ruffini, *op. cit.*, 438.
[18] C. R. Pendleton, Instrument for marking the temperature spots on the skin. *J. Exper. Psychol.*, 8, 1925, 471-474.

Results. No nerve end-organs were found. Certain bodies observed in preliminary studies of tissue taken from autopsies, thought to be end-bulbs, turned out to be cross sections of nerve trunks. All bodies in the two biopsies studied, resembling end-bulbs were nerve trunks. This was demonstrated both by the presence of myelin sheaths and by serial section study. Consequently no comparison of the two patterns, one of temperature spots, the other of end organs was possible.

One of two conditions is possible here—either there are no end-bulbs in the hairy parts of the skin, or the technique used was faulty. In regard to the first possibility, Von Frey[20] found one in the upper arm, but Ruffini says that there is no adequate knowledge of this region.[21] In regard to the second possibility, all we can say is that we used the best technique available. The results of this part of the work quite naturally lead to the search for specific end-organs in the hairy regions of the skin.

II. SEARCH FOR KRAUSE END-BULBS AND RUFFINI CYLINDERS IN THE HAIRY REGIONS OF THE SKIN

In the second part of the work we used skin specimens from human legs taken immediately after amputations; specimens from the palms, the skin of the abdomen, and the conjunctiva from posted bodies; and specimens of palmar and toe pads and of the conjunctiva from the cat.

Procedure: We tried several fixing fluids—10% formalin, Bouin's, Fleming's, and Allen's. The latter was chosen as possibly better than the others, though Bouin's was used as an alternate.

Twelve blocks of human skin, taken from three different amputations, trimmed to 1 sq. cm., after fixation; a half-dozen blocks of smaller size from abdomen; three or four from fingers and palms, one from human conjunctiva, one from cat's conjunctiva, one cat's toe pad, and one cat's palmar pad, were used. The blocks of skin taken from amputations were imbedded as described above and sectioned, some normal, some tangential, and mounted on slides. Samples of all blocks were stained, and two normal sections, seemingly the best were completely stained and finished. Several stains were tried out, and one of these blocks containing over 1,000 sections was stained in iron haematoxylin, the other in iron haematoxylin and eosin. The blocks of other tissues were treated in similar manner, except that not so many slides were made. The blocks from posted bodies were not considered reliable, but they and the other blocks showed what was wanted in a few sections.

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[19] As paraffin sections of human skin are difficult to make, and as none of the published formulae worked well enough for serial sections, that finally adopted as the best is here offered. Fix for 1½–2 hrs. in Allen's fluid, B15; wash 24 hrs. in tap water; wash for 5–6 days in several changes of 50% alcohol; then 70% alcohol, 45 min.; 85% alcohol, 45 min.; 90% alcohol, 60 min.; 3 changes of absolute alcohol of 2, 3, and 3½ hrs. each; 3 changes of chloroform of 4, 8½, and 5 hrs. each; chloroform paraffin 3 hrs; German paraffin melting at 60°C. 3 hrs.; imbed in paraffin melting at about 55°C. With the razor in the best condition, a block of tissue, trimmed after fixation to 1 sq. cm., 4 mm. thick, may be cut without shifting the razor, and a serial section containing over a thousand sections mounted without losing or marring one.

[20] M. von Frey, *op. cit.*, 182.

[21] A. Ruffini, *op. cit.*, 425.

It is believed that any square centimeter of human skin taken just below the knee will contain at least one cold spot. So that the sections of fresh human skin, trimmed to 1 sq. cm. after it had shrunken in the fixing fluid, ought to show at least one cold receptor. These two serial sections were studied in an effort to locate such an organ. The other tissues, palm of human hand, fingers, and conjunctiva, and cat's conjunctiva, toe, and palmar pads, were studied for the sake of comparison.

Results. The results of this section of the study may be summarized as follows.

(1) No Golgi-Màzzoni, Krause, or Ruffini end-organs were discovered in the hairy parts of the human skin. (2) In the human palms, where end-bulbs are known to exist in number,[22] such bulbs were found, and with very little difficulty. They are rounded organized groups of connective tissue and nuclei, different from any of the other well-known elements of the corium, the nerves having no myelin sheaths. (3) In the human conjunctiva, the cat's conjunctiva, the cat's toe and palmar pads, no definite identification of the end-bulbs in the papillae could be made. The places where they are to be found are easily discoverable; some that are probably end-organs may be pointed out. But to say that the tissue in a certain papilla is or is not an end-bulb, seems, on the basis of this study, to be impossible. The reason is obvious on examination of the slides. The dermic papillae where these bodies lie simulate the shape of the end-bulbs so closely that to discriminate the papillae with end-bulbs from those without is too uncertain to be dependable.

On the basis of these results, the probabilities seem to be against the existence of end-bulbs in the lower and middle layers of the corium, *i.e.* Krause, Golgi-Màzzoni, Dogiel, Ruffini, or whatever other name they may have. While the probabilities are about equal, for and against their existence in the dermic papillae just under the epidermis.

Any one of the following conditions may exist. (1) Bulbous corpuscles may exist in the lower layers of the corium, but are so small or so slightly organized that they are confused with other tissues. (2) Bulbous corpuscles may exist in the dermic papillae and function as the cold receptor, in which case they would be much the same as the Meissner corpuscles—though smaller and less organized—supposedly functioning as the tactile organ in the hairless parts. (3) There may be some unsuspected error of technique which makes these forms undiscernable. As a result of these considerations, the next part of the work was undertaken.

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[22] A. Ruffini, *op. cit.*

III. Bielchowsky's Method of Staining the Skin

There are four or five distinct methods of staining nerve tissue. Krause and Ruffini used gold chloride. Dogiel used methylin blue. It is somewhat more reliable than gold chloride, but many losses occur in sectioning. It works best in intravitam staining. Golgi, Cajal, and others used chrome silver and its modifications. All these methods are seemingly unsuited for quantitative work. They stain certain nerve elements and leave others untouched. What they show is brilliant, what they leave unstained may not even be suspected. They were tried out at the beginning of these studies and found too uncertain for the work in hand. But they could be applied qualitatively to the hairy parts of the skin as they have been applied to the hairless parts. Another method of staining has been developed by Bielchowsky, in which silver is used in combination with an alkali. This is claimed by many to be the best of all nerve stains. The application of this method to peripheral nerves, however, is not so good.

Much of the literature on this subject has been examined for a suitable method, with no decisive results. Modifications of Sand,[23], Agduhr,[24] and Boeke,[25] were tried on the conjunctiva, the hairless and hairy parts taken from posted bodies with the result that the connective tissue stained as brown as the nerves and no discrimination was possible. Another modification, that of Schultz, is reported by Von Frey to be the best yet devised. Before this could be tried out, a method was discovered for measuring the depths of the cold receptors, and the work turned in this direction, leaving many loose ends, some of which it is hoped will be taken up again. The greater part of the above is to be taken as a sketch rather than a set of scientific conclusions.

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[23] R. Sand, Une methode simple et elective de coloration des neurofibrilles et des cylindre-axes, *Zsch. wiss. Mikr.*, 28, 1911, 500.

[24] Erik Agduhr, Über Stückfärbung mit Bielchowskys Silberimprägnationsmethode. Einige Modifikationen, *Zsch. wiss. Mikr.*, 34, 1917, 1-99.

[25] J. Boeke, Die motorische Endplatte bei den höheren Vertebraten, ihre Entwickelung, Form und Zusammenhang mit der Muskelfaser, *Anat. Anz.*, 35, 1909, 196.

IV. Determination of the most probable Depth of the Cold Receptors

Strughold's fusion beads[26] suggested a method for measuring the depth of the cold organs, which was the next phase of the problem attacked.

Strughold used small beads of different sizes on the end of fine copper wire leads to show that the relative depths of the Krause end-bulbs below the conjunctiva corresponded to the size of the fusion beads, *i.e.* the amount of heat extracted. The variation in the size of his beads, however, seems out of proportion to the different thicknesses of tissue involved although he made allowances for several physical conditions. This irrelation would seem to be due to a factor which was not considered, namely, the size of the wire lead by which the bead is suspended. In this stimulus there are two masses of metal which extract heat, the copper bead and the copper wire lead. This latter was ignored, although it was possibly a greater factor than the bead itself in the smallest stimulus used, while in the largest it was probably only a small fraction of the total. In other words, there are two factors which make the stimulus in this series of instruments, one is constant and was incorrectly ignored and the other is variable and was used exclusively as a basis of calculation.

Method of Procedure. The method used in the present set of experiments takes advantage of the fact that copper cylinders of varying diameter and sufficient length will have only one variable, the square of the diameter. The length ceases to be a variable if it is sufficiently great, because the flow of heat is such that after a certain distance the copper wire or cylinder is at room temperature although one end be maintained constantly at a higher temperature. The physicists tell us that, with the slight difference in temperature involved and the size of the cylinders used, the length need be only several times the diameter.

If then a set of copper cylinders of varying diameter and sufficient length be used, and the room and the skin surface be kept at constant temperatures, the amount of heat extracted will vary according to the square of the diameter of the cylinders, and consequently the amount of heat extraction necessary to stimulate the cold organ can be measured. This amount will be composed of two quantities: (1) that required to arouse the organ itself in its uncovered state, which to begin with, may be assumed constant for each individual in a state of health and for a short period of time; and (2) that required to lower the temperature of the tissues lying between the surface of the skin and the organ itself. This latter quantity will vary according to the cube of the depth of the receptor, provided that the conditions, to be considered later, remain the same.

The first method for measuring the depth of the cold body is to determine the limen of a cold spot, remove a known quantity of epidermis and determine the limen again. From the three quantities determined, the depth of the cold body below the surface can be measured. The formula will be $x = c/(1-d)$ in which c is the thickness of the epidermis removed and d is the ratio of the two limens. The constants necessary to construct the equation are eliminated, but the constant representing that part of the heat extraction necessary to stimulate the uncovered cold receptor must be ignored in this method. If it is small the results will not be appreciably affected; in any case the value x will fall below the cold organ unless this quantity be zero.

A second method which offers at least a possibility of accounting for this last quantity is as follows. Determine the limen of a number of cold spots in different parts of the body where the skin varies in thickness. Next measure the thickness of the several layers of the skin over the several spots. Then compare the ratios of limens to the ratios of thickness. If the several layers of the skin should vary according to the total thickness, nothing could be gained; but as this seems not to be the case[27] that set of ratios of thicknesses which is nearest to the ratios of the limens will be indicated. If then a constant determinable quantity be subtracted from the limens, to represent that necessary to stimulate the receptor itself, leaving a second set of quantities and ratios which compare more favorably with the ratios of depths of certain layers of skin, then the depths and layers of skin thus found are indicated for the location of the receptors. And the quantity extracted is a measure of the amount of heat extraction necessary to arouse the uncovered cold organ.

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[26] Hubert Strughold, *op. cit.*, 202.

The second method, however, gives only the relative depth of the cold organ. This body might lie two or three times as deep as the position indicated. A comparison of this depth with that found by the first method will remove the uncertainty.

The cylinders were made of copper wire drawn and ground to the proper diameters and mounted by two fine copper wires on round brass key tags stamped with the diameter of the cylinders. They are in two series, one running from 4.00 mm.–0.20 mm. in steps of 0.05 mm.; the other running from 0.26 mm.–0.08 mm. in steps of 0.02 mm. The method of construction allows an accuracy of at least 0.005 mm. but the original wires were not strictly round and the irregularities could not easily be removed. The practice was to take the average of the largest and the smallest diameter as the nominal diameter.

The sizes turned out to be unexpectedly small. Strughold's smallest reported bead had a diameter of 0.22 mm. on a wire 0.10–0.15 mm. The smallest cylinder reported cold was 0.08 mm. which happened 7 times, while the cylinders having a diameter of 0.14 and lower were reported cold 37 times. The surface of the skin was moistened with a glycerin solution to correspond somewhat with the condition of the conjunctiva. Assuming that the cold organ in Strughold's experiment lay just below the conjunctiva, the cold organ in the observations just mentioned—on the back of the hand—must lie well up in the epidermis. The thickness of the conjunctiva varies from 45–80μ.[28] The thickness of the several layers of the epidermis over the spots which gave the above observations may be seen in Table IV. Biopsy No. I and VI where the stratum corneum is roughly 67μ and 63μ respectively.

Observers. The observers in this series were H. S. Oberly (5 limens and 3 biopsies), and C. R. Pendleton (26 limens and 3 biopsies).

Results. The magnitude of the limen is affected by a number of conditions. The state of attention is obvious, but the reported pleasing stroke of a camel's hair brush with the warm glycerin solution had a surprising though slight effect in lowering the limen. Fatigue is the hardest factor to deal with. There seem to be two kinds. The first is slight and heightens the limen, a slight rest will often restore the original state. It is important to note here that if the series be started too high above the limen, the organ will become fatigued before the true limen is reached. Apparently subliminal stimuli will also fa tigue the organ. Both were avoided by using short serie starting at or near the lowest perceived stimulus. The lime is then found to drop slowly until a fairly even position is a tained. After a few series the limen will again jump upwar and the cold spot will not return to normal for some tim This is the second kind of fatigue mentioned above. Usuall two or more spots were studied by turns, so that little tim was lost while certain spots were resting. In calculating th

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[27] V. Drosdoff, De la mensuration de l'epiderme dans les differentes parties du humain et des rapports entre son épaisseur et la sensibilité electrocutanée, *Arch. de Phys. normal et pathol.*, 2 Ser. 6, 1879, 117-134. A. Koelliker, *Handbuch der Gewebelehre des Menschen*, 1889, 161, 203.

[28] G. A. Piersol, *op. cit.*, 1451.

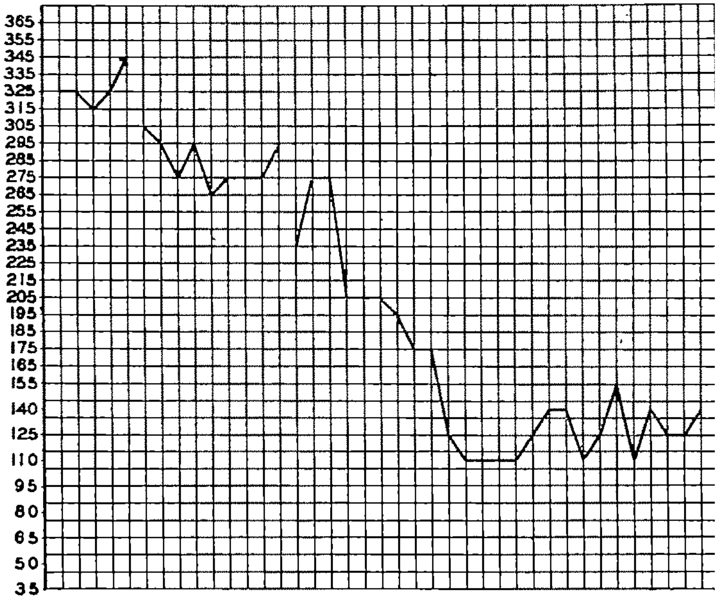

FIG. 1. SUCCESSIVE STAGES OF THE LIMEN OF SPOT NO. 5

limens, those series which seemed to be plainly affected b fatigue were discarded but no rule has been evolved to determi which to rule out. Fig. 1 illustrates how the limen fell as su cessive stages of fatigue were eliminated.

A number of physiological conditions seem to affect t limen. A chilly feeling, cold hands, slight drafts, a cold su face under the hand all seemed to have marked effect on t limen. A woolen cloth was placed under the hand and arm an as comfortable position as possible obtained. Even then cramped feeling from a twisted arm in the palm-up positio sometimes interfered. After tattooing the location of a spot, was necessary to wait sometimes for a week for the injure cuticle to recover completely; a short rest after walking seemed to be necessary, and the amount of caffeine used for breakfast seemed to affect it.

The temperature of the room was kept at 20° C ±0.5 by opening and closing a window and waiting until a thermometer, graduated to tenths of a degree, standing as close as practicable to the cylinders and the observer, recorded a temperature within the required limits. The surface of the skin was moistened with a mixture of glycerin three parts and water two parts; the glycerin to prevent heat extraction from evaporation; the water to give greater fluidity; and the mixture to even out certain irregularities in the surface of the skin, and to approximate the condition of the conjunctiva, with which comparisons have already been made. The accurate placement of the stimulus is more serious than might be supposed. To place a cylinder of 1/10 or even 2/10 of a millimeter in diameter on a point is not easy. Supposing that it is often missed, a few trials for each cylinder were allowed, when there was no response. The time between applications was about a half second and the length of the application varied from an in-instant for the smallest to about one second for the largest. The latter was found necessary; the former seemed to be useful in avoiding fatigue.

Table I gives the limens determined in different parts of the arm and hand of two *Os*, calculated by a form of Müller's method of Least Differences.

It should be noted that, in those cases where no limen is given, but only the lowest perceived, the figures mean practically nothing. The limens probably were never reached owing to fatigue or to some condition of injury. The figures are given merely to complete the record.

In determining the depth of the cold organ by the first method, the cuticle is ground down with sand paper, after the limen is determined. Care must be taken here to prevent injury to the sensitive tissues since this heightens the limen. Accordingly the sensitivity was tested occasionally as the grinding went on. Table II indicates results from this process in two cases.

The next step is to excise two blocks of cuticle, one from the ground portion and one from the normal skin as near and as similar as possible to the first, and measure the thickness of the strata in the two cases, by slides prepared according to the formula given above. The last column in Table II gives these results. It will be seen that $c = 166\mu$, $d = 0.3305/0.6179$; from which $x = 357\mu$. That is, the depth of the cold organ is 357μ below the surface of the epidermis. By comparison with Table III, Biopsy V, column 7, which shows the thickness of the stratum corneum, it will be seen that this position is about 24μ, below the bottom of the stratum corneum or near the top of the living part of the skin.

A somewhat simpler variation for determining the value c is to slice off a piece of the epidermis after the exact location of the spot has been marked, section and measure the thickness of the epidermis removed and proceed as above. This, however, has not been tried.

TABLE I

LIMENS OF COLD SPOTS IN DIFFERENT PARTS OF THE HAND AND ARM

O	Location	Spot no.	No. of series	Limen	Lowest perceived	Biopsy no.
Pendleton	Center arm	1	2	0.2250	0.20	
		2	6	0.1666	0.14	
		3	0		0.20	
		4	0		0.70	
	Inner arm	1	4	0.1450	0.15	
		2	5	0.1550	0.12	
		3	6	0.1900	0.18	
	Back of hand	1*	1		0.22	
		2	17	0.0953	0.08	I
		3	7	0.1859	0.18	
		4	0		0.65	
		5†	0		1.30	
		6	8	0.1775	0.18	
		7	0		0.18	
		8	6	0.1200	0.12	
		9	3	0.3266	0.22	
		10	0		0.20	
		11	3	0.1600	0.16	
		12	0		0.20	
	Palm of thumb	1	17	0.4350	0.40	II
	Center of Palm	2	15	0.9433	0.65	
		3‡	0		1.30	
		4‡	0		1.30	
		5§	0		1.75	
	Heel of palm	6	8	1.1750	0.65	
		7	16	1.1750	0.95	III
(	Back of hand	1	3	0.1733	0.18	
		2	11	0.1036	0.08	IV
	Palm of thumb	3	8	0.3875	0.30	
		4	2	0.5000	0.50	
	Heel of palm	5	21	0.4857	0.20	V

*Moved twice, *i.e.* three locations within 2 mm.
†Apparently injured in tattoo, it was insensitive for some time.
‡Apparently inflamed at the time.
§Not certain of location of spot.

In the second method for determining the depth of the cold organ the limens are determined and the thickness of the overlying tissues measured as described above. But here the cor-

TABLE II

SUCCESSIVE LIMENS IN GRINDING PROCESS

O	Location	Spot No.	No. of grinding	No. of series	Limen	Lowest perceived	Limen raised to ⅔ power	Biopsy	Thickness of stratum corneum
Oberly	Heel of palm	5	Normal	21	0.4857	0.30	0.6197		333μ
			A			0.50			
			B			0.30		V	
			Final	2	0.1900	0.20	0.3305		167μ
Pendleton	Heel of palm	6	Normal	8	1.1750	0.65			
			A			0.35			
			B	4	0.4500	0.35			
			Final	4	0.4250	0.35			

responding layers of skin of different thickness are measured, the ratios found which are nearest to the ratios of the relative depths, *i.e.* the limens raised to the two-thirds power. Table III is a set of limens from 6 cold spots compared with the thickness of different layers of epidermis under them. Column 4 gives the limen in terms of the diameter; column 5, the diameter raised to the two-thirds power; and column 6, the same value as column 5 in ratios, *i.e.* every set of figures is divided by the value obtained in Biopsy I. Columns 7, 9, 11, and 13 give the measured thickness of different parts of the epidermis, while columns 8, 10, 12, and 14 give the ratios of the same values.

It will be noted that column 14, the bottom of the papillae—the juncture of epidermis and the corium—agrees best with the ratios of the limens, *i.e.* the sum of the deviations is smaller. But it is to be noted that the cold organ is somewhere above this point, the distance depending on the amount of the stimulus used in exciting the organ itself. If certain arbitrary amounts be subtracted, to account for this factor, other and higher points of 'best agreement' will be obtained. Table IV is a table of three such subtractions. It will here be seen that as the amount increases to about 0.1000 the sum of the deviations becomes least, further increase enlarges the sum. For this amount that depth of epidermis has the least sum of deviations which corresponds to the bottom of the stratum corneum. This agrees with the depth found by the first method, and with the above comparison with Strughold's data.

But if 0.1000 is the amount necessary to arouse the cold organ itself, this amount must be subtracted from the two members of the ratio calculation above. The quantity d then becomes 0.2305/0.5179, c remains the same, so that x (formula on p. 359 above) becomes 300μ below the surface of the skin. A comparison with the thickness of the strata given in Table IV, Biopsy V, column 7, places this point just above the bottom of the stratum corneum. As the number of observations is small, this calculation must be considered in connection with results from the first method. A position just below the bottom of the stratum corneum, in the top of the stratum malpighi, where sensitivity is generally believed to begin, seems most likely.

At this point it should be noted that the cold spots studied were those having the smallest limens, which are believed to be those in which the cold organs are more superficial. The other spots whose limens are higher may be supposed to lie lower down in the stratum malpighi. From Table III, Biopsy I, column 13, it may be seen that the actual depth of the bottom of the stratum malpighi (bottom of papillae) on the back of the

TABLE III

LIMENS COMPARED WITH THICKNESS OF THE STRATA OF THE EPIDERMIS

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Biopsy	Location of Cold Spot	No. of Series	First Limen			Stratum Corneum		Top of Papillae		Center of Papillae		Bottom of Papillae		Alternate Limen	
			Diam.	⅔ power	Ratio to I	μ	ratio	μ	ratio	μ	ratio	μ	ratio	⅔ power	Ratio to I
I	CRP 2 Back of hand	17	0.0953	0.2086	1.00	67	1.00	83	1.00	125	1.00	167	1.00	0.1086	1.00
II	CRP 1 Palm (thumb)	17	0.4350	0.5741	2.75	283	4.25	358	4.30	450	3.60	500	3.00	0.4741	4.37
III	CRP 7 Palm (heel)	16	0.1750	1.113	5.34	600	9.00	633	7.60	683	5.47	750	4.50	1.013	9.33
IV	SO 5 Palm ground	2	0.1900	0.3305	1.58	167	2.50	233	2.80	283	2.27	333	2.00	0.2305	2.12
V*	SO 5 Palm natural	21	0.4857	0.6179	2.96	333	5.00	392	4.70	450	3.60	516	3.10	0.5179	4.77
VI	SO 2 Back of hand	11	0.1036	0.2206	1.06	63	0.95	105	1.25	130	1.04	155	0.93	0.1206	1.11

*This biopsy was taken a short distance from spot 5, from tissue of exactly the same external character, which had not been ground. The limens in this line were taken from spot 5 before grinding.

hand of one *O* (Pendleton) is 167μ. This corresponds to a cylinder of 0.19 mm. diam. All the limens of this region except one fall within this limit. Laboratory notes show that this spot was dull and hard to measure.

TABLE IV

LIMENS COMPARED WITH THICKNESS OF EPIDERMIS AFTER SUBTRACTING QUANTITIES TO REPRESENT THAT AMOUNT NECESSARY TO AROUSE THE ORGAN ITSELF

Ratio	Biopsy	Limen ratio	Deviations			
			Stratum Corneum	Top of papillae	Center of papillae	Bottom of papillae
original	I	1.00	0.00	0.00	0.00	0.00
	II	2.75	1.50	1.55	0.85	0.25
	III	5.34	3.66	1.26	0.13	0.84
	IV	1.58	0.92	0.22	0.69	0.42
	V	2.96	2.04	1.74	0.64	0.14
	VI	1.06	0.11	0.19	0.02	0.13
	Sum of deviations		8.23	4.96	2.33	1.78
0.05 subtracted	I	1.00	0.00	0.00	0.00	0.00
	II	3.30	0.95	1.00	0.30	0.30
	III	6.70	2.30	0.90	1.23	2.20
	IV	1.77	0.73	1.03	0.50	0.23
	V	3.58	1.42	1.12	0.02	0.48
	VI	1.08	0.13	0.17	0.04	0.15
	Sum of deviations		5.53	4.22	2.09	3.36
0.10 subtracted	I	1.00	0.00	0.00	0.00	0.00
	II	4.37	0.12	0.07	0.77	1.37
	III	9.33	0.33	1.73	3.86	4.83
	IV	2.12	0.38	0.68	0.15	0.12
	V	4.77	0.23	0.07	1.17	1.67
	VI	1.11	0.16	0.14	0.07	0.18
	Sum of deviations		1.22	2.69	6.02	8.17
0.15 subtracted	I	1.00	0.00	0.00	0.00	0.00
	II	7.24	2.99	2.94	3.64	4.24
	III	16.43	7.43	8.83	10.96	11.93
	IV	3.08	0.58	0.28	0.81	1.08
	V	7.98	2.98	3.28	4.38	4.88
	VI	1.20	0.25	0.05	0.16	0.27
	Sum of deviations		14.23	15.38	19.95	22.40

Of the factors which would tend to vitiate the results the most obvious are the crude methods of calculating the limens and the great variability in measuring the depths. In calculating the limens, fatigue prevents anything like a determination of the limen by the $\Phi(\gamma)$ hypothesis. The method of least differences is the best method that could be used. With plenty of time much more accurate results could be obtained, but these probably would not alter the stratum in which the cold organ is found to lie. The measurements on the depths of the layers of the skin are averages of four positions taken, as near as possible, to the actual position of the cold spot.

Other conditions which might alter the calculated depth are: (1) a change in the direction of the flow of heat, *i.e.* it might flow more tangentially in some strata than in others; (2) a difference in the specific heat of different strata; and (3) nearness to blood vessels. All these must be disregarded at present. But the fact that the calculations place the cold organ so near the surface of the skin, and well above those strata which differ markedly in their physical and histological structure, makes these conditions seem less serious.

It seems unlikely that a cold spot near a set of capillaries could be lowered appreciably by a stimulus as small as has been found sufficient to arouse the cold body. Cylinder 0.08 mm. has been reported cold 7 times, and 0.10 mm. and lower 24 times. The papillae under these spots measure roughly 0.05 mm. in diam. They have a net work of capillaries in them. If a part of this stimulus goes to lower the temperature of the intervening tissues the remainder would seem wholly inadequate to lower the temperature of the cold body near the blood vessels. This is even more unlikely if the cold organ be of the bulb type which has blood vessels in it and around it;[29] or if the cold body lies below a plexus of blood vessels.

V. HISTOLOGICAL EXAMINATION OF THE SKIN

Stimulation of the cold spots after the biopsy operation had healed showed a return of the cold spot, after some time (roughly 60 days), in every case at the edge of the incision although the original spot was in the center, as accurately as the eye could see. The incisions were made by means of a two millimeter dermic punch. In every case a scar was left, in some cases an almost perfect circle.

In all but two of the biopsies, the spots for some time after healing, *i.e.* 15 to 30 days, were insensitive to cold at 0°C. One of the two exceptions, Biopsy II, was sensitive at this temperature before the blood clot had disappeared. There are now two spots along the edge of the scar, about one millimeter apart, from which it seems that there were two cold spots near together here, one remaining just beyond the edge of the incision, and the other removed, because the original cold spot lay between two tattoo spots, both of which appear in the sections. Of possible interest here is that this spot was unusually painful in the excision and that microscopic examination showed a large nerve trunk under it. The other of the two spots seemed to respond at 0°C as soon as healing was completed. This response was vague and uncertain, however, and opportunity to make further careful examination was lacking. Sixty days later (roughly) this spot was clearly located on the edge of the scar. From this it may be supposed that the doubtful cold sensation reported was an error. It is unfortunate that this cannot be stated more emphatically. The biopsy removed the epidermis with only a very small layer of the corium (about 17μ). The significance of this fact was not appreciated until after the return of the cold spot on the edge of the scar. When the spots return the limens are plainly higher but exact measurements have not been made. They seem to lower as time goes on but this again has not been measured.

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[29] A. Ruffini, *op. cit.*

In examining the slides made from these biopsies, especial efforts have been made to find end-bulbs. Not one has been discovered, except Meissner's corpuscles in Biopsies IV and V. Limitation on the significance of this should be placed in accordance with what has been said in Sections I and II. The localization of the cold spots is, however, the most accurate known to the writer. The small size of the stimulating instrument on the back of the hand makes accurate placement necessary. The elimination of unnecessarily strong stimuli makes stimulation from a distance unlikely while the elimination of fatigue factors tends to prevent them failing to appear where they are.[80]

Biopsy I is a good example; this was accurately localized after the tattoo point had been made; than a second tattoo point was inserted. The cold spot was found to be between these two spots and a little to one side. A preliminary shallow cut was made with the punch and a drawing of the circle and three points near the center made. The excision was then made and after fixation the two tattoo points could be seen and part of the preliminary circle, the final cut having been made slightly to one side of the preliminary marker. The two tattoo points and the cold spot remaining in the form of a roughly equilateral triangle one half millimeter on a side in the center of the 2 mm.

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[80] It may be suggested that possibly some of the unaccountable absence of marked cold spots on certain occasions is due to their having been fatigued by cold from nearby applications. Only two of the 36 spots studied by this method have ever failed to appear, so far as laboratory notes go. One responded only once. It was plainly inflamed from the tattoo mark. The other seemed to be dull. Both were dropped from further study.

circle. This biopsy was insensitive to cold for some time after complete healing and was deep enough to take out all or nearly all of the corium.

There is reason to believe that the surface spot was removed in every case but the depth varied from just below the epidermis in Biopsy III to probably the full depth of the corium in Biopsies I and IV. It is quite possible that Golgi-Màzzoni corpuscles may have lain at the bottom of the corium where Ruffini says they are found, and were not removed.

Conclusions

The evidence of these studies is against the Golgi-Màzzoni end-bulbs being the cold receptors. They have not been found in the hairy parts of the skin nor in the biopsies of the hairless parts, Biopsy II having been quite deep. If they are found mostly at the juncture of the corium and the tela subcutania, as Ruffini contends, they would be far too deep and covered with too many capillaries to receive the minimal cold stimuli. If they are found in the papillae as Dogiel reports they come under the same category as the Krause end-bulbs mentioned below.

No evidence was found as to the existence or non-existence of the Krause end bulbs in the dermic papillae. If they or Golgi-Màzzoni end-bulbs are in the papillae, there is quite a question whether they are not the same as the Meissner corpuscles. In any case they are deeper than the calculated positions of the cold bodies.

There is no evidence here to support Haggqvist's findings of smooth muscle fibers under the cold spots.

All the evidence points to the supposition that the cold receptors lie in stratum malpighi; some near the top and some deeper down; and that the difference in the limens is due mainly to the thickness of the overlying tissues. In the stratum malpighi there are two forms of nerve endings, the well-known free nerve endings discovered by Langerhans in 1868, and the so-called hederiform nerve endings of Ranvier. As the latter are not so well known, and as there is some reason to suppose that they may be the cold organ, a brief description of them, taken from Ruffini[31] follows.

The hederiform nerve endings are basket-like in character with the concavity turned towards the outer surface of the skin; the basket usually contains one of the prickle cells of the Malpighi layer somewhat modified in its histological characters. These cells were discovered by Merkel in 1875, called *Tastzellen* and thought to be peripheral nerve cells. But Ranvier in 1880, on the basis of more careful studies, concluded that they were epithelial cells having a special relation to these nerve endings. He called them hederiform terminations from their supposed resemblance to ivy.

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[31] Ruffini, *op. cit.*

But Dogiel with better methods found this description not wholly correct. Rather they are basket-like, composed of slender, varicose, non-medullated fibers interlacing to form a net work around the base of one of the prickle cells. These fibers arise from medullated fibers of the superficial plexus of the skin. Accepting this description as the most probable, Ruffini proposes that they be called the intra-epithelial baskets of Dogiel. They lie near the ducts of the sweat glands and are considered the most recent form of nerve endings in the vertebrate epidermis, being found only in mammals.

It is not unreasonable to suppose that the sense of cold with its cold organ appears for the first time with the warm-blooded animals. Since the free nerve endings of Langerhans seem to be the oldest form of nerve endings in the vertebrate epidermis and the hederiform endings are considered the latest, it seems not improbable that the latter are the cold organs. The relation of these nerve end-organs to the sweat gland ducts offers an opportunity to test this supposition.

STUDIES IN AFFECTIVE PSYCHOLOGY[1]

By Paul Thomas Young, University of Illinois

VI. Preferential Discrimination of the White Rat for Different Kinds of Grain[2]

In view of certain difficulties with the introspective study of affective processes[3] it would be desirable to eliminate the human form of verbal report. The following experiments are designed to suggest such a mode of approach through the field of animal behavior.

The study has been limited to the relative preference of the white rat for various kinds of grain. After preliminary experiments with various food substances, grain was selected because of certain advantages in experimentation. The individual kernel is a food unit of fairly constant size; it can be easily handled; it is clean; it remains presumably constant for a long period; it is a normal food substance of the rat, and grain can be easily obtained in unlimited amount.

Three male white rats of middle age were used in the experiments and these will be designated as X, Y, and Z. The animals were fed once a day at the close of the observations so that the degree of hunger would be approximately constant from day to day. The rats were maintained on a diet of bread and milk prior to the experiment and throughout the experimental period.

The experiments were made in the early afternoons during the spring of 1927.

Experiment I

Relative preference for corn (maize) and barley. A table, 100 x 140 cm., was covered with clean wrapping paper and in the center was placed a thin steel sheet, 7.5 x 10.5 cm., which will be designated as the feeding-plate. The steel plate was used because it could be easily cleaned.

In an actual experiment grains of corn (C) and barley (B) were arranged on the feeding-plate before the rat was placed on the table. The animal was then introduced at a predetermined

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[1] Continuation of the series, this Journal, 38, 1927, 157-193.

[2] The present investigation was made possible through the grant of a National Research Fellowship in the Biological Sciences, for which I should like to express sincere appreciation to the Fellowship Board of the National Research Council. I am also indebted to Professor Wolfgang Köhler, Director of the Psychological Institute in Berlin, for extending the facilities and courtesies of his laboratory.

[3] The difficulties are discussed in earlier parts of the present series: The 'trained' observer in affective psychology, and The logic of affective psychology, *op. cit.*, 175, 186.

point. This point was the center of the shorter table-edge at a distance of 70 cm. from the food. The experimenter stood quietly behind the entrance point and observed the behavior of the rat in relation to the grain.

At first the animal explored a great deal around the edges and upon the surface of the table and discovered the food more or less accidentally, but with repeated trials he soon learned to go directly to the feeding-plate.

The manner of eating was observed. The rat would ordinarily take a grain of corn in his paws, gnaw and nibble part of it, then chew and swallow, then gnaw some more, etc. The outer portion of the C was apparently harder to gnaw than the central kernel and the complete eating of a grain of C was often a matter of minutes. Barley was eaten more easily and quickly.

Preliminary observations were made with a single grain of C and one of B presented simultaneously on the feeding-plate.

It was observed that all three of the rats would pick up the C, eat part of it, drop it, eat B and then complete the eating of C. This gave the impresssion that B was preferred to C since B was eaten before C. More careful observation, however, showed that it was always the central part of the C—the kernel containing the germ—which was eaten before the hard outer portions. Consequently it seemed necessary to regard the C as composed of two parts, a central kernel (k) and the peripheral portion (p). What is the order of preference among k, p, B?

To answer this question three Cs and three Bs were placed symmetrically and alternately about the central point of the feeding-plate. The tips of the six grains were pointed inward and all were as close together as possible.

There was a good deal of irrelevant behavior such as exploration, sniffing, listening, preening, etc. All three of the rats would at times carry the grain to a far corner of the table before eating and would later run back to the feeding-plate for more grain. The interesting point in this connection is that the three rats ran to the same corner which was away from the light and away from the experimenter. If we ignore this more or less irrelevant behavior and consider only the actual order in which the grains were eaten, we have the following result. The order of symbols corresponds to the order in which the food substances were eaten by each of the three rats on four successive experimental days.

Rat X: . k k B B k B p p p
k B k k B B p p p
k k B k B (p) (p) B p p (p)
k k k B B B p p p

Rat Y: k k k B B B p p p
k k k B B (p) B p p p
k k k B B p p (p) (one B lost to rat)
k k k B B (p) B p p p

Rat Z: k k k B B B p p (p)
k k k B B B p p p
k k k B B p p p B (final B lost, found later)
k k k B B B p p p

The symbol (p) in parenthesis indicates that the rat nibbled on the peripheral part of a C without completely eating it. The same symbol without parenthesis indicates that the rat completed the eating of the grain.

The results show a uniform and unmistakable preference in the order k B p. Not only are the three rats strikingly similar in their performance but also this uniformity persists from day to day.

Observation showed that chance factors play some part in determining the order of eating. The hungry rat did not hesitate and deliberate in the matter of eating. He appeared to run headlong at the grains or into them and to eat first the grain that first came under his nose. Such contact with the grains was apt to scatter them in an unpredictable manner; twice a grain of B was lost to the rat. When we remember that there was a good deal of exploring and other irrelevant behavior and, further, that the angle of approach to the food was by no means constant the above results are rather remarkable for their uniformity.

EXPERIMENT II

Preference between wheat and oats. The feeding-plate (P, Fig. 1) was moved from the center of the table to the position indicated. In the preceding experiment the rats frequently carried the grains from the center to this corner, and we thought that by moving the feeding-plate observation could be confined to a single part of the table. The surface of the table was illuminated by a 100-watt lamp (L) located on the ceiling 1½ m. from the feeding-plate.

In order to control the line of approach to the feeding-plate a heavy screen (S), 50 x 50 cm., was placed as indicated to the right of the feeding-plate. The rat was always placed on the table at the point (R). At first there was much exploration around the edges and corners of the table, climbing of the partition, and other irrelevant behavior; but with habituation these activities became less frequent until finally the hungry rat would run directly from R to P over a course indicated by the broken line. The experimenter at E could readily observe the behavior of the rat when approaching and eating the grains.

Preliminary experiments, for purposes of habituation, were made with corn and barley. Incidentally the observations confirm the previous experiment.

The general procedure was to place the rat at the point R and to observe until he had eaten the grains. The rat was then transferred to another table. The plate was cleaned and grains arranged for the second test, etc. In the present experiment grains were presented two at a time—a grain of wheat (W) at the right and a grain of oats (O) at the left, and on the next trial W at the left and O at the right. The grains were arranged in the center of the feeding-plate to the right and left of the experimenter (not to the right and left of the rat). The W and O were placed side by side so that the two grains actually touched each other.

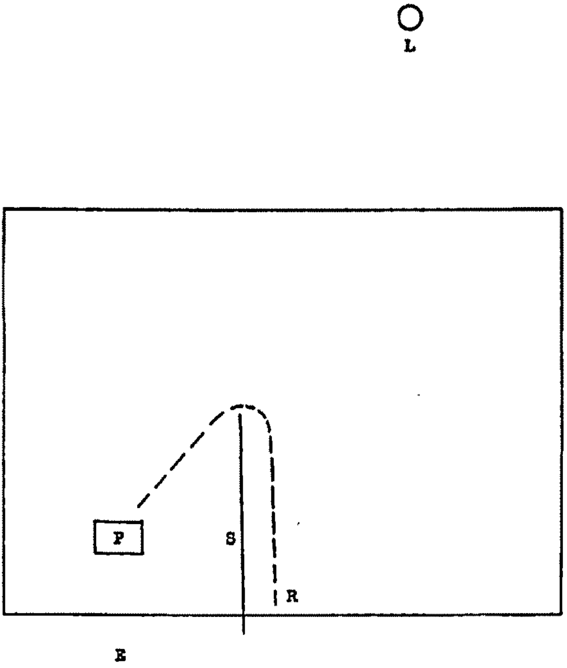

FIG. 1

After 2 days for habituation the series summarized in Table I were taken. Each rat made 10 discriminations per day for 3 successive days. The columns on the left show the spatial arrangement of the grains with respect to the experimenter; right and left were alternated throughout the series. Both grains were always completely eaten; the table has been simplified by recording in every case the *first* grain eaten.

To interpret Table I the reader should follow down the columns of a particular rat for the 3 successive days. Rat X on the first day ate W prior to O in 7 out of 10 trials; on the second day he ate W first 8 out of 10 trials; on the third day W was first eaten in every trial. The increase of frequency in favor of W is interesting. When O was the first grain eaten the O was situated at the experimenter's right. Rat Y quite regularly ate first the grain on the right. In the 30 trials there are but 3 exceptions to the rule and these exceptions favor W as against O. The results for rat Z are puzzling. On the first day O was first eaten 6 out of 10 trials.[4] On the second day W was taken first in every case. On the third day Z took first the grain at the right in every case but one.

TABLE I

RELATIVE PREFERENCE FOR WHEAT AND OATS

Trial	Presented		First grain eaten								
			Day 1			Day 2			Day 3		
	Left	Right	X	Y	Z	X	Y	Z	X	Y	Z
1	W	O	W	O	W	W*	O	W*	W*	W*	O
2	O	W	W*	W	O	W	W	W	W	W	W
3	W	O	O	O	O	W	O	W*	W	O	O
4	O	W	W	W	O	W	W	W	W	W	W
5	W	O	O	O	O	O	W*	W*	W*	W	O
6	O	W	W	W	W	W	W	W	W	W	W
7	W	O	W	O	W	W*	O	W	W*	O	O
8	O	W	W	W	O	W	W	W	W	W	O
9	W	O	O	O	O	O	O	W*	W*	O	O
10	O	W	W	W	W	W	W	W	W	W	W

*In these cases the rat picked up O, dropped it, then ate W and afterwards returned to O.

On the day following the experiments which are recorded in Table I a test was made with a series of 25 trials per rat. These additional data are similar in every respect to those presented in Table I. Taking all the results together, there is clearly a tendency to eat first the grain at the right:

	Rat			
Grain first eaten	X	Y	Z	Totals
Right	37	50	42	129
Left	18	5	13	36

Observation of the rat's behavior indicated that in approaching the feeding-plate the grain at the right was generally encountered before the grain at the left and, further, that there was a marked tendency to eat first the grain first met. It should be noted that, owing to irregularity in the rat's approach and to exploration, the grain at the left was sometimes encountered before the grain at the right, and for this reason the above figures are not an exact index of the tendency to eat first the first grain found.

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[4] On this day the rat explored a good deal and approached the feeding-plate from various angles; it is doubtful whether Z's data on this day indicate any preference at all. Later experiments indicate that with Z the size of the grain is a determining factor and in the present case O is the larger grain.

When a hungry rat finds a grain and eats it the performance has no relation to preference in the true sense of the word. To say that the rat *prefers* the grain at the right is meaningless since from the rat's standpoint the grain at the left may be practically non-existent. The performance is a function of one grain and not of two. The hungry rat explores, finds food and eats what is first found. There is no preferential discrimination.

But the distribution in Table I does indicate very definitely a preference of W to O. Of greatest significance are those cases marked with an asterisk. In 12 of these 13 cases the rat approached the grain normally, met the O at the right, picked it up as if to eat, but then dropped the grain, took up the W and ate it. After W had been eaten the rejected O was again taken and this time completely eaten. These cases are definite exceptions to the rule 'first found, first eaten.' In 13 out of 90 trials O was actively rejected until W had been eaten, and there is not a single case in which W was rejected to eat O!

The true preference of W to O is further shown by those series in which W was first eaten in every trial. See rat Z, day 2; and rat X, day 3. The consistent eating of W prior to O for an entire series, regardless of the spatial position of the grains, may be taken as a mark of preference.

Preference of W to O may be shown by a count of the number of times each grain was first eaten. Such a count gives the following:

	Rat			
First eaten	X	Y	Z	Totals
Oats	10	25	25	60
Wheat	45	30	30	105

On mere chance the frequencies for O and W would be equal. The departure from equality may be regarded as a rough index of preference. From this standpoint all of the rats prefer W to O, but the preference is more apparent in X than in Y and Z. A statistical count tends to cover up the fact that the rats sometimes make preferential discriminations and sometimes express no preference at all.

EXPERIMENT III

Factor of space. In the preceding experiment the path taken by the animal in approaching the food (Fig. 1) favored the discovery of the grain at the right prior to that at the left. Since a rat tends to eat immediately the first grain found, it is clear that the grain at the right will be eaten more frequently than the grain at the left. The so-called 'space error' is well known in experiments upon sensory discrimination.[5]

The apparatus diagramed in Fig. 2 was designed to eliminate the 'space error.'

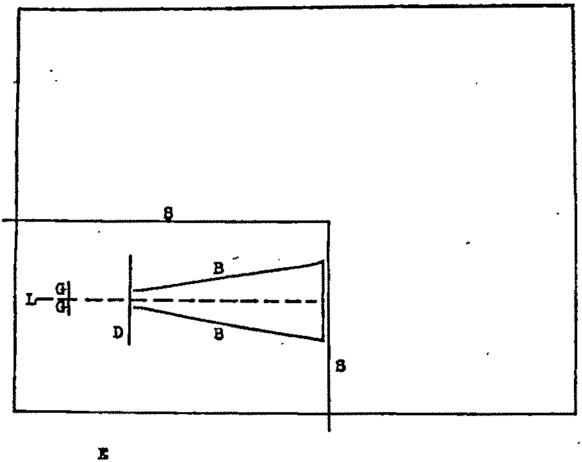

FIG. 2

One corner of the table was isolated by means of screens (S) arranged to cut off direct light and to limit the amount of possible exploration. Within this area a funnel-shaped cage was built. The cage was made of two boards (B), 14 cm. high and 44 cm. long, permanently closed at the large end by a similar board, 18 cm. in length. The small end of the cage was an opening 3.5 cm. wide, just wide enough to let the rat pass in and out, and this end could be opened and closed by means of a board (D) which served as a door. The entire floor of the apparatus was covered with a thick white card. Upon this card and precisely in the central axis of the cage a line (L) was drawn extending 20 cm. to the front of the door. At a point 17 cm. in front of the narrow opening this line was crossed by a short perpendicular line. Grains were placed in positions determined by the intersection of these lines (G). The grains were thus placed directly in front of the door, at a constant distance from the exit, and to the rat's (not the experimenter's) right and left. The cage was covered with glass to aid observation of the rat when inside.

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[5] If the sensory discrimination methods be compared with the present one, there is an outstanding difference. The aim of the former is to train an animal to make a particular discrimination; rewards and punishments are used. The aim of the present method is to discover the natural inclinations and disinclinations, the cravings and aversions of an animal quite apart from training.

The experimental procedure was as follows. The rat was first placed in the cage (sometimes he went in of his own accord) and the door was put in place. The card was then cleaned of debris and the two grains were placed to the right and left of the central line immediately behind the cross-line. The grains were touching each other. The door was then quickly removed and the observation commenced.

At first it was necessary to let the rat explore and become thoroughly habituated to the apparatus. Preliminary trials were made for habituation. When the experiment was under way the rat would come through the narrow door and directly forward to the grain. The experimenter observed and recorded the rat's behavior with respect to the grain.

TABLE II

RELATIVE PREFERENCE OF WHEAT AND OATS

Trial	Presented		First grain eaten								
			Day 1			Day 2			Day 3		
	Left	Right	X	Y	Z	X	Y	Z	X	Y	Z
1	O	W	W	O	W	W	W	W	W	W	W
2	W	O	W	W	O	W	O	O	W	O	W
3	O	W	W	O	W	W	W	W	W	W	W
4	W	O	W	W	O	W	W	O	W	W	W
5	O	W	W	O	W	W	W	W	W	W	W
6	W	O	O	W	W	W	W	O	W	W	W
7	O	W	W	W	W	W	W	W	W	W	W
8	W	O	W	W	O	W	W	O	W	O	W
9	O	W	W	O	W	W	W	W	O	W	O
10	W	O	W	W	W	W	W	O	W	O	W
11	O	W	O	W	W	W	W	W	W	W	O
12	W	O	W	W	O	W	W	O	W	W	W
13	O	W	W	W	W	W	W	W	W	O	O
14	W	O	O	W	O	W	W	W	W	W	W
15	O	W	W	W	W	W	W	W	W	W	O
16	W	O	O	W	O	W	W	O	W	W	W
17	O	W	W	W	W	W	O	W	W	W	O
18	W	O	W	W	O	W	W	W	W	W	W
19	O	W	O	W	W	W	W	W	W	O	W
20	W	O	W	W	W	W	W	W	W	W	W
21	O	W	W	O	O	W	W	W	W	W	W
22	W	O	W	W	O	W	W	W	W	W	W
23	O	W	W	O	W	W	W	W	W	O	W
24	W	O	O	W	W	W	W	W	W	W	W
25	O	W	O	W	W	W	W	O	W	W	O

The main experiment was made with wheat (W) and oats (O). There were 25 trials per day per rat for three successive days. The results are presented in Table II. The terms 'right' and 'left,' as above noted, refer to the rat's right and left and not to the experimenter's right and left as in Experiment II.

The table reveals an unmistakable preference of W to O. On day 2 rat X ate W first in every one of the 25 trials and on day 3 he ate W first in every trial but one! The preference is not so pronounced with Y and Z as in the case of X, but both Y and Z have long series of successive trials in which W was regularly eaten before O. Consider, for example, Y, day 1, trials 10–20; day 2, 3–16 and 18–25; and for Z consider day 2, trials 17–24; day 3, 1–8 and 18-24.

A comparison of the present experiment with Experiment II is instructive. In both experiments the results agree in expressing a uniform preference of W to O and in both experiments this preference is more clearly expressed by X than by Y and Z. A count of frequencies gives the following result:

	Rat			
First eaten	X	Y	Z	Totals
Oats	8	14	23	45
Wheat	67	61	52	180

In other terms W was preferred in 80% of the total cases and in Experiment II, W was preferred in 63.7% of the total cases. In considering these percentages it should be remembered that mere chance would give 50%. It is likely that the change of apparatus, rather than any change in the rats themselves, is responsible for the above increase of percentage.

One other comparison between Experiments II and III is interesting. In Table I certain trials are marked with an asterisk to indicate that the rat picked up O as if to eat the grain and then rejected it, eating first W and then O. In Table II these cases are not indicated. Rat X did this once or twice but with Y and Z there is no mention of this type of behavior in the record. Probably this relative lack of positive rejection is one result of the elimination of the pronounced space factor which was present in Experiment II.

There were cases in which O was touched by the nose but immediately passed over and left until W had been eaten. This is perhaps a form of passive rejection. Several times both grains were picked up together and it was difficult to observe what happened; apparently sometimes O and sometimes W was first eaten in these cases. When this occurred the observation was repeated at the close of the series.

The relative preference for W is shown by long series of successive trials in which W was first eaten regardless of the position of the grain. Such series are lacking for O; the Os occur sporadically. There are only two cases (X, day 1, 24–25; Z, day 1, 21–22) in which two Os occur successively.

An examination of the series in Table II shows that 'preference' for W and O may alternate on successive trials. This is especially marked with rat Z and present to a less extent with rats Y and X. Such alternations very strongly suggest that despite the apparatus some spatial principle of selection is involved. A careful study of the data, however, shows that the rats do not consistently take either the grain at the left or the grain at the right. Rat Z, for example, on day 2 takes first the grain on the right for trials 1 to 13; but on day 3 this same rat takes the grain on the left with equal consistency for trials 8 to 18. It looks as if there were a temporary set of conditions favoring the right or the left. There is no evidence for a permanent 'space error.'

To account for this temporary 'space error' is another matter. I observed that rat Z sometimes turned his head to the right on leaving the cage and approached the food with a head movement swinging towards the left which favored the grain at the right. A temporary habituation to some particular manner of approach would account for the results. Possibly visual stimulation played some part in determining the approach? The screen to the right was black and the wall to the left was relatively much lighter. Possibly the presence of the experimenter on the rat's left was a determining condition? Possibly the explanation of the temporary 'space error' lies wholly within the organism?

The experiment may be summed up by the statement that a reduction of the 'space error' brings to clearer light the consistent and uniform preference of W to O.

EXPERIMENT IV

Size of grain as a condition of preferential discrimination. In the foregoing experiments the grains were of approximately uniform size; unusually small and unusually large grains were rejected. To determine whether size of grain is a significant condition of preferential discrimination the largest and smallest grains of wheat and oats were selected and presented under the conditions of Experiment III. The large grains of W were approximately 6 mm. long by 3 mm. broad; small W were 4.5 to 5 mm. long by about 1.5 mm. broad. Large oats were 12 to 13 mm. long by 2 to 3 mm. broad; small oats were 8 to 9 mm. by 1.5 to 2 mm. broad. To accentuate the factor of size large W and small O were presented together; and then small W were presented with large O.

The plan of the experiment is indicated by the following summary:

	Trials 1-6	Trials 7-19	Trials 20-25
First day	Large W and small O	Small W and large O	Large W and small O
Second day	Small W and large O	Large W and small O	Small W and large O

There were thus 25 comparisons of large W with small O and 25 comparisons of small W with large O. The following summary gives the number of times out of 25 that wheat was eaten prior to oats:

Grain	Rat X	Rat Y	Rat Z
Large W and small O	24	18	20
Small W and large O	19	20	9

The data for X and Y indicate that size of grain makes very little difference in the result. The preference of W to O persists regardless of the relative size of the grains, so far as the present data go. The case of Z needs special comment.

At first sight it looks as though Z 'prefers' the larger grain. On the first day he showed a distinct tendency to take first the larger O. Trials 7 to 19 on this day give the following 'preferences:' O W O O O O O O O W O O O. Unfortunately the behavior of Z at this time was distinctly abnormal. Laboratory notes on this day record the following: "Z is unusually fearful and cautious to-day; he is sluggish in coming out of the cage to the feeding-plate. When he does approach the grain he stretches slowly forward towards the feeding-plate keeping his hind legs and tail inside the cage. Then with a quick dart he snatches up a grain, draws back into the cage and eats it. When Z is outside the cage he is unusually restless, explores a good deal and once attempted to bite the experimenter." Since this was the only day of the experiment on which Z acted in an unusual manner, we may conclude that there was probably some abnormal physiological condition. Z was apparently less discriminative than normal and more apt to eat first the grain which first came under his nose, *i.e.* the larger grain. It will be recalled that in Experiment II, Z did not show a very marked preference of W to O (see Table I, rat Z, day 1). With Z the size of the grain is probably a factor which may determine the order of eating the grains. The order of eating does not always indicate that the rat is making a preferential discrimination.

The uniform preference of W to O, which was found in Experiments II and III, is a preference for the *smaller* of two grains. This result suggests that the preferential discrimination rests upon some factor other than size. The basis is probably olfactory.

With X the size of grain may possibly be a significant condition of the order of eating, but the figures are not convincing.[6] If size were an unmistakable condition of preference, this could still be explained on an olfactory basis. The large grains of W probably gave more intensive olfactory stimulation than the small since the latter were sometimes dried and wrinkled.

In general, we may conclude that the preference of W to O is relatively independent of the size of grain. There is evidence that the size of grain may determine the order of eating in a purely mechanical, non-preferential manner, and this is more noticeable with Z than with X and Y.

EXPERIMENT V

Condition of grain in relation to preference. The apparatus diagramed in Fig. 2 presented one difficulty: the rat of his own accord would not go quickly into the cage but would frequently stay outside to explore. This necessitated a good deal of handling of the animal and loss of time. Putting the rat inside occasionally met with resistance from the animal. Consequently a new apparatus was built to eliminate the constant handling of the animals and to cut down the possibility of exploration.

A triangular cage, Fig. 3, 36 cm. at the base and 58 cm. from tip to base, was provided with a floor of stiff white card-board. The walls of the cage were 14 cm. high. Near the apex a partition (P) divided the cage into two compartments, a large one for the rat and a small one for the food. The partition could be raised or dropped thus permitting the rat to enter the food compartment or confining him within the other compartment. The top of the cage behind this partition was covered with glass through which the behavior of the rat could be observed. The grains were placed in the small end to the right and left of a line bisecting the angle at the apex. A perpendicular cross-line, 7 cm. from the tip, indicated a constant position for the grains. To facilitate cleaning and arranging the grains part of one side (B) was removable. The base (A) was also removable so that the rat could be introduced at the large end. The cage was placed with its tip away from the light to provide equal illumination for the right and left sides.

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[6] Götz found that hens have a tendency to peck at the larger grains. W. Götz, Experimentelle Untersuchungen zum Problem der Sehgrössenkonstanz beim Haushuhn, *Zsch. f. Psychol.*, 99, 1926, 247-260.

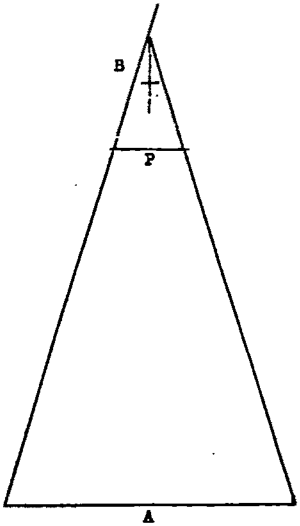

FIG. 3

In an actual experiment the procedure was as follows: The partition (P) was closed and the rat introduced by removing the base (A) which was then firmly fastened with the rat inside. The wall (B) was removed, the floor cleaned and the two grains arranged. The grains were placed to the right and left of the central line, back of the perpendicular cross-line; the grains touched each other. After the grains had been arranged the wall board (B) was slid back into place. The partition (P) was then raised and the observation commenced. The rat approached the grain at the apex; his performance was observed and recorded. Sometimes the rat attempted to climb out at the narrow end and in this case he was always knocked back. When the rat withdrew from the narrow end of the cage the partition was dropped and the grains arranged for the next experiment.

Problem. A grain of wheat is taken up by the rat and eaten without any preparation but the case is different with a grain of oats. The rat holds the oat in his paws, removes the outer husk and eats only the inside kernel, leaving the husk to be cleared away by the experimenter. Eating an oat, therefore, involves a certain amount of preparatory activity which is absent with wheat. Does this necessity of preparing the grain prior to eating have any relation to the preferential order of wheat to oat?

To test the matter two kinds of oats were used: (1) prepared oats (P) in which the outer husk had been carefully removed with a knife; and (2) unprepared, normal oats (O) of the type used in previous experiments. P was smaller than O and to the human eye had a totally different appearance.

Prior to the main experiment an habituation series of 10 trials was given with wheat and oats. Incidentally, the preliminary trials confirm the previous experiments with W and O. The main experiment was commenced on the following day.

The plan of the experiment is indicated by the following summary:

Day	Trials 1–6	Trials 7–19	Trials 20–25
First	W and O	W and P	W and O
Second	W and P	W and O	W and P

There were thus 25 comparisons of W with O and 25 comparisons of W with P. The following summary gives the number of times out of 25 that O was first eaten and that P was first eaten:

	Rat			
First eaten	X	Y	Z	Totals
Normal Oats	2	4	5	11
Prepared Oats	7	9	6	22

These figures indicate that the prepared oat is slightly preferred to the unprepared oat. The figures also indicate that the preference of W to O persists regardless of the condition of the O.

TABLE III

CONDITION OF GRAIN AS A DETERMINANT OF PREFERENCE

Trial	Presented		First grain eaten					
			Day 1			Day 2		
	Left	Right	X	Y	Z	X	Y	Z
1	P	O	P	P	O	O	O	O
2	O	P	P	O	O	P	P	O
3	P	O	O	O	P	P	O	P
4	O	P	P	P	O	P	P	O
5	P	O	O	P	O	P	O	P
6	O	P	P	P	P	P	P	O
7	P	O	P	P	O	P	P	P
8	O	P	P	P	O	P	O	O
9	P	O	O	P	P	P	P	O
10	O	P	P	P	P	P	O	O

On the two days following the above experiment, series of 10 trials were taken in which P and O were presented simultaneously. The results are presented in Table III. Rat X shows a distinct preference of P to O. Rat Y prefers P to O on day 1, but on day 2 the outcome is ambiguous. This result reinforces the above.

Once again the results for Z are puzzling. Z appears to favor O to P! It will be recalled (Experiment IV) that the factor of size is relatively more important for Z than for X and Y. Possibly mere size as given visually or mechanically is a determinant of the order of eating? And in this case it is a question as to whether the term 'preference' is applicable.

With X and Y size of grain is not important. The prepared oat is smaller than the unprepared. Possibly the basis of discrimination is olfactory, and removal of the husk brings more intense and definite olfactory stimulation.

In general, we may conclude that the preference of W to O is relatively independent of condition of the O—whether husked or unhusked. The experiment presents evidence that P is preferred to O; and that with one rat (Z) size of O may possibly be a prior condition of eating.

EXPERIMENT VI

The serial principle. The serial principle may be stated as follows: if *A* is preferred to *B*, and if *B* is preferred to *C*, then (if the serial principle is valid) *A* will be preferred to *C*. This principle sounds very simple but its validity may not be assumed without a test.

Experiment I revealed a preference which was expressed symbolically as k B p. The other experiments revealed a second sequence: W O. What is the relation between these two series? Is the principle of serial order valid? In order to answer these questions it was necessary to make cross-comparisons between corn and wheat and between barley and oats.

(A) *Preference between corn and wheat.* Table IV presents the results of the rat's preferences between corn and wheat. The symbol (C) indicates that the kernel (k) of the corn—and in a few cases the entire grain—was eaten prior to the wheat (W). Whenever possible the peripheral part of the corn was removed by the experimenter to save time and to avoid over-feeding. The symbol (W) indicates that the grain of wheat was completely eaten before the corn was touched.

Rat X prefers W to C; the preference is not certainly expressed on day 1 but is clearly established on days 2 and 3. Similarly the outcome for Y is uncertain on day 1, whereas days 2 and 3 express an unmistakable preference for W.

Once again the performance of Z needs special discussion. On day 1 there is a 'preference' for C—the larger grain—to W. On day 2 there is a decided swing towards W. On day 3 no preference at all is shown—the grain at the left being taken in every trial. (See footnote 8).

The results for all three of the rats suggest that a preference for W may be developing. Possibly at the start there is little preference but with repetition the rat becomes habituated to a certain preferential order. It is to be regretted that the grain supply gave out after 50 trials and that the matter could not be decided. A new supply of grain might not be comparable with the old.

The experiment shows a decided tendency for the rat to take first the grain at the left. Of the 150 trials recorded in Table IV the left grain was taken first 104 times and the right grain 46 times. Observation gave the explanation of this tendency. When the partition (P in Fig. 3) was raised the rat usually approached the food along the wall on his right. Since the grains were placed well towards the tip of the triangle it happened that this type of approach brought the rat's nose first to the grain at the left.

Why the three rats approached along the wall to their right is another problem. Possibly there was a small difference in illumination. Possibly the presence of the experimenter near the left wall was a factor. Possibly a factor within the rats (corresponding to right-handedness in man) may account for the result.

We are well aware of the general rule 'first found, first eaten.' The present apparatus favors the position at the rat's left and it is therefore a failure. The arrangement shown in Fig. 2 is better than that shown in Fig. 3.

This rule 'first found, first eaten,' explains the performances of Z, if we assume that the larger grain is apt to be discovered before the smaller on purely mechanical grounds.

In general, X and Y clearly prefer W to C. With Z the case is ambiguous and the ambiguity rests in part upon Z's tendency to take first the larger grain, and in part upon a 'space factor' introduced by the apparatus.

TABLE IV

RELATIVE PREFERENCE OF CORN AND WHEAT

Trial	Presented		First grain eaten								
			Day 1			Day 2			Day 3		
	Left	Right	X	Y	Z	X	Y	Z	X	Y	Z
1	W	C	W	W	C	W	W	W	W	W	W
2	C	W	C	C	C	C	C	C	W	W	C
3	W	C	W	C	C	W	W	C	W	W	W
4	C	W	C	C	C	W	C	W	W	W	C
5	W	C	W	W	C	C	W	W	W	W	W
6	C	W	W	C	C	W	C	W	W	W	C
7	W	C	C	W	C	W	W	W	W	W	W
8	C	W	W	C	C	C	W	C	W	W	C
9	W	C	W	W	W	W	W	W	W	W	W
10	C	W	W	C	W	W	W	C	C	W	C
11	W	C	W	W	C	W	W	C			
12	C	W	W	W	W	W	W	C			
13	W	C	C	W	C	W	W	W			
14	C	W	C	C	C	W	W	C			
15	W	C	W	W	W	W	W	W			
16	C	W	C	C	C	C	W	C			
17	W	C	W	W	C	W	W	W			
18	C	W	C	C	C	W	W	W			
19	W	C	W	W	C	W	W	W			
20	C	W	C	C	C	W	C	C			

(B) *Preference between barley and oats.* We decided not to change the apparatus in the middle of the experiment and so continued for four successive days, substituting barley (B) and oats (O) for corn and wheat. There were 65 trials per rat (10 on day 1, 25 on day 2, 15 on day 3, 15 on day 4).

Rat X revealed a distinct preference of B to O. Rats Y and Z showed no clear-cut preference although the frequency of O is slightly greater than that of B. A count of frequencies gives the following:

	Rat			
First eaten	X	Y	Z	Totals
Barley	45	31	30	106
Oats	20	34	35	89

The results for Y show a curious wavering between B and O which is only in part dependent upon the spatial arrangement of the grains. For example, on day 2 the series of grains *presented* at the left was B O B O B O . . . and the first grain eaten was B O B O O B O O O O O O B B O O O O B B B B O B B. This type of variation may indicate approximate equality as regards preference between B and O.

The factor of space may be indicated as follows:

	Rat			
Grain first eaten	X	Y	Z	Totals
Left	37	45	58	140
Right	28	20	7	55

In general, X shows preference of B to O; the results from Y and Z are ambiguous. The ambiguity rests upon inability to distinguish between (1) equality of preference (2) failure to make preferential discrimination. A 'space error' exists.

(C) *Test of the serial principle.* Bringing together the results of previous experiments we have the following summary:

	X	Y	Z	Experiment
(1)	k > B	k > B	k > B	I
(2)	W > O	W > O	W > O	II–III
(3)	W > k	W > k	(k > W)	VI–A
(4)	B > O	(O > B)	(O > B)	VI–B

Hence:

(5) W>k>B>O W>k>(O>B) (k>W)>(O>B)

Row (1) summarizes the result of Experiment I. All of the rats perferred k to B; this preference was unambiguous in every case. Row (2) summarizes the result of Experiments II and III. Here again the three rats agree. All of the rats preferred W to O, but they differed in the *degree* to which they showed this preference. The preference was most pronounced with X, less marked with Y, and still less with Z. In rows (3) and (4) certain pairs are enclosed in parentheses. This is to indicate some uncertainty—either approximate equality of preference or failure to make preferential discriminations. Very probably these uncertainties depend upon the inadequate type of apparatus used in Experiment VI together with a small number of trials, and I believe that further experiments under better conditions would bring about uniformity.

Row (5) shows that for every rat the data can be combined into a single series so that every preferential sequence is valid. The serial principle appears to hold. It would be safe to add the term (p) at the lower end of every preferential series, thus giving a series of five terms.

The results all indicate that W and k are preferred to O and B but in the case of Y and Z there is uncertainty about the order of W and k, and about the order of O and B.

On the basis of the above results it was predicted that a comparison of wheat and barley would reveal a uniform preference for wheat. The matter was then tested experimentally. Table V records the result.

TABLE V

RELATIVE PREFERENCE OF WHEAT AND BARLEY

Trial	Presented		First eaten			Presented		First eaten		
	Left	Right	X	Y	Z	Left	Right	X	Y	Z
1	W	B	W	W	W	B	W	W	B	B
2	B	W	B	B	*W	W	B	W	W	W
3	W	B	W	W	W	B	W	W	W	B
4	B	W	W	B	B	W	B	W	W	W
5	W	B	W	W	W	B	W	B	B	B
6	B	W	W	W	B	W	B	W	W	W
7	W	B	W	B	W	B	W	B	W	W
8	B	W	W	W	W	W	B	W	W	W
9	W	B	W	W	W	B	W	W	B	W
10	B	W	W	B	W	W	B	W	W	W
11	W	B	W	W	W	B	W	B	B	B
12	B	W	B	W	W	W	B	W	W	W
13	W	B	W	W	W	B	W	W	W	W
14	B	W	W	W	B	W	B	W	W	W
15	W	B	W	W	W	B	W	W	B	B

*Rat Z picked up both grains together, dropped B and ate first W and later B.

There is no doubt about the fact that W is distinctly preferred to B. A count gives the following summary:

	Rat			
First eaten	X	Y	Z	Totals
Wheat	25	21	22	68
Barley	5	9	8	22

The possibility of prediction indicates that we are dealing in the present experiment with a genuine series of preferences.

DISCUSSION

The Criteria of Preference. One fact stands out clearly in the above experiments: the rat frequently fails to express any preference. He may consistently eat the grain at the right or the grain at the left; or he may eat the first grain he happens to meet. The hungry rat does not hesitate;[7] when he meets a food object he generally eats it without delay. The rule seems to be 'first found, first eaten.'

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[7] Three times for Y and twice for Z the writer observed a back-and-forth head movement from grain to grain prior to taking either which might be called 'hesitation.' This type of behavior is exceedingly rare since it was observed only five times in well over 1000 observations.

Now the order in which the grains are found by the rat depends upon the apparatus. One type of apparatus may favor the discovery of the grain at the left prior to that at the right; another type of apparatus might give the reverse favoring.

The larger of two grains is apt to be first found and first eaten. The larger of two grains is the more *obvious*—mechanically, visually, and possibly also to the nose. Size of grain plays an important part in the performances of Z, but the factor of size is less important with Y and X.

When spatial factors determine the order of eating two grains it is not possible to speak of true preferential discrimination. This type of performance is determined by one grain at a time and not by two simultaneously. It is as if one placed a series of grains a meter apart and then found that the rat ate them in the order met!

Alternation of the positions of grains on successive trials is an essential part of the methodology. This plan makes it possible to distinguish between true preferential discrimination and its absence. A long series of alternations in which the rat consistently takes first the grain at the right (or at the left) is pretty good evidence that the data express no preferential discrimination.

It is important to distinguish between series which express no preference and series which show equality of preference (see Experiment VI–B). A gross count may fail to make the distinction. Compare, for example, the two following series:

(1) A B A B A B A B A B
(2) A A A A A B B B B B

Both series have 5 As and 5 Bs; but the first probably indicates a failure to express preference whereas the second may well express equality of preference.

How then is true preference shown? In the above experiments the following criteria have been used:

(1) *Rejection of one grain for another.* With W and O, for example, there were cases (Table I) in which the animal first found O and picked it up as if to eat; he then dropped O and ate W; after this he again took O and ate it completely. This was also observed with C and B. The normal performance was to pick up the C and eat the kernel, then to drop the grain and eat the B, then to eat the peripheral portions of the C. These rejections were always in the same direction: O was rejected to eat W and never the reverse; p was rejected to eat B and never the reverse. The three rats were consistent and uniform in their rejections in so far as rejections occurred.

(2) *Passing by of one grain for another.* In numerous cases O was knocked aside, touched with the nose, smelled, but not

eaten until after W. This is perhaps a passive form of rejection. The rule 'first found, first eaten' breaks down completely for the grain which is first found is definitely passed by and the more remote grain is first eaten.

(3) *Desertion of partly eaten grain.* In Experiment I the rats frequently left p uneaten to explore, sniff, listen, scratch, preen, etc. This was not noticed with k and B. The rats were easily distracted during the eating of p. This may be taken as an indication of relative 'dislike' for p.

(4) *Consistent eating of one grain prior to another for a series of trials.* Despite the right-left alternations in the position of two grains a rat will sometimes consistently eat one kind of grain first. Rat X, for example, ate W prior to O in 49 out of 50 successive trials (Table II). This is perhaps the most reliable criterion of preference and it goes directly against the principle of 'first found, first eaten.'

(5) *Uniformity of preference.* Preferences are constant in direction from day to day and further the group as a whole are uniform in their relative preferences. These uniformities can not be explained as the working of chance factors. When uniformity is lacking[8] it is likely that further experiments would reveal the grounds for divergence.

The first three of the above criteria may be applied to the single experimental test, but rejection, passing by, and desertion of grain are only occasionally observed so these three criteria are not very serviceable. The last two criteria depend upon the series of tests as a whole and are the most reliable means of determining the direction of preference and of distinguishing preference from failure to express preference.

It is scarcely necessary to add that all of these criteria of preference point in the same direction. They would all agree, for example, in indicating a preference of W to O.

Apparatus requirements. In an ideal apparatus every factor tending to influence the animal's finding of the right prior to the left food object, or *vice versa*, would be eliminated or balanced. Elimination of the 'space error' is a most important consideration. The apparatus diagramed in Fig. 2 proved best in this respect and the apparatus diagramed in Fig. 3 was unsatisfactory. In the latter the rats approached the food along a wall and this favored the grain at the left. The food should be in the open and the approach should be an open one. Observation is also easier with open feeding.

The apparatus in Fig. 3 had some advantages over that shown in Fig. 2. The former eliminated the necessity of constantly handling the animals and reduced the possibility of exploration to a minimum thus saving time.

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[8] The data of Z (Table IV), for example, apparently show two tendencies. A tendency to take the larger, more obvious grain is opposed to what might be called the natural preference. In the comparison of C and B (Experiment I) size and natural preference operated in the same direction; but in the comparison of C and W these factors were probably opposed and consequently Z's performance reveals two incompatible tendencies.

An ideal apparatus should make it possible to control the approach and exit of the rat to the feeding-plate without constant handling of the animal. Also the possibility for exploration should be reduced to a minimum.

Fig. 4 suggests a type of apparatus designed to correct the faults of those used in the present experiments.

An open, circular feeding-plate (P), about 30 cm. in diam., is supported from beneath at a convenient height from the floor. If supported from the center the rats will not climb down. The food is placed in the center of the plate in slight indentations permanently provided for this purpose, or at marked positions. Ideally the two food objects should be of equal size and area. When bulky foods are used I would suggest placing the material in a hollow tube a few mm. in diameter. Two such tubes could be easily mounted in the center of the feeding-plate in such a way that simple rotation on a common shaft would reverse right and left positions.

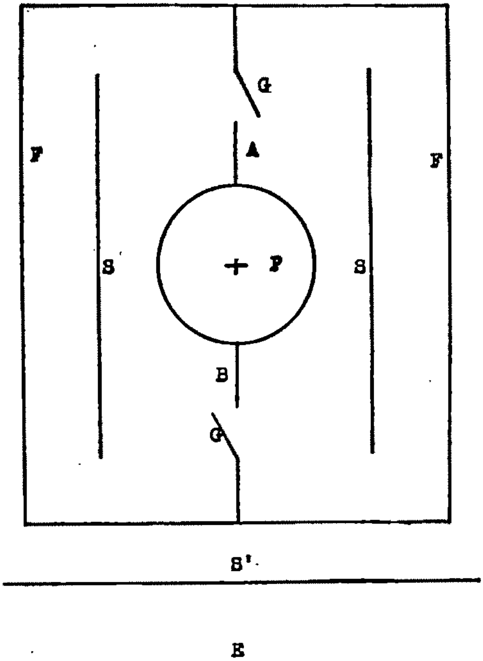

FIGURE 4

A strip (A), about 2 cm. in width, serves as an approach to the plate and a similar strip (B) serves as an exit. Two gates (G) regulate the approach and exit of the rat to the feeding-plate. These gates could be operated by the experimenter at (E) by a simple mechanical device. A frame (F) makes it possible for the rat to go from exit to approach and provides very little opportunity for exploration. Possibly this pathway could be made to pass directly under the feeding-plate. A single pathway to the right might bias the rat's approach.

To the right and left of the feeding-plate are two neutral gray screens (S) equally but not too brightly illuminated. The purpose of these screens is to give optical balance to the visual field and so to avoid possible head and body movements towards the right or left. A similar screen (S′) is placed between the rat and the experimenter at a height which does not interfere with observation, so as to give the rat a uniform visual field.

Since the position of the experimenter may possibly be a factor determining the rat's movements to the right or left, it is suggested that the experimenter be located directly in front of the approaching rat at E. Incidentally, this position would be best for observation.

With an apparatus such as this, any 'space error' would probably be due to conditions inside the rat himself.

Conditions of preference. It is important, as we have seen, to distinguish between true preference and non-preference. The former depends upon two groups of conditions; those outside and those inside the organism.

The character of the food substances is an essential condition of preference. We have assumed that our grain supply was constant throughout the experiments, but we do not assume that the result is valid for all W, all O, all B, all C. The age and variety of grain, the amount of moisture it contains, the character of the surface, etc. may well be factors determining preference.

The physiological conditions may be conveniently regarded as neural (including peripheral and central conditions) and chemical. The sensory basis of discrimination in the present case is probably olfactory. In non-preferential behavior it is likely that visual and tactual stimulation plays an important part.

When we consider the physiological conditions of preference from a broad chemical point of view, a whole world of problems lies open before us, and we realize the need and the possibility of an exact, quantitative, objective science of *psycho-chemistry*.

Some physiological questions which could be studied by the above methods are: How do food preferences vary with the degree of hunger? How does a specific deficiency in the diet affect food preferences? Does a specific deficiency create a specific craving? How, for example, does salt hunger vary with the amount of salt in the diet?[9] Do special hungers appear in disease,[10] during pregnancy, nursing, various phases of the sex cycle? How do preferences vary when this or that ductless gland is removed, when this or that drug is injected? Does the preference for foods adapt itself to the available food supply? Do animals learn to prefer this or that food to others when consistently fed with it over a long period of time? How do preferences vary with age? How do preferences vary with the temperature of the environment?[11] How do special cravings and desires for food develop? These and other questions may be answered by application of the methods developed in the above experiment.

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[9] See the discussion in J. L. Mursell, Contributions to the psychology of nutrition; I. Hunger and appetite, *Psychol. Rev.*, 32, 1925, 320 f. See also a paper by Green in which it is reported that phosphorus deficiency in the diet leads cattle in South Africa to the eating of bones (osteophagia). H. H. Green, Perverted appetites, *Physiol. Rev.*, 5, 1925, 336–348.

[10] It is common knowledge that dogs sometimes eat grass when sick and that grass is not a part of the dog's ordinary diet.

Other methods. Osborne and Mendel,[12] and Mitchell and Mendel,[13] offered rats and mice a free choice between a deficient and an adequate diet and found, in general, that the animals selected that diet which facilitated growth. In some animal-feeding experiments the conditions are so arranged that the animal has a free choice between different foods about as a human being has in a cafeteria. The weight of each substance eaten is determined and the variations of these weights under different conditions studied. The method may be valuable for experiments in nutrition but from the standpoint of psychology it is rather gross.

Washburn[14] refers to the preference method of Graber which was used to study sensory discriminations. She writes: "This was Graber's favorite mode of studying the effect of stimuli upon animals. Applied to olfactory stimuli it consisted in offering a choice between different compartments, containing each a different odor. The animal's power of discrimination was argued from the tendency to choose certain odors rather than others. Such preferences were shown by insects. The method, however, as was noted above, is unsatisfactory, because discrimination might exist where preference did not." This method, further, lacks a motive or drive which is essential in animal work.

Simmons[15] studied the effectiveness of bread-and-milk and sunflower seeds as incentives to learning, and found that the former produced quicker learning. Possibly we can argue from such a result to 'preference,' but the method would be too cumbersome for a detailed study of preferences.

Objectivity in the study of preferences. The term 'preference' has not been defined but it has been used in a descriptive sense. Preference means that an animal more or less consistently takes *A* before *B*. The term may be used in the same sense in human psychology.

The question whether the rat *feels* pleasantness when he eats the preferred grain has not been raised. I see no way to answer the question and apparently one's answer makes no difference in carrying out an experimental program.[16]

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[11] It is known that peoples living in the far north eat much fat. If an animal be maintained at a low temperature, does the relative preference for fats develop?

[12] T. B. Osborne and L. B. Mendel, The choice between adequate and inadequate diets, as made by rats, *J. Biol. Chem.*, 35, 1928, 19-27.

[13] H. S. Mitchell and L. B. Mendel, The choice between adequate and inadequate diet, as made by rats and mice, *Amer. J. Physiol.*, 58, 1921, 211-225. See also C.P. Stone, Some effects of inanition on animal behavior, *Psychol. Bull.*, 25, 1928, 13-18.

[14] M. F. Washburn, *The Animal Mind*, 1926, 50, 79.

[15] R. Simmons, The relative effectiveness of certain incentives in animal learning, *Comp. Psychol. Monog.*, 2, 1924 (No. 7). See also Washburn, *op. cit.*, 323 f., for studies of *drive* in animals.

[16] Even in human psychology the judgment of relative preference is ambiguous regarding the existence of felt pleasantness and unpleasantness. The problems of preferential discrimination and the problems of felt experience have been badly muddled in the so-called 'method of impression.' M. Yokoyama, The nature of the affective judgment in the method of paired comparisons, this JOURNAL, 32, 1921, 357-369.

CONCLUSION

The present experiment demonstrates that problems of preference which have been associated with affective psychology can be studied with animal subjects.

Three male white rats were found to be fairly uniform and consistent in their preferences for different kinds of grain. The degree in which preference is shown varies from rat to rat.

It is essential to distinguish between preferential discrimination and non-preference. The latter expresses itself in a tendency to eat first the first food substance encountered which may be the grain at the right or the grain at the left varying with the type of apparatus. The larger of two grains is apt to be eaten before the smaller, and this is especially noticeable with rat Z.

True preference is indicated in several ways and among them the following: (a) the consistent eating of one grain prior to another regardless of spatial arrangement and regardless of relative size; (b) the active or passive rejection of one grain for another; (c) abandonment of a (unpreferred) grain for exploration or other activity.

The present experiments show that the serial principle is valid. This principle may be stated as follows: If A is preferred to B, and if B is preferred to C, then A is preferred to C.

The aim of the study is methodological. The work shows that problems regarding food preference—and possibly also problems of like and dislike, aversion and craving—may be studied by an objective, quantitative, experimental method.

VII. CONFLICT OF MOVEMENT IN RELATION TO UNPLEASANT FEELING[17]

In every-day life conflict of impulse is generally an unpleasant experience, and the more fundamental and permanent conflicts of life are also associated with unpleasantness. When opposed movements are brought into conflict in the laboratory does unpleasant feeling occur?[18]

This question was studied by establishing relatively simple habit systems and then, without warning to the subjects, by bringing these habit systems into simultaneous opposition. The plan of the experiment may be illustrated schematicaly as follows:

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[17] The experiment to be reported was done in the fall of 1920 at the University of Minnesota. Publication has been delayed in the hope of extending the work; but to date nothing further has been done with the method. The original results are now presented as a small contribution to the methodology of affective psychology.

[18] According to a theoretical discussion by C. J. Herrick (*An Introduction to Neurology*, 1916, 257-258), conflict of movement impulses should regularly bring about unpleasant feeling.

A previous study by the writer (this JOURNAL, 32, 1921, 40 f.) showed that muscular strain was associated with unpleasant feeling and muscular relaxation with pleasant feeling. Muscular strain suggests conflict of movement impulses as a possible and occasional basis.

$$L \rightarrow K$$
$$K' \leftarrow L'$$

A forward kick of the foot (K) was associated with the flashing of an electric lamp (L); and a backward kick of the foot (K′) was associated with the flashing of another electric lamp (L′). Both reactions were thoroughly habituated. Then, without any warning to the *S*, L and L′ were flashed simultaneously.

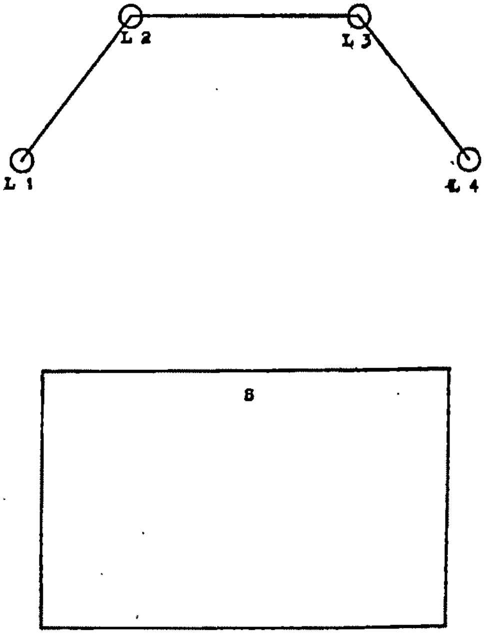

Figure 5

Apparatus and Method. The subject (*S*) sat upon a table so that both feet could swing freely. Directly in front of the *S* at the level of the head were four 2-c.p. electric lamps arranged as shown in Fig. 5.

S was instructed as follows: "On the frame before you are four lamps which are to be associated with movements of the feet. The lamps to the left indicate movement with the left foot and those to the right movement with the right foot. The two forward lamps indicate forward movement; the two lamps towards the rear indicate backward movement. Thus on the chart (*S* was given a diagram) L1 means left foot backward; L2 means left foot forward; L3 means right foot forward; L4 means right foot backward.

"Maintain a passive attitude towards the lights. When a light appears extend the foot as far as possible forward or backward and then relax it so that it comes to rest in the vertical position."

The experimental room was darkened and then dimly illuminated so that the signals would appear under constant lighting conditions.

The direction and extent of foot movement were recorded by means of a mechanical device attached to the shoes of the *S*. The time between signal and beginning of movement was automatically recorded on a stop-watch. Since the objective data reveal nothing of importance they have been ignored in the following report.

There were 4 *Ss*, two men (A and C) and two women (B and D) all connected with the psychology department at the University of Minnesota (1920-21). The *Ss* were kept in ignorance of the true aim of the experiment. They were led to believe that it dealt with the affective character of simple movement and to give this impression they were occasionally asked to report whether pleasant or unpleasant feelings were associated with foot and leg movements. After every critical test with conflicting signals an affective report was requested.

To give an affective setting the *Ss* were instructed to make affective judgments upon odors. There were four judgments prior to the movement series and four judgments after. The experiment with odors has been reported elsewhere.[19] It had an aim independent of the present experiment.

Every *S* gave 3 experimental periods a week to the work and the total work covered a period of 6 weeks.

Habituation series. Before the critical tests could be made it was necessary thoroughly to habituate the *Ss* to the reactions. Every *S* was given 10 trials per experimental day for a period of three to four weeks. The reactions were equally distributed among the four signals. A had 100 trials; B, 110; C, 100; D, 120. The signals were given without warning and in a haphazard order. The *Ss* more or less passively awaited the lights and, at the close of the practice period, responded in a seemingly automatic manner.

A moved his foot slowly and rather deliberately and then let it drop back to the vertical position. The other 3 *Ss* moved more quickly and automatically than A, but they too, let the foot drop back to the vertical.

Reports show that forward and backward movements of the foot are generally indifferent. Three *Ss* (B, C, D) report that forward movement is preferred to backward; A mentions no preference for direction of movement. At times, however, all of the *Ss* report weak Ps and Us associated with movement. As P, the *Ss* report that the movement itself was P, the freedom of movement was P, a relief from strain was P, the movement was automatic (hence P), expectation was satisfied. As U, the *Ss* report conflict of impulse, indecision, delay of waiting, making the wrong response, preparatory tension, blocking. Expectation may be followed by P or U according as the expected signal comes or fails. There are a good many cases of strain associated with U and of relaxation associated with P, and there is no record of a reverse association.

D proved to be moody. Some days when 'dead' and with 'no pep' practically every movement was reported as U.

Conflict series. At the close of the habituation period the *Ss* were responding mechanically to the signals. We determined, therefore, to introduce the critical tests. Without any warning to the *Ss* the two following series of lights were used on successive experimental days: (Series 1) 3, 4, 1, 2, (3 and 4), 2, 1, (1 and 2), 4, 2. (Series 2) 4, 1, (1 and 2), 3, 4, (1 and 2), 1, 4, (3 and 4), 2. The critical signals are the double lights (1 and 2) and (3 and 4).

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[19] Although the two experiments were independent in aim they went together very nicely. The olfactory experiment demanded a pause to offset olfactory adaptation; which pause was furnished by the movement series. The present experiment demanded a definite affective setting, which was given by the work with odors. The olfactory experiment has been described in *J. Exper. Psychol.*, 6, 1923, 183.

The entire control apparatus was concealed from the view of the *S*s and connections had been previously arranged so that by throwing a switch two lamps could be lighted simultaneously.

The results for each of the 4 *S*s are presented below. It should be remembered that the *S*s were thoroughly habituated to the signals and that no warning was given for the double signals.

Subject A. With the single lights the affective report was of indifference except several times active movement was reported as P. No U was reported with the single lights.

The reports for the critical, double-light, tests are: "An unusual situation, slightly U. It was difficult to know how to react. The inhibition was U." "Slightly U, bewildering. I made no movement at all." "The same again, bewildering. First I saw light 2 and almost reacted to it; then I saw 1. This catching of myself has an element of U in it." "No report." "Not the slightest movement. Slightly U."

Thus it is clear that with A, conflict was induced by the double signals and that the affective report was consistently U. Under conflicting stimulation the foot showed slight alternating back and forth movements.

Subject B. The single-light trials in both series were reported as affectively indifferent.

The double signals gave the following reports: "There was a moment of doubt—whatever that is. Then a certain P experience because it was in the nature of a joke. There was a relaxation of the facial muscles in smiling (*S* was seen to laugh slightly)." "Absolutely indifferent. I saw two lights but there was no tendency to move." "No movement (*S* was seen to laugh slightly)." "No movement at all; absolutely indifferent. Then there was a P because it is like a joke on me. It is as if some one played a trick on me and it didn't work." "It was indifferent. Even the joke side of it was not present that time."

Thus for B there was on the first conflict-experiment doubt followed by amusement which latter was reported as mildly P. In 3 of the 5 critical tests B reported amusement, or was seen to laugh. In no case with the single lights was there amusement, smiling, laughter.

Under conflicting stimulation no movements of the foot were visible to the experimenter.

Subject C. The reports with single-light stimulation were in every case affectively indifferent.

With double signals C reported as follows: "There wasn't any P or U. I merely didn't know which way to go and I went one way. I was amused; it made me smile. Amusement was perhaps P or I might call it indifferent." "Exactly the same as the other. It made me laugh and I kicked forward after a certain hesitation—just to do something." "There was amusement." "Amusement." "There was amusement with a slightly agreeable feeling."

Conflicting signals gave some doubt and hesitation, and it is noteworthy that in every case of conflicting signal there was amusement, laughing, smiling, which was reported as P or as indifferent.

A delayed forward movement of the foot was made in every case of conflict. (In the habituation series C reported forward movement as preferred to backward.)

Subject D. The results of D are complicated by a sluggish mood in which any movement was apt to be reported as U. With the single lights the affective report was generally U, sometimes indifferent.

Every test with conflicting signals gave a report of U. On account of D's permanent mood it is not possible to associate U and conflict, although conflict *may be* a factor underlying the U feeling. In two of the five conflict tests, D laughed slightly. In the tests with a single light there was no smiling, laughing or amusement.

Forward movement of the foot was made in four out of five of the conflict trials. (Forward movement had previously been reported as the preferred direction.)

Taking the 4 *S*s together it is clear that the conflict signals are associated with affective processes absent when single lights are flashed. With A the result was what we had expected. There was blocking, bewilderment, conflict, and along with this unpleasant feeling. With B there was occasional doubt and hesitation followed by smiling. Amusement, smiling, laughing, was the characteristic affective response with B and C. With D, the conflicting signals were associated with unpleasant feeling as in the case of A, but a permanent disagreeable mood renders the association of conflict and U uncertain in significance. In 2 of the 5 critical trials D showed signs of amusement and there was no trace of amusement with the single light tests.

The conflicting signals, therefore, give a type of experience which is differentiated from the simple one-light tests in two ways. (1) There is sometimes conflict of impulse, hesitation, doubt, bewilderment, and along with this unpleasant feeling. (2) There is sometimes amusement, smiling, laughing and along with this pleasant feeling or indifference. Doubtless these two types of affective reaction are related. B, for example, reports doubt (conflict) followed by amusement. We suggest that amusement is a form of release from the conflicting, impossible situation and that such a release is typically P. The situation sometimes appears to have an element of humor in it; this is a primitive, almost physiological, humor.

Habituation to Conflict. We next desired to observe the effect of repetition of the conflicting signals. Consequently for two experimental days following the above work double signals were used.

Every presentation was a combination of two lights calling for simultaneous movements. Four of the combinations are possible to perform (1 and 3, 1 and 4, 2 and 3, 2 and 4) and two combinations (1 and 2, 3 and 4) are impossible. These six combinations were arranged in a haphazard series and presented under the above-described conditions.

The instruction was changed by adding the following words: "Lights will be flashed in pairs of two. Whenever lights appear make the appropriate response."

At the start the conflicting signals gave the same result as previously. There was sometimes inhibition, blocking, conflict, doubt, strain, and unpleasant feeling. There was also laughter, amusement, smiling, which was reported as pleasant or indifferent. These affective responses were absent from those signals which were possible to carry out.

As the work proceeded the conflicting signals were taken by all *S*s intellectually as a sign for no movement at all. When this stage was reached there was no longer any conflict and U; nor was there any amusement (as though the joke had been destroyed by too much repetition).

It became clear that with habituation to the conflicting situation the affective character vanished. When this had been ascertained the experiment was brought to a close.[20]

Affective Habituation. Disappearance of the P or U of an experience when the inducing situation is repeated illustrates affective habituation. In the above experiment conflicting signals at first brought about a definite affective character, but with repetition of the double-signal the affective component vanished. At the same time the double-signal took on a new meaning—the meaning of no movement.

An experiment upon the constancy of affective judgment to odors[21] was performed simultaneously with the above. The same 4 *S*s made affective judgments upon 8 selected odors. Judgments were made on each experimental day for a period of 5 successive weeks. The results show a considerable degree of constancy in affective judgment throughout the experiment, and the conclusion was drawn that "there is very slight evidence of progressive and regressive changes in judgment." There were daily variations[22] in judgment but no evidences of habituation. A recent experiment by Kenneth[23] appears to confirm my result.

What shall we say of the fact that two experiments, done at the same time and with the same *S*s gave opposed results? Is it that affective habituation existed in the experiment with double-signals, and that affective habituation did not exist in the experiment with odors? This view is quite possible and further work should be done to clear up the situation. At first sight we seem to be in the presence of another one of those cases in which logically opposed conclusions follow from different sets of experimental data.[24]

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[20] At the close of the experiment a few tests were made with the knee-jerk (using a reflex hammer) combined with the light signals. Once the forward signal and the knee-jerk came at about the same time and there was marked laughter and amusement and the comment "my reaction was made for me." There were also several cases of blocking associated with U.

[21] See footnote 19.

[22] My original conclusion regarding constancy was as follows: "The series of affective judgments show various degrees of constancy. The most variable odors are in the region of indifference while the most constant are either 'very pleasant' or 'very unpleasant.'" Dr. J. G. Beebe-Center, of Harvard University, has pointed out in a conversation, that this apparent constancy of the extremes of intensity rests upon a statistical limitation in the scale-of-values method. If a series of judgments are averaged, the average value and the maximal variation are interrelated. For example, the upper limit of our scale was +3. In order that a series of judgments should average +3 the m.v. would have to be ±0, for any variation towards indifference would lower the average. Every average has a limit beyond which variability cannot go. If limit of variability be plotted against average, a mound-shape curve results. The original results have been reconsidered in the light of this criticism. Between +2.5 and −2.5 actual variability falls within the mathematically determined limit. There is, however, no evidence that judgments are more constant at the extremes of this range than in the region of indifference. Hence our conclusion must be revised to state that daily variations in affective judgment are about the same in all regions of the scale. The greater constancy of affective judgments at the extremes of the scale is a statistical rather than a psychological matter. I had a suspicion of such an error when I wrote: "It may be that the intense feelings are actually variable, but that a scale with only three degrees of U and three degrees of P is not adequate to the intensive differences (*op. cit.*, 189)." Dr. Beebe-Center has made experiments upon affective habituation which confirm the above interpretation.

[23] J. H. Kenneth, A few odor preferences and their constancy, *J. Exper. Psychol.*, 11, 1928, 56-61. Kenneth (p. 58) appears to confirm my conclusion regarding greater variability in the region of indifference than in the extremes of the scale, but his argument rests upon the same kind of a fallacy as my own.

[24] See The logic of affective psychology, this JOURNAL, 38, 1927, 186-9.

My own tentative solution of the difficulty is as follows. The affective judgment of an odor is ambiguous regarding *felt* P and U.[25] Realizing the possibility of such an ambiguity, statements in the earlier article were made about the affective *judgments* and not about the *felt* experience. A series of affective judgments is therefore equivocal regarding the point at issue—the existence or non-existence of habituation to *felt* P and U.

CONCLUSION

Simultaneous signals to conflicting movement called out an affective experience which was absent with single signals. With one *S*, and possibly with two, conflict evoked unpleasant feeling. With 3 *S*s, amusement, smiling or laughter was evoked and this was typically P or indifferent. It is suggested that the latter type of response be considered a release from the conflicting and impossible situation. Repetition of conflicting signals led to the dropping out of both affective characters.

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[25] See footnote 16.

AUTOKINESIS AND THE STREAMING PHENOMENON[1]

By J. P. Guilford, University of Kansas

In a previous article[2] it was shown that the eye-movement theories fall short of an account of the autokinetic sensation of Aubert. In the first place, there are too many facts inconsistent with such theories, and in the second place it was shown by means of a photographic record of eye-movements during the apparent movement, that the illusion occurs independently of any correlated eye-movements. It was suggested that the spot of light moves because the surrounding retinal elements are being stimulated for movement in the opposite direction. This theory was ventured on the basis of reports of two observers that movement of the field in the opposite direction can be seen; that we have here a case similar to the apparent movement of the moon behind floating clouds. The theory was tested logically by an attempt to take care of all the known facts about the phenomenon. Since then, the writer has secured observations from three other observers and has submitted all the hypotheses which were set up in connection with the 'streaming' theory to various crucial tests. The theory has been vindicated on all essential points, but in some respects has had to be modified and extended in the light of new facts.

Observers. The *O*s for the experiments were, Dr. Elizabeth F. Möller (M), instructor in psychology; Charles W. Fox (F), assistant in psychology; and David Bidwell (B), fellow in psychology. All three have had considerable training in psychological observation and, so far as the writer knows, came to the particular problem without theoretical bias and without any intercommunication during the course of the experiments.

Direct observational evidence for the streaming theory. From descriptions under 3 sets of instructions the following facts have been verified: (1) There is a general movement of the film[3] in the whole field in a direction opposite that taken by the spot. (2) Films of various tints and hues are seen drifting across the face of the moving spot. (3) The moving disk leaves behind it a trail of light gray or of a hue complementary to that of the moving spot. This last fact may be taken to mean that the retinal streams are washing chemically altered material away from the region of stimulation.

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[1] From the Psychological Laboratory of the University of Illinois. I wish to take this opportunity of thanking Professor Madison Bentley for valuable suggestions and criticisms.

[2] J. P. Guilford and K. M. Dallenbach, A preliminary study of the autokinetic sensation, this JOURNAL, 39, 1928, 83-91.

[3] This term is used in the sense in which it is employed by Katz, *Die Erscheinungsweisen der Farben*, 1911.

The stimulus light was, in most of these observations, a faint, yellow, circular spot 1.2 cm. in diam., or else a 0.6 cm. red disk, about 100 cm. distant and with foveal, monocular stimulation.

Results. A more complete description of the phenomenon is given in some typical reports from the *O*s.

Instruction 1. "Describe any phenomenal or psychological changes which occur in the moving spot."

M_1: "Sometimes it disappears altogether, but that is not for very long. Sometimes behind the red film as it moves there is a faint gray which is also a film. The meaning is that the red film ducks under a dark cloud and I see this trail after it like the tail of a comet. The trail is often very long, often across one quadrant in the path the spot has taken."

F_1: "Twice I noticed a thin light greenish blue line going from the spot down to the left. The light greenish line followed the spot; it was narrow and very long. Occasionally other parts of the field were in motion, *i.e.* it was not merely a dark gray background."

B_1: "I think there is a correlation between the side of the spot that changed in color and the direction of the movement. The movement seemed to be toward the side away from which the change takes place."

Instruction 2. "Describe any phenomenal changes in the spot and in the field around the spot."

M_2: "As the spot moves, it loses in chroma. Sometimes all color fades out and there is just gray. It means that this disk is floating in a film and sometimes it ducks under this film and then looks grayer. There is more movement there than just the movement of the disk, because the change in chroma is a gradual thing; it floats over the disk, across it. That is where I get the meaning that it is moving in a film. They [the films] are always moving. It does not mean that the disk itself has changed, but that the background is doing it."

F_2: "Rather late in the experience I saw very light greenish blue lines. These usually ran from the spot down and to the left. When the spot is moving the line is always, I think, back of it."

B_2: "These changes in the field seem to flit across the face of the spot. The spot seemed to change in brightness, *just like clouds flitting over the moon.* The change in appearance went across from the left to the right; the left side of the spot changed first, lightened or darkened." "There was movement all over; the general drift seemed to be upward. When I notice this movement in the periphery the spot remains perfectly still. Only when I ignore the movement in the field does the spot begin to move. The spot moved downward, and only when I stopped noticing the movement in the periphery." "Movement was not very extensive, but in general downwards. It started with a large dark after-image underneath near the edge of the field. It [the after-image] seemed to be growing and involving the rest of the field. It could be noticed as either a motion of it toward the spot or of the spot toward it; I got both."

Instructions 3. "Describe any phenomenal changes in the field, giving your attention especially to the films in front of the spot."

M_3: "There are little wisps of gray that are moving constantly over the disk front. Sometimes something that is almost a surface in black is there and the disk goes under it. It means that the disk is moving in a constantly moving film. Over the surface of the disk there is a whirling, moving, bit of gray. *It moves the way clouds move*, so it is always in movement. Then other pieces of film much closer to me than the disk come out over it." "This little disk plows through a film. It is a thick film so that it leaves a rather definite boundary line where it piles up. If the disk is moving up, then the black film on each side moves in on both sides of the disk and changes the outline somewhat. If the disk moves sideways, the film folds in from the top and bottom, not perfectly parallel, but V-shaped. While it is going on this way, there is a gray film which usually moves in an opposite direction from that in which the disk is moving. If the disk moves up, the film moves down, only it never moves clear across but slides off before it reaches the bottom."

F_2: "There were bands of darker and lighter bluish gray in the film or haze. At one time this seemed to be moving down to the left over the spot, and as the bands passed over the spot there were changes in chroma in the color of the spot itself." "At one time the whole film seemed to be moving, either up and to the right or down and to the left; there were particles in the periphery moving along that axis. The spot was moving up and to the right. Before the spot changed in chroma, I knew that the change was coming because I could see the bands moving down and to the left. Once there was a very dark band, almost black, coming, but it did not cover up the spot; instead the spot pushed into it; the black covered only the front part of the spot, the side ahead."

B_3: "There seemed to be at the start a heavy mass of color at the bottom, very dark blue of low saturation. Movement started immediately in this, up and to the left, contrary to the spot. These masses of color move over the spot—there are changes in the color of the spot, although less than the colors outside the spot. Shortly afterward the color became more diffuse, more equal over the field, and at the same time the spot lost its motion."

Just which cue, the movement of surrounding films or the drifting of films over the face of the disk, has greater effect is hard to say. In some of the reports it was stated that the gray films which cross the face of the disk often slip off the sides or else go part way across and then retreat, as in the following report:

M_4: "The film that I see between me and the disk is made up of little particles and wisps—not of uniform film—and these bits are moving slowly all the time. They are not going from place to place—there is no aim in their movement, they just float around. They move over the surface of the disk, but never completely cover it. They slip off, somehow, and never cross the face of the disk. They fall off the sides or else go back."

Later observations tend to show that movements in the periphery of the field are very potent factors, as in this report:

M_5: "The spot at which I am looking moves, and it moves in a cloud of black. I found this, too, that the black moves in the same direction as the spot. There is a gray out beyond this and this is moving in the opposite direction. This can be seen only when I spread my attention beyond the black."

The same fact is indicated in the reports B_2 and B_3 quoted above.

Direction of movement. In the previous article mentioned above it was indicated that the supposedly greater outward tendency for the movements, as found by Carr[4] and others,[5] squares with the inward flowing streams of Edridge-Green,[6] and that the prevailing directions of the autokinetic phenomenon in the 45°-axes of the field, squares with the typical streams given by Ferree[7] as well. Further results, however, force us to conclude that neither of these streams is responsible for the general direction of the apparent movement.

First we shall mention an attempt to find recurrent patterns of movement such as were presented for one *O* in the earlier study.

The procedure was the same as in the earlier experiment. *O* watched the moving disk until he had as much pathway as he could remember; the room was illuminated and he immediately drew the path from memory. Six drawings were secured for each eye for every *O*, with foveal stimulation, and 24 drawings with monocular, extrafoveal stimulation. In the latter, the spot was a yellow disk 1.2 cm. in diam., placed 2°—in terms of visual angle—from a 0.4 cm. red fixation-point, in four directions 45° from the horizontal and vertical axes of the field.

Recurrent paths were found for only one *O* (F), and these came in rather regular cycles, a broad loop up and to the left and a narrow loop down and to the right, alternating, the spot continuing to mount upward. A complete cycle required about 74 ± 15 sec. It must be added that the pattern occurred clearly only when the stimulus-spot was in the upper left or lower right positions. M had as a rule, gross and roughly circular paths of movement; but B had only irregular and sporadic paths.

Concerning the predominance of outward movements. It was noticed from the drawings, contrary to previously accepted facts, that the initial direction was inward, toward the center of fixation, about as often as it was outward. With a disk at the center, of course, all initial movements would have to be outward; this case is excluded. But according to Carr[8] and Adams,[9] a spot in any part of the field shows an outward preference, and the more peripheral the spot the greater the preference, an average of 88% being outward.

We therefore found it desirable to test this matter by means of systematic experiments. Four eccentric spots were used, as before, with a red fixation-spot at the center. The visual angle between the fixation-dot and the eccentric spots was varied from 2° to 30°. The procedure was as follows. *O* kept fixation upon the red dot and after a 'ready' signal one of the 4 lights was turned on. *O* was to report whether the initial direction was 'out,' 'in,' or 'neither.' A 'neither' judgment was given when the exposed disk moved tangent to a circle which passed through it and whose center was the fixation-spot.

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[4] H. Carr, The autokinetic sensation, *Psychol. Rev.*, 17, 1910, 42-75, 46 ff.

[5] H. F. Adams, Autokinetic sensations, *Psychol. Mon.*, 14, 1912, no. 59, 8 f.

[6] F. W. Edridge-Green, Visual phenomena connected with the yellow spot, *J. Physiol.*, 41, 1910, 263-275.

[7] C. E. Ferree, The streaming phenomenon, this JOURNAL, 19, 1908, 484-503.

[8] Carr, *loc. cit.*

[9] Adams, *loc. cit.*

O found it difficult at first to give a report because the fixation-spot was already in motion before the disk came to view. In this case, he was instructed to decide which of the two spots was in the lead, for they always moved off together. During all the hundreds of such observations we did not find a single case of independent movement of the two spots, corresponding to movement of type II of Carr.[10] There were slight oscillations; the two spots would spread apart slightly and snap together "as if held together by rubber bands," but never over a few mm. in extent. How one spot could move so far as 20° while the other one remained stationary is beyond our comprehension. The very slight oscillations which were observed were *within* the moving *group* of dots, and without exception, one did not move without a corresponding or compensatory movement of the other. One case, already mentioned, a slight pulling apart and then together, was perhaps the most common. In another type, the one in the lead would suddenly spring forward a little, then the rear one would make a jump ahead, "as if playing tag." In a more rare type there would be slight, but very slight, turning, or torsion, which was immediately corrected, "as if it were a balloon with its basket." Indeed, a further study of the mutual movements of two spots seems worth while in itself.

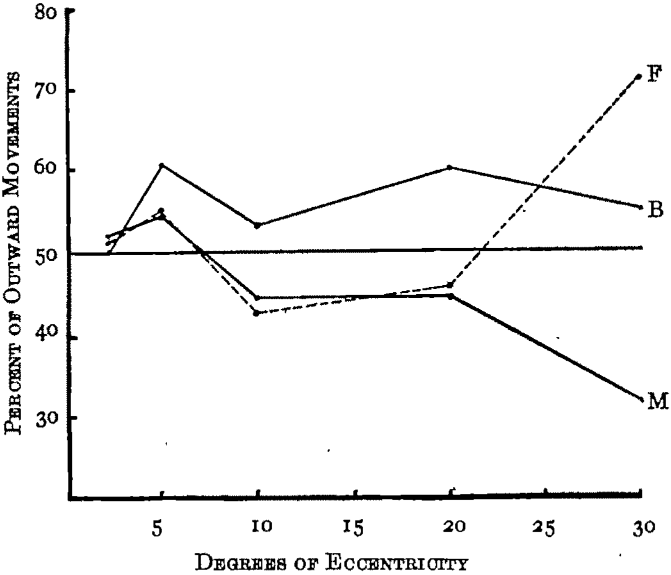

FIG. 1. SHOWING PERCENTAGE OF OUTWARD MOVEMENTS WITH EVERY DEGREE OF ECCENTRICITY

Two hundred observations were secured from each *O* for every angle of eccentricity used. The results, in terms of percentage of outward movements, may be seen in Fig. 1. Only one *O* (M) shows a consistent tendency for the direction of initial movements, and that tendency is contrary to the expected one; the more eccentric the spot, the less strong is the outward tendency. One *O* (F) shows a sudden increase to 71% at 30°, but this is to be accounted for from his verbal reports. Peripheral observation is extremely difficult, as everyone knows, and furthermore in this case, at least, it induced incipient eye-movements in the direction of observation. Such eye-movements, as investigators of autokinesis know, interfere with the illusion. All the *Os* reported that the movement of the spots in the periphery becomes short, jerky and uncertain, not at all like the long, smooth, sweeping movements which occur with foveal stimulation. Indeed, it was just because of these difficulties that the degree of eccentricity could not be carried beyond 30°. With an eccentricity of 45° all *Os* found it impossible to get the illusion or to report upon it.

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[10] Carr, *op. cit.*, 43.

If one secures an average for all 5 positions combined, the percentage of outward movements are 55.8, 53.6, and 45.7, for B, F, and M respectively, with probable errors not greater than 1.2. Thus it appears that whether the initial movement will be outward or inward is almost a matter of chance, so long as it is the illusion we are dealing with. Since the initial movements come immediately after exposure of the light, and since the direction is already predetermined by that of the fixation-spot, and since the exposures are made by chance, we have a random sampling of the prevailing directions of movement. In fact, one would expect, *a priori*, since the two spots move together, that the outward or inward movements would be a matter of chance, unless the fixation spot spends more time in going in some directions than in others.

The most significant fact which comes out of this result is that the direction of spots anywhere in the field is determined all at once, at least within 30° of the center. This means that the limited streams of Edridge-Green and of Ferree are out of the question, that the streaming occupies the greater part of the retina and that there is one general direction. A further test of this conclusion was the use of 2, 3, and even 4 additional spots simultaneously. All moved off together in the direction already set by the fixation-point, retaining their mutual relations and distances, except for the fixation-point, which was smaller and of a different color. The minor striking readjustments of this spot in the new patterns of 2, 3, and 4 moving lights are a subject for study in themselves.

The fact that the direction for apparent movement in all parts of the field is the same at any one moment agrees with the *Os'* reports of the streaming phenomenon. The usual report is that the field is a boiling, bubbling, billowy mass, and that this has one *general* direction opposite to the direction of the moving spot or spots. The following reports are typical.

M_6: "The field is a film made up of different waxen grays and these bubble and roll *like smoke does*. Then one part of the field gets very black and moves in a definite direction across the field. The spot is in that black, going with it in the same direction. The rest of the field is going in the opposite direction." [*E*: "How much of the field does the black occupy?"] "If the field were one foot square the black would be about 1 inch by 3 inches."

B_4: "The visual phenomena are rather difficult to observe. They boil so much; the movement is so erratic it is hard to say just what the general drift is in them. When the visual phenomena move one way, the spot moves in the opposite direction. The best thing to compare them with is slowly *moving masses of smoke*."

In no case have the *O*s reported anything like the streams of Edridge-Green, or even those of Ferree, except for spirals or whirling movements which do not endure for any great length of time. The boiling movements give us precisely the basis which we need for 'explaining' the minor oscillations in the phenomenon. The spot may be floating smoothly in a general direction; it may go by little spurts or jumps; it may suddenly make a slight detour; quite often it is seen to make a complete loop of small diameter in its path. At other times, when moving in a horizontal direction especially, it has the appearance of a "rubber ball bouncing along."

Theoretical implications for localization and for movement in general. Two very important theoretical considerations come in at this point. We can merely mention them because they are secondary to the specific problem and because there is insufficient data to draw any assured conclusions. One concerns the problem of absolute visual localization, and the other concerns the stimulating conditions for movement in general. As to the latter, it can be said that there appears to be a stimulating condition for movement which may be independent of, and which possesses a lower threshold value than that for sensory quality. The stimulating condition in this case is the streaming of something in the retina. Whether the stimulation is due to changes in the chemical medium for the rods and cones as the streams move across them, or whether it is from shadows of particles in the medium, which cross the retina, cannot be determined. It is probably the former, as indicated by the trail of the negative after-image behind the moving spot. At any rate, there must be a succession of stimulations sweeping across the receptive layer. Whereas the change is so slight that differences in quality are observed with difficulty, the impression of movement is quite clear and produces the striking consequences of the autokinetic illusion and of loss of orientation in general in the dark. This seems very close to pure movement without quality, or pure phi, as it has been called. The phenomenon might well receive more attention from those who are interested in theories of phenomenal movement in general.

As for absolute visual localization, it appears to be almost a pure myth, as some of these experiments tend to show. In the first place, the fact that a spot of light can be localized successively in different parts of the field, as far from the original starting point as 45°, while the same retinal elements are being stimulated, means that the effect of absolute localization is not very potent. To be sure, the first moment the light is fixated it is properly localized. It almost immediately begins to move and from that time on does not regain its 'proper' orientation, except as it passes through the starting point on its return excursion. *O* feels that he 'knows' where the starting point is; it remains as a sort of anchorage about which the rest of his space world is suspended. He may even feel that his eyes, head, even the whole body are turning to follow the moving spot. In that respect the spot of light retains its proper localization with respect to him—to his eyes and body. On the other hand, if *O* is asked to localize the spot of light with respect to the center of his field of vision, he invariably places it at some distance from the center.

It was of interest to know what would happen to the moving spot, its localization and *O*'s general orientation, if after the spot had moved some distance, other lights were suddenly exposed in different parts of the field. This was done in the same way as in our study of the effect of eccentricity above. After the fixated spot had moved outward, a light, in one of 4 directions, varying from 2° to 30° in the periphery, was turned on. It was expected that the moving light would jump back to the center of the field and that general orientation would be regained. Such was not the case. The general rules were as follows: (1) The coming of the additional light suddenly retarded the movement, but very rarely stopped it. The effect is approximately proportional to the intensity of the additional light. No exact measurements were made. (2) After this momentary pause, the two went on together in the original direction without a regression to the original center.

Two *O*s agree to this in response to the instructions, "Describe what happens to the movement when the additional light comes on."

F_4: "When the spot of light comes on the movement of the red spot is retarded, almost stops sometimes."

B_5: "When the bright light comes on the effect is to slow it down or completely to stop it. The stoppage is greater when the light is in the direction toward which the spot is moving."

The third *O* however, reported no marked changes under these conditions:

M_7: "The red one is already in motion in a certain direction when the yellow light is turned on. The yellow one comes and without anything more moves along with the red one in the same direction."

The disappearance of the additional light has far greater consequences. The usual thing is (1) a sudden acceleration of the movement of the fixation spot, and (2) a disturbance of the general direction of the spot. The following report is typical:

> F_5: "Very often when the white spot is removed the red one shows an immediate acceleration in the direction in which it is moving. This period is very brief. Immediately after this period the red spot seems to lose its stability, changes direction, and moves around uncertainly in various directions, then it resumes its ordinary movements."

Localization of the spot is thus apparently linked up with the whole phenomenal field whose changing orientation, from the visual aspect, is dependent upon the shifting stimulation upon the retina. The rôle of bodily pressures will be discussed in what follows. The specific effects of addition or removal of spots of light in the field could form the basis for a special study.

The rôle of eye-pressures in the movement. According to the theory of Carr,[11] the loss of orientation, and with it, the apparent displacement of the spot, is due entirely to a shifting of eye-pressures which result from differential fatigue of the extrinsic muscles. The theory is based upon the fact that the direction of movement is usually in the direction of eye-strains. It was decided to test this correlation with our *O*s, and if it was verified, to examine into the relation between these pressures and the retinal streams. It was suggested in the previous study that the strains are a result of the visual phenomenon, perhaps set up in the attempt of *O* to inhibit the tendency to follow the moving light with his eyes.

The *O*s were unacquainted with the eye-pressure theory and the facts upon which it is based. One *O* (B) during the early experiments stumbled upon the correlation and expressed a conviction that the eye-strain had something to do with the illusion. The *O*s were started on this problem with the very general instructions: "Describe any pressures which might be correlated with the movement of the spot."

The most noticeable pressures accompanied upward or downward movements. These were localized chiefly in the eyes, but also in the head, throat, shoulders, and chest. During upward movements they were 'bright,' 'buoyant,' upward-pointing streams which meant to *O* that he was rising to follow the moving spot. During downward movements, the pressures were 'dull,' 'depressing,' and downward-pointing. The impression of *O* was again that he was attempting to keep with the spot, to keep it in sight. A very good description is found in one report from M.

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[11] *Op. cit.*, 67 ff. Professor A. A. Schaeffer in a recent paper (Spiral movements in man, *J. Morph. and Physiol.*, 45, 1928, 347 f.), has suggested that the autokinetic sensaton is closely related to the phenomenon of spiral movements which he has been studying. Blindfolded *S*s who are attempting to go directly toward a distant imaginary point, by walking, swimming, driving a car or a motorboat, invariably go in paths which form clockspring spirals. A complete cycle for the autokinetic movement requires about the same length of time as a single looop in these spiral movements. If the stimulus-light is attached to *O*'s body in such a way, that as he turns on his heel to follow the autokinetic movement the stimulus-light will also be moved, his body will make a complete rotation in about the same period as for a loop in the spiral phenomenon. Professor Schaeffer attributes both phenomena to a deep-seated 'spiral mechanism' which he has found in many species of animals even as low in the scale as the amoeba.

M_8: "There are bright, strainy, pressures localized in the eyes, particularly up toward the eyebrows. The only way the spot went was up. The strains grew more intense as the thing kept going and the pressures instead of being tiny ones, fused into a big mass. I think that I raised my eyebrow, because you think that in a moment the thing is going to be beyond them. There are other pressures, light ones, especially around my shoulders, as if I were lifting myself up with the red thing too. If the thing keeps moving up this pressure becomes very unpleasant and other pressures seem to come in. These are around the neck and shoulders. They are light, bright things and they come from real movements. They mean that I am getting up to keep above the spot. Then when it goes to the side, all those pressures shift to the side, including the eye-pressures. The eye-pressures go first. Going down it is not so bad. There is a sort of relaxation and instead of being bright, the pressures are dull."

One report of pressures correlated with right and left movements is enlightening:

F_5: [After the instructions, "Observe any eye-pressures which may be correlated with the movement of the spot."] "When the movement to the left begins, these pressures which have been in my eyes disappear, *i.e.* they cannot be attended to. The experience which has been fairly well localized in the region of the eyes suddenly spreads out into a much larger field, a field that comes to me as bright, and certainly pleasant. Out of this large, undifferentiated experience, come pressures in my shoulders, which mean that my left shoulder is moving backward and my right shoulder is moving forward, and more indirectly, that the whole upper part of my body is twisting slightly to the left. The corresponding thing holds for movements to the right. Whenever there is a movement which has both horizontal and vertical components, both the pressures in my eyes and the pressures in my shoulders may appear. But they do not seem to form an integrated pattern, possibly because I attend to one and then to the other. Each part seems to have its own reference."

All *Os* later came to identify specific eye-pressures with certain directions of movement. One important result, in view of Carr's theory, is the presence of corresponding bodily pressures. No doubt these also enter into the loss of general orientation, but it is difficult to attribute them to a differential muscular fatigue. Would it not be more reasonable to attribute them to the shifting visual field? One gets the same 'illusion' of bodily movement in the face of a waterfall or of a slowly moving train.

We next secured some observations which make the latter interpretation plausible. If the shifting visual field is responsible for the bodily pressures, these pressures should occur without a spot of light in the field. The *Os* were accordingly asked to report "any similar pressures" with the eyes closed. Only one *O* (M) was able to do so without further practice. This one reported with some surprise that the pressures continued to "roll around in the same fashion" as when the eyes were open and fixating the spot. Her report on this occasion is as follows:

M_9: "The pressures start to go up; when I open my eyes the spot is also going up. Sometimes the pressures would move over to the left, I would open my eyes and the spot *would be over to the left and moving in the same direction as the pressures.*"

A new procedure was then followed. The light was exposed long enough for the pressures to develop in a definite direction. It was then turned off for varying intervals increasing in length. The instructions were: "Try to maintain fixation even when the light is turned off. Describe what happens to your eye pressures after the light disappears." The reports of all *O*s agree remarkably well upon what happens here.

M_{10}: "When the red dot went out, the pressures continued. Once or twice when the dark interval was very short, the pressures went on, and when the red dot reappeared the dot was going the same direction the pressures were."

F_6: "When the light is first turned off, the first thing that happens, is that everything drops away for an instant; everything becomes leveled. Then these pressures appear again about as they had been during the period when the light was on, but they are incoherent; they have no definite pattern. Then I recover my fixation, *i.e.* this fixation becomes noticeable, the pressures which happen to be there again become coherent. There are dull pressures, almost strain-like, that are localized above my eyes and to the front of my head. When the light comes on, these pressures seem at first to disappear completely, but they are back almost immediately, and are smoother. The movement in every case was up. During each movement the smooth pressures above my eyes moved backward. They were much less intense than when the light was off."

B_5: "When the light is turned out the pressures fade out just for a moment. There were bright pressures in the upper right hand corner of the eye, between the eyelid and the eyeball. They moved to the upper right and it did seem to be correlated with the direction of the spot. Then when the light went out it would almost disappear for a second and at the same time I would get more diffuse pressures all around the eyeball as if my eye were moving. Then the other, bright pressure, would come back and if the light were turned on it would move in the same direction."

Prediction of the direction of movement. The ability of the *O*s to predict the direction of movement from the direction of eye-strain before the light was turned on is very evident. A test was next made to determine just how accurately *O* could make this prediction. Other tests consisted in prediction of the direction of the illusion from the general direction of the streaming of the field before the light was turned on. From a comparison of these two sets of predictions it was hoped to determine, if possible, which of the two is more directly connected with the apparent movement. The tests were given on two different days and alternated so as to take account of practice and fatigue effects. None of the *O*s, after initial practice, had difficulty in abstracting the pressure and visual cues from each other.

The procedure was as follows: *O* was asked to announce a prediction as soon as he could, with the light turned off, but with fixation in that direction. The light was immediately turned on and *O* reported the direction of movement. Both prediction and check report were given in terms of the face of the clock, the smallest unit used being the half hour. Table I gives the results in terms of the average errors, in terms of hours on the clock, made by each *O* using the two different cues.

TABLE I

PREDICTION AND CONTROL

O	Prediction				Control	
	Visual cues		Pressure cues		By pressures	
	Av. error	Cases	Av. error	Cases	Av. error	Cases
B	.54	40	1.50	53	3.12	13
F	.18	49	.25	71	.01	57
M	.11	48	.15	40	1.55	49

In every case, the error is greater for those predictions based upon pressure cues. This might be expected and the reasons for it appear in *O*s' reports. They were asked after each series of predictions to give a complete account of the cues which they used. The following reports were verified on at least two different occasions; there are some individual preferences in the way of cues, but on the whole there is agreement:

Pressure cues.

M_{11}: "Of all the pressures in the eye there is one little band that moves around and which is the criterion. I do not know where they are from, because I know that when I roll my eyes they are not the same pressures; they are another set. These little things move around. They are innumerable, but they make up one little band and where that band goes the spot goes too. There are many more intense pressures to deal with, but the thing goes where the little ones go. [*E*: "Where are these pressures localized?"] "They are localized around the eyeball, not only under the lid, but deeper inside the eyeball."

F_7: "Rather dull pressures, not extremely dull, over my eyeball, not very far front, although relatively, mean an upward movement; straight up. If these pressures are displaced a little to the right, that means a horizontal component along with the upward movement."

B_6: "A definite pressure, a complex of pressure, cold, and a little bit of pain, under the eyelid. That was the sensory factor upon which I based the judgment. I abstracted from the visual cues. The spot of pressure stayed almost always near the junction between the two lids, at the inner corner. It seemed to belong more to the lids than to the eyeball."

Visual cues.

M_{12}: "One part of the field gets very black and moves in a definite direction across the field. The spot is in that black, going with it in the same direction, and the rest of the field is going in the opposite direction."

F_8: "There are bands of lighter and darker gray extending in the direction of the movement to come. The ones which pass through the center of the field are the clearest. Sometimes I can see a movement within the bands in the direction opposite the predicted movement of the spot. This movement seems to involve small particles in the field. Sometimes this movement of the streaks becomes very unclear and practically disappears, and that leads to ambiguity in the prediction; for example I predicted 7, and 1 came."

B_7: "When I make a judgment it seems purely visual, *i.e.* I would see streaks or bands which move across the field opposite to the direction I predict."

The visual cues permit more accurate prediction because the spot takes its place in a field which is already moving in a direction that is readily determined by *O*. The pressure cues, on the other hand, are non-visual and less precise. *O* has to decide in what direction the resultant strain lies. The visual phenomena seem to be more closely connected with the apparent movement of the spot than do the pressures.

Control of direction. A question arises here as to whether *O* was really predicting the direction or whether he made it go in the predicted direction by autosuggestion. We are convinced that the predictions are genuine. Earlier in the course of the study the *O*s had been instructed to make the spot go in a direction they wished, or in a direction named by *E*. All three *O*s were unsuccessful on every occasion and they were convinced that control of direction is impossible. After one period in which predictions were made the *O*s were again told to make the spot go in any direction named by *E*, but this time, by establishing eye pressures in the proper direction. All *O*s were able to do so with some degree of success. Another period of predictions followed this in which all *O*s showed some improvement, but no more than could be attributed to practice. The average errors for the control of direction differ considerably from those for prediction, being much greater in two cases, M and B, and very much less in the case of F (see Table I). The secret of F's remarkable success may be found in his report (F_9) given below. There are cases, even after *O* has had much practice in control and has confidence in his ability to use it, in which the spot refuses absolutely to go where he wants it to go.

The fact of voluntary control is highly important for a knowledge of the relation between the eye-pressures and the retinal streams. That the two are correlated cannot be doubted. Which one is genetically prior is our next problem. It was suggested that the shifting stimulation from the streams on the retina may give rise to the eye-strains through their oculomotor consequences. This hypothesis will have to be given up in view of the facts of voluntary control and because of another crucial test which was made. The fact is that *O* can, by producing a strain in a certain direction, alter the streaming phenomenon in a corresponding manner. The pressures therefore need not depend upon the streams.

First let us be sure that the pressures which are set up voluntarily are the same as those previously reported, the following reports will, I think, lay any doubts as to this fact; the descriptions are almost identical.

M_{13}: "What I try to do is make the pressures go the way you call. It seems that when you say '5,' I try to roll my eyes down although I keep fixation. I know I move my head because of pressure on my chin, trying to get pressures that will go down. The spot sometimes does go down, and I notice that there is that eye-pressure going down that I had before. But sometimes it will not pull the thing down and then it wobbles, *e.g.* if you say '5' and it is going toward 1, it does not go in a definite straight line, but it zigzags down and up and sometimes winds up at 2 or 3. When it does that, I notice that it goes the way the little eye-pressures are going and all this bulky stuff, like that from pushing down with my head, does not mean a thing. They are very slight eye-pressures which it follows, but it goes right along."

F_9: "I use the same cues that I reported before, in pressures. The pressures are all above the eyes. If I want a downward movement the pressure is at the front, over my eyes; slightly narrowing or drawing down the upper eyelid helps to bring that about. For an upward movement there are pressures moving back over my eyeball. For left movements the pressures are focalized at the left; for right, to the right. It is really a problem in physical resultants. I say to myself, 'I want to get the movement away from 5 and up to 12; how much vertical and how much horizontal power will I need?' Thus I exaggerate one or the other component to bring it about more quickly."

The following reports came in response to the instructions, "Observe what happens to the visual field as you exert strains in the new direction."

M_{14}: "Sometimes there is a black strip and that may be moving, say, toward 9. Then I change all my pressures toward 1. The black melts into the gray and there is a general movement of the whole thing up toward 10 or 11, but that is just a bubbling, vague thing, not very definite. When it goes the way it should go, the black cloud moves toward the proper number, the pressures are there and everything. When it is going one way and I want it to go another, it just melts like colored ice cream; there is nothing but a slow boiling and no certain direction. Then when the spot eventually goes the way I want it to go, the field gets rearranged. There is a gray background, and on it, just around the spot, is a very black film which moves with the spot in the proper direction across the gray."

F_{10}: "The movement at first was to 2. It was a rather bluish gray field. Below the spot, and above it, apparently several inches, were bands of gray extending in the direction of movement. You said '11' and I set up pressures at the back and left, and I soon got movement to 11. These streaks moved around very clearly so that they were again parallel to the direction of movement, as if the spot were in the center of a dark gray long rectangle with the lighter gray edges. There seemed to be movement in these streaks toward 5. In the transitional period, between the two directions, if the change is sudden and striking, the field becomes chaotic. Then these parallel bands appear, sometimes before the new direction of the spot is decided or definite."

Source of the eye-pressures. There is little doubt from these facts that the change in the retinal streams results from pressures exerted in a given direction. What the source of the pressures is, and what normally conditions their changes, aside from voluntary control, is an interesting problem. There are three sets of eye muscles which could be responsible, the extrinsic, the intrinsic, and the muscles of the eyelids—granting that the pressures are muscular. Our verbal reports are on the whole against the extrinsic muscles (see reports M_{11}, B_6, and F_9).

Report M_{11} favors the intrinsic muscles in that the pressures are localized within the eyeball. This suggests the hypothesis that the intrinsic (ciliary) muscles, by means of their connection with the hyaloid membrane perhaps, can change the pressure on different parts of the retina. In the case of those *O*s who have a repeated pattern of movement and corresponding cycles of streams, one wonders whether this periodic shifting of the streams serves the function of continual restoration of sensitiveness of the retina. One wonders, too, whether the streaks, which are reported by some *O*s, and which are correlated with the direction of the movement, are produced by a temporary astigmatism.

Reports B_6 and F_9 indicate that the eyelids have something to do with the characteristic pressure. Can it be that the eyelid changes the shape of the eyeball and in doing so sets up the streams in a certain direction? The pressures experienced might not be of muscular origin at all, but cutaneous, from the conjunctiva. The qualities of smooth pressure, dull pressure, cold, and pain are not unknown in connection with the covering of the eyeball. All these qualities have been mentioned in descriptions of the characteristic pressures.

Relative strength of the extrinsic eye muscles. There is some evidence against the rôle of the extrinsic muscles, at least in the way in which the Carr-Adams[12] theory would use them. The extrinsic muscles of our *O*s were tested by an optometrist in order to determine the relative strengths of the superior and inferior sets, and of the external and internal. These data were compared with the prevailing directions of the apparent movement. The latter were determined from the mass of data secured in the study of eccentricity and may be seen in terms of percentages in Table II.[13]

Two *O*s have an upward preference as opposed to a downward, one of them decidedly so, and the third shows no preference in this respect. As to the right-left dimension, two have a stronger left tendency and one a corresponding right preference, none of them being very decided.

What do we find with regard to the corresponding eye muscle strengths? Two *O*s (F and M) have exactly the same balance of muscular strength, convergence 16 prism diopters, divergence 6 Δ, sursumvergence 2 Δ, and deorsumvergence 2 Δ. The third *O*, (B), had a marked instability of muscular balance according to the following report of the optometrist: "All motor muscles very weak. Breaks fusion easily. Sadly in need of development of muscular tone. No ability to maintain, without fatigue, concentrated binocular fixation on one point." This report, incidentally, is highly significant in view of B's type of autokinesis. The movement had for him no regular patterns, was often short and jerky. This undoubtedly means that short involuntary eye-movements were breaking up the usual more extended and more regular movement which is found in *O*s with better fixation. It suggests also, that if fixation were always perfect, regular patterns might be found for all *O*s.

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[12] Carr, *op. cit.*; Adams, *op. cit*, 27.

[13] 'Up' or 'down' means that the movement had an upward or downward component respectively. The probable error in each case was 1.2% or less.

There is nothing in this comparison to verify the Carr-Adams theory based on differential fatigue and recovery of the extrinsic muscles. Although there is a decided upward preference of direction for the apparent movement in the case of two *O*s, tests show that the upper and lower muscles are perfectly balanced for strength. One of the two *O*s has a right preference and the other a left preference, yet their external and internal muscles have the same relative strengths. The net result of these tests is that we find no correlation between the relative strength of the extrinsic eye muscles and the prevalence of direction of the illusion. This does not mean, however, that the extrinsic muscles are excluded from having a rôle in determining the direction of the retinal streams and consequently of the apparent movement, but that the difference in strength and consequent difference in rate of fatigue is out of the question. The problem of the source of the eye-pressures which are correlated with the illusion, which is mainly physiological in character, will have to be solved in another study.

TABLE II

PREVAILING DIRECTION OF MOVEMENT

O	Percent of total number of movements				N
	Up	Down	Right	Left	
B	50.6	49.4	44.7	55.3	817
F	68.7	31.3	45.9	54.1	711
M	80.4	19.6	55.0	45.0	767

Streaming not the source of the eye-pressures. A crucial test was devised to determine the effect of a moving field upon the eye pressures in question.

An artificial streaming was produced, apparently to *O*, by means of a slowly moving kymograph drum set up before his eye, in dim illumination. It was only 20 cm. distant, and so occupied a rather large portion of the field. *O* was to report upon eye-pressures during the rotation (apparently from right to left) and after it was stopped (negative after-image, apparently from left to right). Two *O*s immediately and repeatedly reported that the eye-pressures 'pile up' at the side toward which the apparent movement occurs, both in the original and in the negative after-image.

The third *O*, however, reported just the opposite. It was found in this case that he took a small reflection on the surface of the drum as his object of perception. This reflection appeared to move in a direction opposite to that of the drum. The test should be repeated with the *whole* field in motion. There is a possibility that the first two *Os* took the whole drum as their object of perception. Unfortunately our experiments had to be brought to a close at this point.

If the kymograph test can be accepted, it means that movement of the field in a certain direction sets up eye-pressures in the *same* direction, not in the opposite direction as our former hypothesis calls for. The effect of the streaming field upon the bodily pressures, especially of the head and shoulders, is still to be determined. There is no reason, *a priori*, against the connection, and we have other well-known cases of 'illusory' bodily movement in the presence of larger moving bodies, *e.g.* the waterfall illusion. It is very likely also, in view of some verbal reports (see F_5 above), that the bodily pressures and the eye-pressures are separate phenomena, individually conditioned.

SUMMARY

In the present paper we have reached the following conclusions:

(1) The streaming theory of the autokinetic sensation stands the test of observation and of various crucial experiments.

(2) The streams responsible for the apparent movement are not those of Ferree or of Edridge-Green, but they are streams which involve the whole retina at once.

(3) Movement of a spot in any part of the field, at least within 30° of the center, will have the same direction at any moment, and a number of spots will always move together in the same general direction.

(4) Whether the initial direction of movement will be outward or inward, except for a spot at the center of fixation, is a matter of chance no matter where the spot is in the field, within 30° of the center.

(5) The fact that the movement is in the direction of certain eye-pressures has been verified.

(6) An observer can predict with great accuracy, either on the basis of the correlated eye-pressures or on the basis of the visual streaming, in what direction the spot will move when it is exposed.

(7) An observer is able to control the direction of movement of the spot by setting up the proper eye-pressures.

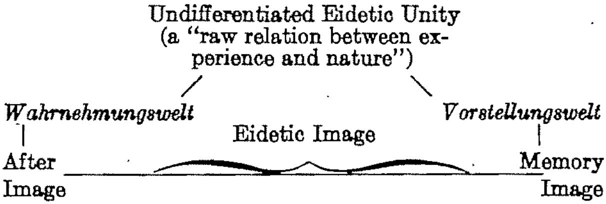
(8) A change in the eye-pressures produces a corresponding change in the direction of the spot by breaking up the retinal streaming and giving it a new general direction.

(9) Differential fatigue and recovery of the extrinsic eye muscles, as dependent upon a differential muscular strength, are out of the question as determiners of the direction of the movement.

THE EIDETIC IMAGE AND THE AFTER-IMAGE

By Gordon W. Allport, Dartmouth College

E. R. Jaensch contends that from an original undifferentiated eidetic experience derive both perception (*Wahrnehmung*) and idea (*Vorstellung*).[1] In support of this theory it is alleged —and this is a crucial point—that, in respect to its many attributes, the eidetic image stands midway between the after-image and the memory-image (to be understood as equivalent of *Vorstellungsbild*). Sometimes the EI (eidetic image) is *nachbildnah* (especially in the "T type"); sometimes it is *vorstellungsbildnah* (especially in the "B type"); sometimes it occupies a well-balanced position between.[2]


Undifferentiated Eidetic Unity
(a "raw relation between experience and nature")
Wahrnehmungswelt
Vorstellungswelt
Eidetic Image
After Image
Memory Image

If the EI (especially in children where the phenomenon is manifest in its purest form) can be shown to possess an inherent relationship with *both* the AI and the MI, evidence on the descriptive side at least would be at hand in support of the theory. If the existence can be demonstrated of a "unitary type,"[3] in which neither AI nor MI can be secured, but only the "raw" EI, then the theory acquires a probability in the genetic sense. If these contentions cannot be supported the theory breaks down, for the proposed *terra media* (both descriptive and genetic) between perception and idea vanishes.

The following considerations will show that the hypothesis of a continuum of *Gedächtnisbilder* (AI, EI, MI) is not acceptable, and that the weakness seems to lie chiefly in the relating of the EI to the AI. In reality these phenomena have only a superficial and accidental resemblance. On the other hand, such a close relation obtains between the EI and the MI that the former, in all probability, should be considered as merely a limiting case of the latter.

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[1] E. R. Jaensch, *Ber. über d. VII Kong. f. d. exper. Psychol.*, 1922, 23.

[2] E. R. Jaensch, *Die Eidetik*, 1925, 2; *Zsch. f. Psychol.*, 102, 1927, 35-36.

[3] P. Krellenberg, *Zsch. f. Psychol.*, 88, 1922, 56-119.

The affinity alleged to exist between the EI and the AI rests upon evidence which may be examined under five headings: (1) the discovery of the *eidetische Anlage* in an observer through an examination of the behavior of his AI; (2) the existence of negatively colored EI; (3) the fact that both the EI and AI are localized in visual space; (4) *occasional* resemblances in behavior of the two types of image; and (5) the case for the "unitary type," in which no AI as such is secured, but where all experience is eidetic.

(1) *Method of discovery.* It is said that marked *Eidetiker* are easily discovered, for they have AI which are positively colored.[4] In point of fact, very few cases are reported. Kiesow[5] examined this contention and found but one rather doubtful case among 49 children of mixed ages. Klüver writes: "In all cases the AI had complementary color."[6] Klüver's work is based upon a study of 27 cases, including 12 children. In an unpublished investigation, A. L. Allport, employing a 1-in. red square and an exposure time of 45 sec., found 43 children (av. age 11) who reported negative AI, 11 (av. age 11.8) who reported achromatic AI, and 9 (av. age 9.6) who spoke of original coloration in the image. It is entirely possible that these younger children characterized their image from the color of the stimulus, not being aware of the existence of complementary color values. The younger the child the less reliable the introspection. For this reason it is not necesssary to accept the view of Herwig that positive images (exposure time 20 sec.) are typically possessions of younger observers,[7] (evidence presumably for the "unitary type"). From the results of the 124 children just surveyed it would seem that no more than 10 rather questionable cases of positive AI can be listed. The number of genuine *Eidetiker* among children should exceed this percentage (8%).[8] Most important of all, the subjects reporting positive coloration in these studies are not the subjects who were found to possess eidetic ability.

(2) *Coloration.* Eidetic images are supposed to appear either in positive colors or without chroma (as does the MI), or in negative colors (as does the AI).[9] Evidence is conclusive that accurate positive coloring is often obtained in the EI, but it is by no means as certain that the EI can of itself appear in complementary colors. Zeman reports that the majority of his *O*s secured EI without any color, and in *no* case did a com-

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[4] Jaensch, *Die Eidetik*, 6 f.
[5] F. Kiesow, *Arch. f. d. ges. Psychol.*, 53, 1924, 447-484.
[6] H. Klüver, *Gen. Psychol. Monog.* I, 2, 1926, 185.
[7] B. Herwig, *Zsch. f. Psychol.*, 87, 1921, 147 f.
[8] H. Zeman, *Zsch. f. Psychol.*, 96, 1925, 226.
[9] Herwig, *op. cit.*, 132.

plicated *Vorlage* call forth a negative EI.[10] Using a 10 sec. exposure, Klüver found 28 clear cases of positive coloration and 10 of negative,[11] but the more complicated the picture and the less rigid the fixation, the greater was the tendency to keep the image in the original color.[12] Whenever a steady positive coloration is obtained the phenomenon does not characteristically appear and reappear; but when negative coloration is present the image seems less stable as though a conflict were occurring between the image itself and the complementary color processes.[13] This conflict is described also by Herwig,[14] but he fails to adopt the rather obvious explanation that the brief occurrence of negative coloration is due to the arousal of a temporary AI.[15] I am unable to find any account of EI appearing after a lapse of days in faithful negative coloration.

The conclusion seems inescapable that, when retinal fatigue is ruled out so that the phenomenon cannot be merely an AI, the coloring of the EI behaves in every particular like any memory color, sometimes good in saturation and positive in tone, but more often, as Zeman found, inexact or colorless. It seems probable that every case of negative EI may be explained by AI aroused through fixation, even though the subject may not have been aware of keeping his attention constant enough to produce the required retinal fatigue.[16] Even when the exposure is controlled and brief (10–15 sec.), it is not improbable that when the image takes on faint negative coloration, as it sometimes does,[17] it is due to a correspondingly faint arousal of complementary color processes.[18]

(3) *Localization.* The EI, like the AI, is said always to appear in visual space; it is "in a literal and true sense 'seen.'" It is localized outside the body, never "in the head." There is a tension and pressure felt in the visual organ "incomparably stronger than that which attends the MI."[19] In these regards the behavior of the AI and the EI are indeed similar. But does this similarity prove any necessary relation between the two phenomena?

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[10] *Op. cit.*, 226.

[11] *Op. cit.*, 102 ff.

[12] *Ibid.*, 127 f.; cf. Herwig, *op. cit.*, 150 ff.

[13] *Ibid.*, 190.

[14] *Op. cit.*, 159.

[15] Cf. F. Scola, *Arch. f. d. ges. Psychol.*, 52, 1925, 333.

[16] Cf. Scola, *loc. cit.*

[17] Klüver, *op. cit.*, 127 ff., 171 ff.

[18] Unquestionably too much is made of the distinction between AI and EI on the basis of the method of arousal (*i.e.* fixed *vs.* roving regard). Cf. K. Koffka, *Psychol. Forsch.*, 3, 1923, 137. The instructions to produce an EI by looking at the stimulus "much as one would look at any picture" do not, of course, preclude fixation. It is only natural that negative coloration should be obtained occasionally under such an *Aufgabe*.

[19] O. Kroh, *Subjektive Anschauungsbilder bei Jugendlichen*, 1922, 24, 28; Jaensch, *Ber. über d. VII Kong.*, 1922, 3.

An experimental error should be noted at this point. In order to demonstrate the phenomenon of the EI to his *Os*, Jaensch first has them develop an AI.[20] "What a 'real image' is can best be shown by having a customary physiological AI produced at the beginning of the investigation, a thing which we always did in our mass investigation, in which we obtained our cases with EI. None of these observers localized the EI in his own head."[21] It is probable that the attitude aroused through the first experiment determines the localization of the EI.[22] Even in the production of the EI with closed eyes the procedure is first to observe the stimulus upon a background, then to observe the image in respect to the retinal gray when the eyes are closed.[23] The localization naturally tends to remain outward under such an *Aufgabe*.

To be sure Jaensch himself has said that "the localization of the EI is determined by the *Aufmerksamkeitsort*."[24] But this statement apparently is not meant with its full significance. He means that attention determines *where* in visual space the image will be localized, not *whether* it will be located inside or outside the head.

There is in reality nothing to account for the phenomenal projection of either the AI or the EI excepting attitude. How true this is of the AI Braddock has shown: "An attitude of one sort may give the AI sharp outline and definite texture . . . . but as soon as this 'objective' attitude becomes ever so slightly critical, the localization ceases to be definite, the outlines blurred, and the image is again essentially soft and insubstantial."[25] This variability in localizing an image was long ago noted by Galton: "The place where the image appears to lie, differs much. Most persons see it in an indefinable sort of way, others see it in front of the eye, others at a distance corresponding to reality. There exists a power which is rare naturally, but can, I believe, be acquired without much difficulty, of projecting a mental picture upon a piece of paper, and of holding it fast there, so that it can be outlined with a pencil."[26]

It should be noted that the attitude of children is characteristically so concrete, so *dinghaft*, that a tendency naturally is present to project images if they are vivid enough and complete enough to simulate perceptual data. It is probably for this reason that confusion in the child's mind often occurs between appearance and reality.

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[20] Jaensch, *Die Eidetik*, 5.

[21] Jaensch, *Aufbau der Wahrnehmungswelt*, 1923, 52.

[22] Perhaps the commonest deficiency in technique is the failure of certain of the Marburg investigators to control such factors as serial order.

[23] Jaensch, *loc. cit.*

[24] Jaensch, *op. cit.*, 59.

[25] C. C. Braddock, this JOURNAL, 35, 1924, 166.

[26] F. Galton, *Inquiries into Human Faculty*, 1893, 99. The suggestion which Galton makes here that eidetic powers can be cultivated (or, if Jaensch is right about the recession of the ability in adolescence, re-cultivated) deserves investigation. An artist told the writer that he had with a little effort regained much of the eidetic prowess which he had as a youth.

It is perfectly natural for the adult also to regard the AI or the EI as existing in perceptual space. Habit associates all retinal stimulation with the physical world, and only with the "critical attitude," which Braddock speaks of, can we prevent an extraordinarily vivid image from cleaving to a ground and obeying the laws of visual stimulus.

That the AI and the EI are both characteristically projected does not prove any inherent relation between the two. Both are vivid images (the one probably peripheral, the other probably central). But since all vivid visual experience, with a few exceptions, arises in the outer world, the exceptions themselves take on, by virtue of association, pseudo-spatial attributes.

The "tension in the visual organ" reported for both the AI and the EI is only another factor in the same associational complex. Whenever visual space is under regard, accommodation and convergence produce their natural kinaesthesis. It is to be expected that when the gaze is directed in a kind of search for the projected image, that the same kinaesthesis will result. The tension indeed may be stronger than that obtaining for normal vision.[27] This condition is natural when we consider the conflict in kinaesthesis resulting from the tendency to fixate the projected image, and the simultaneous tendency to retract the muscles in face of a phenomenon which is recognized (normally) as having only a subjective existence.

(4) *Occasional resemblances.* Other characteristics offered to demonstrate a relation between the AI and EI are not claimed by the Marburg school to be invariable. The EI *may* under certain circumstances

(a) show a weight or intensity in respect to the ground as great as that of the AI;[28]
(b) show as little, or as much, three-dimensionality as the AI;[29]
(c) show as much narrowing of the visual field as in the case of the AI (*i.e.* a decrease in the distance from the center of the image to the position at which a peripheral stimulus is effective);[30]
(d) cohere with the projection ground as closely as does the AI;[31]
(e) tend (though rarely) to conform to Emmert's Law;[32]
(f) *be produced through fixation, as is the AI.*[33]

Now if these minor resemblances be considered, it will be seen that the last in the list rather naïvely contains a surrender.

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[27] Kroh, *op. cit.*, 28.
[28] P. Busse, *Zsch. f. Psychol.*, 84, 1920, 27.
[29] *Ibid.*, 31; E. Gottheil, *Zsch. f. Psychol.*, 87, 1921, 87 ff.
[30] Busse, *op. cit.*, 26.
[31] Krellenberg, *op. cit.*, 105.
[32] Herwig, *op. cit.*, 144.
[33] Krellenberg, *op. cit.*, 98 ff.; Herwig, *op. cit.*, 139 ff.

While wielding their measuring instruments the investigators have lost sight of the fact that that which they have called an EI is without doubt often an AI (since it "can be produced through fixation"). It is not strange under such conditions that the behavior of the image should conform to that of its own genus![34]

(5) *Types.* A large amount of the writing of the Marburg school is devoted to a relating of the EI and the AI through these questionable occasional resemblances. In place of a recognition of the simple fact that where close resemblances occur there is probably a true AI involved, there has grown up a cluster of "psycho-physical types"[35] to explain the behavior of the "eidetic" image which is in most respects *vorstellungsbildnah* and again so strangely *nachbildnah.*

The most striking creation of this nature is the theory of the "Unitary Type." An individual of this type "cannot produce negative AI; even with protracted fixation he is able to secure only an EI in natural colors. . . . Likewise he is unable to produce true MI, for these pass over immediately into EI. The original Eidetic Unity is likewise close to perception, and can often be interchanged with it."[36] An inspection of Krellenberg's records however, as Koffka has shown, reveals that he has much overstated his case.[37] Most of his argument is based upon the criterion of the *Invarianzgrad.* Klüver more recently has failed to find that any fruitful comparison can be made between the AI and EI in this regard, and states that he could not discover evidence for the "Unitary Type."[38] The writer has previously given his reasons for doubting the existence of the type.[39]

The doctrines of "types" seem to be to a large extent a superfluity. Many of the alleged resemblances between the AI and EI are due to the fact that the EI is often nothing excepting a misnamed AI; or, in cases where it seems to occupy a middle ground between the AI and MI, it simply shows the effects of the partial arousal of the complementary color processes. The only resemblance which is found to obtain invariably between the AI and EI is the tendency toward projection of the images into perceptual space. No vital connection need exist between the phenomena on the basis of this resemblance, since as it has been shown associational processes readily account for the projection in both cases.

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[34] Scola (*op. cit.*) and Koffka (*op. cit.*) have made the same criticism. In a recent counter-attack (*Zsch. f. Psychol.*, 102, 1927, 35-36), Jaensch accuses the *Gestalt* school of exalting unduly the physiological AI, even to the point of declaring it an open sesame for phenomena which are purely mnemic. In refuting Koffka, Jaensch points out the numerous differences between the AI and EI, thus stressing the essential affinities between EI and MI. In so doing, Jaensch seems unintentionally to lend weight to the argument of this paper, that whenever the EI can be proved to be free from confusion with the AI, it is merely a member of the family of MI (his declaration on p. 46 notwithstanding). Herwig too, it seems, (*op. cit.*, 140-146) leans backward so far to avoid the accusation of confusing AI and EI that he falls into a position of identifying the EI and MI. It is indeed difficult for the Marburg workers to occupy two stools at once, declaring at the same time their independence of either!

[35] Jaensch, *Die Eidetik*, *passim;* also W. Jaensch, *Monatschrift f. Kinderheilkunde*, 22, 1921, 1-10.

[36] Krellenberg, *op. cit.*, 117.

[37] Koffka, *op. cit.*, 152.

[38] Klüver, *op. cit.*, 185.

[39] G. W. Allport, *Brit. J. Psych.*, 15, 1924, 113 f. Jaensch has replied (*Die Eidetik*, 25 ftn.) without materially altering the force of the objections.

Turning to the other alleged affinity, that obtaining between the EI and MI, the evidence is more convincing. In fact, the EI readily show itself to be a special variety of MI. Some of the substantial identities may be summarized as follows:[40]

(a) both are influenced in content according to interest and other associative determinants;
(b) a brief exposure of the *Vorlage* serves to arouse both types of image; the slightly longer time usually required for the EI (*circa* 10 sec.) is simply because more detail and greater clearness are expected in it;
(c) the frequency of production conditions the clearness and intensity of both;[41]
(d) the richness and detail of both greatly exceed that of the optimum AI;
(e) the content of both is influenced by preceding images;
(f) both alter their content within the limits of experience at will;
(g) if colored at all, both tend to retain their original color;
(h) their behavior under conditions of distraction stimuli is the same;
(i) they persist as long as desired, and recur after intervals of time, sometimes bidden, sometimes unbidden;
(j) they grow indistinct and less accurate with disuse;
(k) they may arise spontaneously (*i.e.* without *Vorlage*), which of course is *never* the case with the AI.

Although in all these respects the EI shows that it is a member of the general class of MI, it does possess features which distinguish it in *degree*. It is completer, livelier, more accurate. The usual MI is skeletonized, faint, conceptual. EI, like hallucinations, are projected, but this fact does not of itself prove that these images are not essentially a variety of what is here called the MI.[42] The point of the present argument is that *whatever MI is lively and accurate enough to simulate perceptual data tends to acquire the kinaesthetic context which gives it the added attribute of pseudo-spatiality.*

Is it profitable to distinguish at all between the EI and the MI? Shall we not call the former merely a vivid visual image,[43] or else accept Martin's designation,[44] "projected memory image?" The writer is inclined to answer 'no'. For various reasons it seems desirable to retain a distinction in terms.

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[40] For a more complete account of these resemblances, see *opp. citt.* Kroh, Scola, and Allport.

[41] Jaensch, *Ber. über. d. VII Kong.*, 1922, 26.

[42] Still equivalent to *Vorstellungsbild*, not *Errinerungsbild*. Cf. H. Ebbinghaus, *Grundzüge der Psychologie*, I, 1919, 568.

[43] As C. Fox insists upon doing: *Educational Psychology*, 1925, 81.

[44] L. V. Martin, *Zsch. f. Psychol.*, 61, 1912, 321 ff.

In the first place, the signal discovery by Jaensch, that this particular variety of MI is a dominant characteristic of the child's mental content but not of the adult's, necessitates a distinctive term of reference. Secondly, certain of Jaensch's inferences regarding the probable significance of eidetic imagery for aesthetics, education, epistemology, and psychology merit careful investigations. For this reason it is well to retain his terminology.[45] Finally, it seems desirable always to discriminate between members of the family of MI as fully as possible, for only through such analysis can the ultimate problems of imagery receive light.

In conclusion the writer wishes to express his sense of great obligation to Professor Jaensch and his co-workers. Even though the eidetic image be no more than a vivid, accurate, projected type of memory image, its nature and its significance lay almost unrecognized until the enthusiastic explorations of the Marburg school. To indicate certain inadequacies in method and in theory is not to deny to these investigators a grateful recognition of their work.

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[45] For a discussion of English equivalents, see Allport, *op. cit.*, 100 (footnote).

EYE-MOVEMENTS AND VISUAL AFTER-IMAGES

By CARL N. REXROAD, Ohio State University

The author accidentally noticed that, after the tip of an electric light bulb had been fixated, the after-image which appeared when the light was switched off drifted in the direction of the filament from the tip. After fixating various points on the bulb, the direction of movement would always be that of the filament from the fixation point. On close scrutiny he was convinced that the drift was due to a turning of the eyes. This suggested a relation between eye-movements and after-images and led to the investigation here reported.[1]

Method of developing the after-image. In all parts of the investigation after-images were developed by requiring the *S* to fixate for 20 sec. a small square to one side or above or below a 1½-in. brightly illumined one. These 5 squares were cut in the center of a 2½ x 2½ ft. gray cardboard, the 4 small fixation squares being ½ in. from the edge of the large or stimulus square. Back of these was placed a piece of ground glass and back of it a 40-watt light. The bulb was mounted in a chalk box, the ground glass replacing the lid. The chalk box was placed upon an adjustable cross arm between two uprights. The *S* stood with the squares 3 ft. directly in front of him, and adjustment was made so that they were on a level with his eyes. Work was done in a dark room.

PRELIMINARY STUDY

The first problem was to determine whether drifts similar to those accidentally noticed occurred for others. *S*s were instructed to fixate one of the small squares designated by the experimenter. The room lights were turned off and the stimulus light turned on. When the stimulus light was turned off, the *S* was requested to report the onset of the image and then was asked if it were standing still. Ten *S*s were employed, an after-image being developed for each *S* with fixation on each of the small squares. Without exception movement was reported, and always the direction was that of the large square from the fixated one, *e.g.* when the large square was to the right of the one fixated, movement was to the right.

These *S*s and the author acting as *S* noted several other points that might be brought out with fuller instructions and with other procedures, and as a consequence further investigation was developed along three lines.

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[1] The author wishes to acknowledge the assistance of Edward R. Pfleiderer, who efficiently carried out the routine work of the experiment.

MAIN EXPERIMENTS

PART I

Procedure. *S*s in this part of the experiment were given the following typewritten instructions and were allowed to ask questions concerning them:

> "When the light back of the cardboard is turned on, you are to fixate one of the small squares. The experimenter will tell you which one. Be sure to hold your eyes continuously on the square designated. When the light is turned off, try to retain your fixation toward the same place. An after-image will appear, and you are to notice whether it moves; if so, notice the direction of movement and the number of times movement occurs. Be sure, however, to retain your original fixation point. When the image has disappeared say, 'Now,' at which signal the experimenter will turn on the room lights. Just as you say 'Now' fixate the place where you consider the square is, and be prepared to locate this point for the experimenter."

With these instructions 25 *S*s were used. Each reported 8 after-images: 2 for each of the 4 fixation-points.

Results. The results are shown in Table I.

TABLE I

Direction of large square from small	Direction of drift	Drifts		End-fixation	
		Av. number	Av. distance	Position	Av. distance
Right	Right	5.3	10.2	32R 11R+D 7R+U	5.3
Above	Upward	5.3	10.0	38U 12U+R	5.0
Left	Left	5.5	11.9	34L 11L+D 5L+U	4.8
Below	Downward	5.9	13.6	38D 12D+R	5.4

Columns 2, 3, and 4 of the first row are made up from 50 reports similar to the following: "The image moved straight to the right. It would move out about 12 in. and then disappear and then reappear near its original position and again move to the right about the same distance. It did this 7 times." It will not, the author believes, be questioned that the eyes turn, and that when they have turned a maximum they jerk back, due to the intra-organic factors. Nearly all *S*s who were for the most part students in elementary psychology, noted their own eye-movements and were quite impressed by their inability to control them, *i.e.* to keep the image from drifting. Further evidence of eye-movements was secured in one phase of the preliminary studies. Faint general illumination was used while the after-image was present, and by watching the *S*'s eyes the *E* could tell when the image was drifting, the direction of drift, and the instant of disappearance.

Columns 5 and 6 of Table I are derived from the position given by the *S*s as that where they considered the stimulus to be after the image had disappeared. These positions were indicated sometimes by pointing and sometimes by verbally directing *E* until he touched the position. The fact that the position given was always in the direction toward which the image had drifted indicates that there was not only eye-movement but also head and body movement. Three *S*s made such extensive body turns that their data could not be included. The extreme case was encountered when one *S*, who had fixated the small square below the large one, toppled over backward, reporting that he had no suspicion that he was leaning backward until balance was irretrievably lost. He, as well as the other two who made these marked body movements, was quite surprised to find himself, after fixating the square to right or left, facing far to the left or right, and this surprise continued through the various trials.

Discussion. Assuming that the drifts are due to eye-movements, the question arises as to the explanation of the direction of the eye movements. One is immediately reminded of Loeb's studies of heliotropisms and his theory to account for them. If the eye be regarded as a positively heliotropic organ, stimulation from the left should induce a heightened tension in the recti attached to the left side of the eyeball, from above in the superior recti, etc. Eye-movements of infants in fixating and following a bright light conform to this theory. The eye-movements which give rise to the apparent drift of images in the present investigation also follow these rules. During stimulation the tendency of the eyes to fixate the center of the large square is overcome by reënforcement from the instructions to fixate a designated small square, but when the stimulus light is turned off, there are no impulses arising from the small square to be reënforced and the conditions—sensory, neural and muscular—imposed by the highly illumined 1½-in. square come into evidence. The persistence of these conditions may be accounted for in terms of continued excitation in the retina, by the continuation of the gradient pattern imposed on the nervous system, or by the activity of homedetic arcs. In short, it seems safe to conclude that the pattern of stimulation for each position of fixation sets up a particular and persisting pattern of tension in the eye muscles and most probably in the head and bodily musculature as well. If the individual were to fixate the center of the large square, it is to be supposed that a tension pattern would be imposed, but that due to antagonistic action of the muscles no movement would occur. No drift was reported under such conditions by *S*s employed in the preliminary studies.

This part of the experiment lays bare the positive heliotropism of the eyes and brings out the fact familiar to all investigators that images will drift with eye-movements. There is, however, one observation that suggests a more intimate relation between the image and tensions in the eye muscles. When the eyes jerk back to the forward position, the image disappears, as though the tension which is imposed by the stimulation were a necessary correlate of the image or, as the behaviorist might say, were the image. If, for example, stimulation has been such as to impose a movement to the right, movement to the left from intra-organic stimulation destroys the image, which is practically equivalent to saying that when the image is destroyed it is destroyed. If this intimate correlation or identity of image and eye tensions exists, *S*s should not report the presence of an image when they are caused to make movements incompatible with the one imposed by stimulation. The second and third parts of the investigation were devised to discover whether such reports would be made.

PART II

Procedure. In the second part of the experiment all the *S*s fixated the small square to the left, so that the imposed eye-movements would be to the right. As soon as the image had appeared, dim lights were turned on and the *S*s were instructed to fixate and to follow a knot on a string. The knot could be moved by *E* slowly to the right or left.

Results. When the knot was moved to the right the image remained, proceeding along with the knot. The eyes turned smoothly without any evidence of the characteristic fixation pauses and jumps. When, however, the knot was moved to the left, the first reports were that the image stood still or even moved to the right, and then disappeared when the eyes jumped to the left to catch up with the knot. It was apparent to *E* and the *S*s that the eyes were not really following the knot in these cases, and hence the *S*s were instructed to make a greater effort to follow the knot closely. With this added effort 19 of the 25 *S*s, who were either members of the staff or graduate students, succeeded in following the knot to the left, and reported that the image was absent while doing this. The knot was moved as far as 18 in. The other 6 *S*s continued to make jerky movements while attempting to follow to the left, but all reported that the image appeared when they turned their eyes right to go back, as it were, to see if they could not pick up the image. As soon as it appeared, they would turn back left to catch the knot, when the image would disappear. *E* noticed that they made movements of a nystagmic sort rather than pauses and jumps, the rivalry between the movements imposed by the stimulation and those imposed by attempting to follow the knot being thus evident. While the movements imposed by the stimulation were in ascendancy, the image was present, and while those involved in following the knot were in ascendancy, the image was absent.

PART III

Procedure. After the image had been developed, *S*s in this part of the experiment were instructed to fixate at a signal one after another four X-marks which were made with pencil on the cardboard.

The X-marks were 4 in. from the center of the stimulus square, one above, one right, one below, and one left. The X-mark to be fixated was designated by calling out 'above' or one of the other words that designated the other 3 positions. At this signal the X-mark was fixated and as soon as an image had appeared while fixating the particular X, *S* said 'Now' and again fixated the center of the square until a signal was given to fixate some other X. After a stimulation the *S* was requested to fixate 12 times. 3 times on each of the 4 X-marks. The order in which the fixations were called for was made out beforehand to give a random series. Each *S* was given 20 stimulations while fixating each small square. Thus, after fixating the small square to the right, fixation was made 60 times for each of the 4 X-marks and likewise after fixating each of the other small squares. To fixate the X in the same direction from the center of the square as the direction of the large square from the fixated square required a movement compatible with that imposed by stimulations; to fixate the X in the opposite direction required a movement directly opposed to the one imposed by stimulation, and to fixate either of the other two X-marks required a movement at right angles to the one imposed by stimulation.

The time taken for the image to appear after making any one of these movements was secured by means of a stop-watch. The watch was started simultaneously with the giving of the signal to fixate a particular X and stopped when the *S* said, 'Now.' This time, shown in Table II, includes the time consumed in making the fixation and *E*'s reaction to 'now." To secure the true time for the appearance of the image after fixation, there should be subtraction. The amount to be subtracted was, approximately, one second, being found by giving a signal and having *S* report 'now' as soon as fixation was achieved.

Results. The results are shown in Table II.

For M the table shows in the first column of figures that after stimulation with the large square *below* the point of fixation it required for the report of the appearance of an after-image an average of 1.3 sec. when requested to fixate the cross in the same direction (below), an average of 3.1 sec. when directed to fixate the cross at right angles (left or right) and an average of 4.1 sec. when requested to fixate the cross in the opposite direction (above); 1.3 and 4.1 are averages of 60 reports, while 3.1 is an average of 120 reports.

It will be noted that in all cases the average time required to report an after-image is shortest when the *S* is directed to fixate the X in the same direction, the movement required being one compatible with the tension imposed by stimulation. For the 3 *S*s in order this average time is less by 1.7, 1.4, and 1.0 sec. than the average time when the *S* is requested to fixate X-marks at right angles, differences which are 7.9, 18.5, and 23.8 times the sigmas of the differences, and is less by 2.6, 2.7, and 1.8 sec. than the average times when the *S* was directed to fixate the X in the opposite direction, differences which are 26.3, 20.5 and 25.7 times the sigmas of the differences. The average time for reporting an after-image when the *S* fixated X-marks at right angles is 1.1, 1.2, and 0.8 sec. less than the average for the X in the opposite direction, differences with reliabilities of 10.5, 9.0 and 12.1. It seems, then, that the differences are unquestionably significant ones.

TABLE II

S	Position of X	Direction of large square from small fixation square				
		Below	Above	Left	Right	Av.
M	in same direction as large square from one fixated	1.3	1.1	1.0	1.0	1.1
N		1.2	1.0	1.1	1.1	1.1
O		1.2	1.2	1.0	1.0	1.1
M	at right angles to direction of large square from one fixated	3.1	2.9	2.5	2.7	2.8
N		2.8	2.4	2.4	2.3	2.5
O		2.1	2.2	2.2	2.1	2.1
M	in opposite direction	4.1	3.6	3.6	3.5	3.7
N		4.1	3.6	3.9	3.2	3.7
O		2.8	3.0	2.8	3.0	2.8

Discussion. The differential times found in this part point to the same relation between the image and eye-movements as did Part II. Suppose that stimulation has been such as to impose a tension which tends to turn the eyes to the right at a certain rate. If, in responses to a signal to fixate the X to the right, the eye is turned more rapidly, the image is not reported, but as soon as this movement is slowed by virtue of the X coming into the line of vision the image appears; while if the signal given is for fixation of the X to the left the fixating movement must be stopped and movement in the opposite direction started before the image appears. *S*s reported that the image appeared almost simultaneously with the seeing of the X to the right and that the image continued to move on to the right, while it did not appear until after the X to the left had been held and fixated for some time. They further reported that after a time the fixation on the X to the left began to waver, and that as this wavering began, the image appeared and started moving to the right. The time differences and the reports thus bring out the fact that there must be movement of a particular sort when the image is reported.

General Discussion

The correlation of image and movement had been proposed by Washburn as a theory without direct evidence. She states, "the power of getting true spatial images seems to be bound up closely with the power of moving the sensitive surface. We get our best tactile space perceptions through active touch, involving movements of the hands and fingers; our visual perceptions are profoundly influenced by eye-movements."[2] The present writer takes this as a concise statement of the thesis presented in Washburn's book, *Movement and Mental Imagery*. It seems to him that she is not far from proposing identity between specific movements and particularized images, in which case the term 'image' describes not a mental experience but a form of behavior. On the basis of the present investigation it seems that this identity can as well be maintained as can that of emotions and visceral activities or of thoughts and implicit substitutive responses. In other words it seems justifiable to hold that the image is a specific pattern of muscular tension.

This view may be argued from the data obtained in the studies of illusions. The illusion has been regarded by various investigators as depending upon the eye-movements made. The behaviorist would prefer to drop this dualism with one thing depending upon another; he would prefer to say that seeing *is* in part eye-movements, rather than that seeing depends upon eye-movements.

Also in line with this argument is the proposal by Joseph Peterson to discard the term 'local sign'.[3] He argues that localization is localization and not a shading or peculiar and undefinable quality of sensations. To perceive the location of a stimulus is to orient or tend to orient in a specific way. It is but one step beyond this to say that to perceive the form of a stimulus patch is likewise to orient or tend to orient in a definite manner or, as we have stated, to have an image is to have a specific pattern of muscular tension.

This view of the image has been proposed by Weiss[4] as the necessary one for behaviorists, but he was forced to admit the inability of behaviorists to localize the contractile effects in responses called imaginal. This investigation, it seems, makes it possible to refer the contractile effects of visual images rather largely to the recti. These contractile effects do not ordinarily reveal themselves because of the antagonistic actions involved but under the conditions established in this experiment they result in movement because of the unequalness of contraction in antagonistic muscles.

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[2] M. F. Washburn, *The Animal Mind*, 3rd ed., 1926, 237.

[3] J. Peterson, Local signs as orientation tendencies, *Psychol. Rev.*, 33, 1926, 218-236.

[4] A. P. Weiss, *A Theoretical Basis of Human Behavior*, 1925, chaps. XI and XII.

If images are regarded in this manner, then the appearance of memory images becomes readily understandable in terms of the conditioned reflex. An object which gives rise to a definite pattern of visual stimuli would give rise to a definite pattern of tension in the recti and bodily musculature and other associated stimuli would become capable of eliciting this tension pattern, so that the individual would seem to see the object in its absence.

SUMMARY

(1) After a small faintly illumined square beside a brightly illumined one had been fixated for a time the eyes will, when the stimulus lights are turned off, turn in the direction of the brightly illumined square. An image will be reported as present and drifting in the direction of the eye-movements.

(2) When the eyes make some other movement incompatible with that imposed by stimulation no image is reported. The eyes will make such a movement after they have turned a certain distance (Part I), and also when the individual is directed to follow a knot moving in the direction opposite to the direction of stimulation-imposed movement (Part II), or is requested to fixate a point in the opposite direction (Part III).

(3) These findings lead to the conclusion either (a) that the visual image and recti tensions are correlated, or (b) that the visual image is in large part recti tensions. The author favors the latter conclusion.

AN INTERPRETATION OF AESTHETIC EXPERIENCE

By FREDERICK H. LUND and ANNA ANASTASI, Bucknell University

The art forms and their object, the aesthetic experience, have always claimed a fair share of man's interest, and, in fact, have been cultivated with a perseverance almost unequalled in any other sphere of human endeavor. But this interest, so generally accorded to aesthetics, has been attended by only few and hesitating attempts at scientific determination in the field. Doubtless the extremely subjective nature of aesthetic evaluations and the absence of adequate technique in dealing with them have been responsible for this lack of scientific treatment. Theories on the subject have been plentiful enough, but they are sadly wanting in agreement and fall short of any systematic account of the conditions uniformly associated with the aesthetic.

The shortcomings of earlier accounts are also attributable to an attitude purely objective, *i.e.* an interest in the materials of art (paintings, sculpture, music, etc.), in the stimulus or the objective medium, rather than an interest in the experience itself and the principles determining it. But for the psychologist, at least, beauty and the aesthetic cannot inhere in the object apart from the subject or observer. It is this circumstance of viewing art objectively instead of in its stimulus-response relationships which has been responsible for the general absence of recognition of common elements in the diverse materials which arouse the aesthetic attitude. The literary forms of art are treated apart from the plastic forms, the visual apart from the auditory, the spatial apart from the non-spatial. Objectively, they are, of course, distinct. Objectively, there is no similarity between music and architecture, between painting and sculpture, between a gem and a poem, a symphony and a cathedral. Subjectively, however, they occasion similar feelings which make them classifiable under one head, *the aesthetic.*

The object of the present study is to present a new method of experimental approach, and on the basis of available data to seek a more inclusive interpretation of the principles operating through the mediumship of different forms of artistic material.

For the artist, the object of his profession must be to express beauty in form, color, sound, speech, or movement. For the

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psychologist, the object of art is to produce certain desired emotional experiences by means of stimuli most effective to that end. The artist is engaged in the construction of objects and combinations of stimuli through which he may perpetuate some desired but too fleeting experience, and convey to others the emotional states he himself finds good. What we want to know, then, is not simply the nature of the stimuli most effective in this respect, but the characteristics of the reaction-sequence which has the desired tonal quality.

That some degree of uniformity exists in the stimuli having aesthetic value for different individuals should help us in discovering the inherent factors providing for the agreeable tone of the experience. It is no doubt because of the greater uniformity in the sound stimuli having this tonal value that modern experimental work has been so largely limited to the elements of musical composition. This sphere of artistic endeavor may also have appeared more accessible because the elements of construction are so much more obvious and thus lend themselves more readily to scientific treatment. In view of these circumstances we may profitably take the aesthetic experience attending music as our point of orientation. No detailed discussion, however, can be given either of the experimental work in this field or of the results obtained. For these we suggest to the reader Bingham's *Studies in Melody* as one of the best references.[1]

Aesthetic value is found to inhere in tones or musical sounds, not when heard singly, but when combined to form a *melody*, defined by the musician as a rhythmic composition of *related* tones, dominated by a single element, the *tonic*, and expressive of a characteristic motive or idea. This statement must be regarded as an objective description of the stimuli, and is not psychological. Introspectively, the notes forming a melody and capable of arousing the aesthetic attitude seem to follow each other with a certain naturalness and inevitableness. One seems *prepared* for the successive reactions touched off by the series of stimuli. What the musician calls *tonality*, or relatedness of the melodic series, is felt subjectively to be a set of expectations aroused with the occurrence of the leading note, and progressively organized as the melody proceeds.[2]

Now may it not be that somewhat similar conditions obtain where visual stimuli gain aesthetic potency, and that our reactions to spatial elements acquire aesthetic value when they are so *related* that each leads naturally over into and prepares us for the next. What could we mean otherwise by *harmony of construction* in a visual presentation? Is it not possible that just as the demands or expectations of the hearer must to some extent be fulfilled if a series of notes is to be accepted as a melody so, too, the anticipations and preparedness to react in visual observation must to some extent be satisfied in order that the presentation may qualify as aesthetic?

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[1] W. Van Dyke Bingham, Studies in melody, *Psychol. Monog.*, 12, 1910, no. 50, vi, 88

[2] Cf. Bingham, *op. cit.*, 37.

The resemblances in musical design and spatial design must indeed be obvious even to superficial observation. Both seem to require balance and symmetry. In both, harmony is wrought through the dominance of a given element or through the presence of some principle of integration. Again, just as in musical design there is an 'antecedent and consequent' relationship between the notes, a re-echoing of the corresponding parts, so, too, in spatial design the lines and forms drawn in one part demand complementary construction in corresponding parts.

These, in fact, are the conclusions to be drawn from a series of simple experiments performed by the writers. While the investigators of the aesthetics of form have usually provided their subjects with the materials considered aesthetic, and then asked for an order of merit arrangement of the material, or a rating of it on an aesthetic scale, it was believed to be equally, if not more, desirable to use a 'completion method' in the case of which only certain elements were furnished and the subjects required to add to these such other elements as would give the completed figure the most aesthetic effect.

130 *Ss*, all students of psychology in Barnard College, were put through these experiments. The material was arranged in three sets, A, B, and C. The instructions for Set A were:

Add to each figure such lines or elements as will give to the presentation the most satisfactory, pleasing, or aesthetic effect, without making the completion too great a departure or elaboration of the given elements.

In Plates I and II the figures under the head of 'original figures' are representative of the structural elements in Set A. The numerals below the completions indicate the number of times (expressed in percent) these particular completions occurred. In these results a strong tendency to complete the figure in accordance with some familar object becomes apparent. In fact, the given figures are elements of familiar figures more or less completely reproduced. Thus in Plate I Figs. 1–8 represent elements of a star; Figs. 9–13, a flower design; Figs. 14–16, sectors of a circle. In Plate II Figs. 1–4 represent elements of the Greek cross; Figs. 4–10, lines belonging to a vase. It is not surprising that, as in Figs. 1–8 (Plate I), the tendency to draw the star should increase according to the redintegrative efficacy of the figure, or according to the completeness with which the elements composing the star are present. What shall be drawn depends upon the suggestive or redintegrative value of the element present. In Figs. 14–16

(Plate I) the tendency to draw the crescent is greatest when the arc is presented in the familar position most suggestive of the crescent. The importance of the *familiarity principle* will be given more attention later in the discussion. It doubtless depends on the fact that reaction tendencies already established in the individual are, other things being equal, more pleasant than unfamiliar ones. In music the principle asserts itself most strikingly in the case of melodies which become more pleasing with repetition.

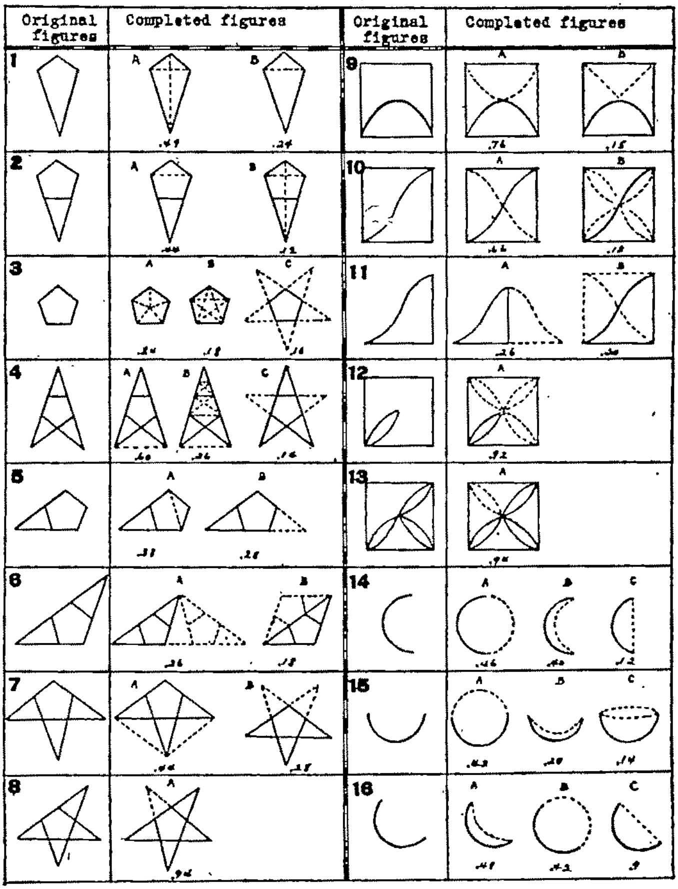

PLATE I. COMPLETION OF SOME OF THE FIGURES USED IN SET A
The numerals below the figures indicate the number of times (expressed in percentages) these particular completions occurred

Another outstanding feature of the completions is the strong desire for balance and symmetry. These requirements belong to the alphabet of aesthetic theory and are easily traced to an experiential basis. Lack of balance suggests instability and unrest, while the equal weighting of both sides with reference to a central point or fulcrum gives the spectator the agreeable sense of poise and stability. We like symmetry because of its suggestion of orderliness, which in turn makes for intelligibility and ease in grasping the significance of the whole. Such orderliness appears to be most effective when the repetition of the motive or dominant element is introduced with variations, without, however, destroying the unity of the whole. In fact, *unity in variety* is a feature common to most aesthetic forms. *Unity facilitates reaction and diversity is mentally stimulating.* The chief difference between primitive and modern art is the extent and subtlety with which variations are introduced while still preserving a certain uniformity with balanced relationships. The chief difference between popular and classic composition (most apparent in music) is in the amount of diversity in theme and construction.

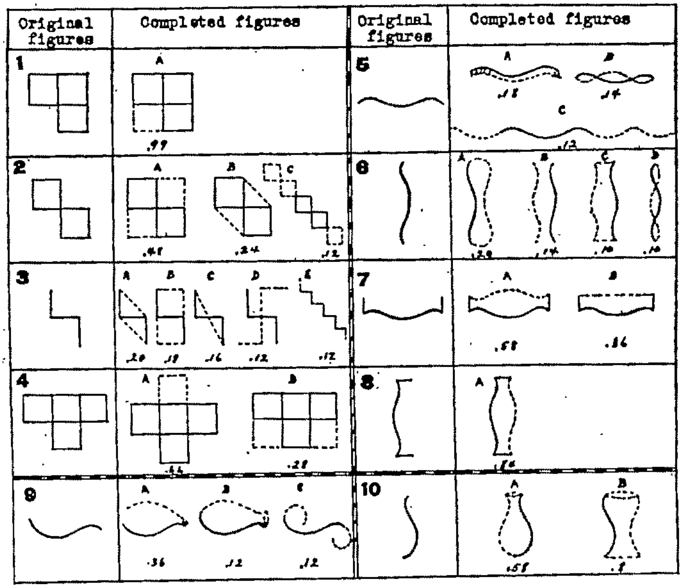

PLATE II. COMPLETIONS OF SOME OF THE FIGURES USED IN SET A
The numerals below the figures indicate the number of times (expressed in percentages) these particular completions occured

The conclusions were confirmed in the subjects' introspections, the most representative of which we quote:

"The suggestion of rhythm obtained through repetition was pleasing."

"Desired to make the figures balanced and symmetrical."

"Figures were most pleasing when the lines could be made into a somewhat familiar figure."

"It seemed that my personal experience had something to do with how the figure should be in order to be most pleasing."

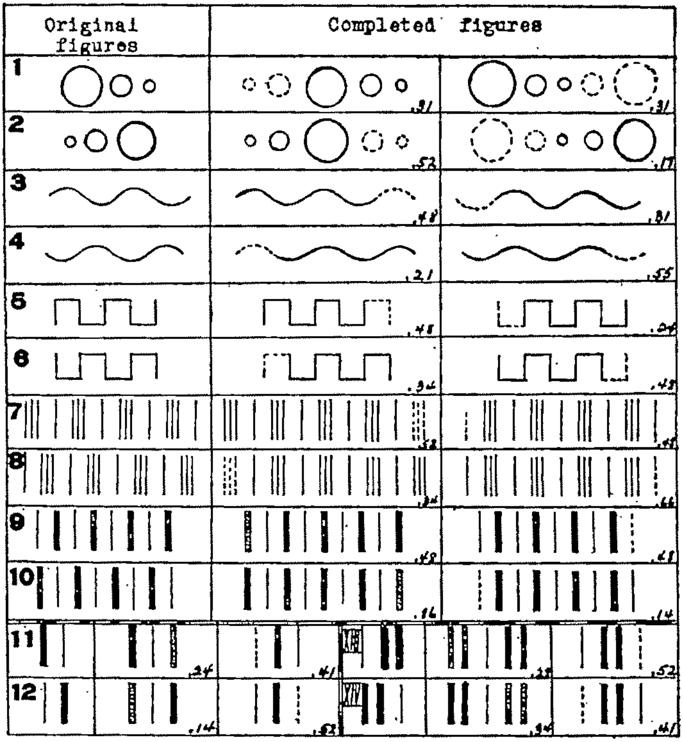

Plate III. Completions of Some of the Figures used in Set B
The numerals below the figures indicate the number of times (expressed in percentages) these particular completions occurred

Set B called for completion of the figure through the addition or duplication of elements given in the figure. The results of typical figures are given in Plate III. A difficulty which became apparent from a preliminary experiment was the strong tendency to add to the right of the figure because of motor habits in writing. To take due account of this as a determining factor in the completion it was necessary to present each figure a second time in the reverse order of the first. Only after comparing the results of both arrangements does the true tendency become evident. Thus, it is only when the results of both Fig. 1 and Fig. 2 are considered that the preference for the descending series with the heavier element in the center (doubtless due to the stability principle) becomes apparent.

In all completions symmetry and balance are observed by preference for beginning and ending on the same element.

In Figs. 3, 4, 5, and 6 (Plate III) the interest of the experiment was in determining whether there was any preference for beginning and ending on a descending element rather than an ascending one. The results show no marked preference.

Comparing the results of Figs. 9 and 10 with those of 11 and 12, it appears that in the former there is a preference for beginning and ending on the major element. This has its analogy in music in the preference for a return to the tonic, or for beginning and ending on the tonic chord. This tendency to end on the major element in spatial series has also been observed by Rowland.[3] The *Ss* seemed to feel, according to their introspections, that such an ending gave greater finality to the series and brought it more satisfactorily to a close. That Figs. 11 and 12 should show preference for ending on the 'lighter' element is probably due to the fact that they are apprehended as figures rather than patterns or rhythmic series, and that the preference, therefore, conforms to the principle observed in connection with Figs. 1 and 2. That Figs. 7 and 8 give slightly different results from those shown in Figs. 9 and 10 throws some doubt on the apparent inferences to be drawn from the latter, and makes further experimentation desirable.

In the case of Set C the *Ss* were merely to indicate their order of preference for the figures presented singly and in series. Some of these figures and their serial combinations are presented in Plate IV. The preferences for first place expressed in terms of ratio were:

Figure	Ratio of Preference			
	a	b	c	d
1	1 :	17 :	18 :	25
2	1 :	3 :	4	
3	6 :	9 :	5	
4	1 :	8		
5	1 :	5		
6	3 :	4		
7	3 :	4		

Evidently there is a decided preference for serial combinations as compared with single figures. In most cases the single figures were given last place. The strength of the preference for series, however, depends somewhat upon the manner of the combination and the nature of the element from which the series is constructed. A figure itself disagreeable and obviously lacking in aesthetic value is not specially enhanced through repetition in some design. The results showed also a preference for serial combinations with variation as compared with those without variation. According to the introspective accounts the series were preferred because:

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[3] E. H. Rowland, The psychology of repeated space forms, *Harvard Psychol. Studies*, 2, 1906, 225.

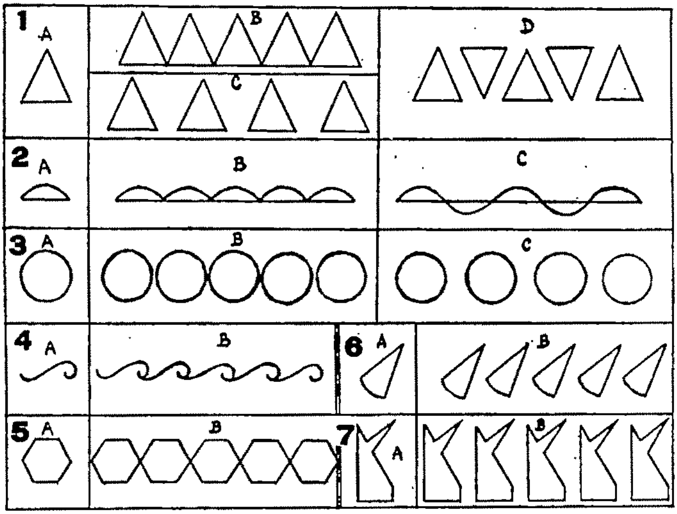

Plate IV. Some of the Single Figures and Corresponding Series used in Set C

> "They give one a feeling of rhythm," "they have more meaning than a single figure," "there is a feeling of familiarity in design," "series are more unified and meaningful," "repetition and symmetrical arrangement produce a stronger impression," "they represent more significant and complex objects," and "are more interesting."

The circle repeated in a series did not seem to gain in the *Ss*' evaluations. It was considered "complete in itself and perfectly satisfying as it stands." That it rarely appears except singly in designs may also have had something to do with placing the series at a disadvantage.

We may conclude that in general pattern effects with their rhythmic suggestion are more agreeable than single figures for the same reason, doubtless, that rhythmic combinations of notes are more agreeable than single notes. A series of visual or auditory reactions, if more or less similar, must progressively facilitate each other. Moreover, a serial combination constitutes a more complex presentation which may, nevertheless, easily be apprehended mentally.

The frequency with which the subjects referred to the series as having more "meaning," or being more "significant," may be due to the fact that in the field of aesthetics the pleasantness attaching to a situation is usually referred to as its "meaning," and this pleasantness being greater for the series than for the single figures gives the former more "meaning." If a thing is beautiful, that seems to be meaning or purpose enough for its existence.

The results of our study, while not conclusive, are quite suggestive as to the nature of the aesthetic experience, particularly when viewed in connection with the findings of other investigators. At the present stage of experimental inquiry, however, no attempt at specific formulation of a doctrine sufficient to cover the facts can be considered final. At best such a doctrine might indicate the direction of future inquiry. Before presenting our own interpretation we shall summarize some of the traditional views on the subject.

HISTORICAL

Due to the philosophical and deductive methods employed in the past, a great variety of doctrines on the nature of the aesthetic has arisen. The diversity of the theories makes it impossible to review them here. They may, however, without great injustice be treated summarily under two heads: the sensation theories, and the motor theories.

(*A*) *The sensation theories.* Possibly due to a lack of distinction between sensory reactions, as such, and the perceptual and associative reactions, the concept of the aesthetic 'feeling' as a quality inherent in the sensation has occupied theorists a good deal. To the uncritical observer it would seem that the beauty and delight of his experiences is present in the direct impress of external objects on his senses.

One of the simplest and most naïve expressions of the sensation theory is presented by Grant Allen,[4] who contends that the aesthetic feeling is "the concomitant of the activity of a well-fed and under-worked sense organ." He attributes "the pleasurable but non-aesthetic" value of play to the *active organs*, *i.e.* the muscles, the aesthetic feelings to the *passive organs*, *i.e.* the sense organs. The author overlooks the simple fact that if the pleasure of play is in the activity of the muscles it, too, must be a sensory phenomenon.

It is a matter of serious doubt whether there is any aesthetic value in isolated sensations apart from associative values. The painter does not concern himself simply with beautiful color schemes without formal or associative significance; nor does the musician expect his audience to find delight in a mere succession of unrelated notes. That is to say, while certain auditory and visual sensations, even when occurring singly, may be more pleasing than others, they are not, strictly speaking, aesthetic.

(*B*) *The motor theories.* The motor theories must stand or fall with the sensation theories if their supporters have in mind the explicit or overt muscular innervations, as such, since the only awareness arising from these innervations when visual and cutaneous sensations are excluded, comes from the kinaesthetic sensations. The motor theorists have maintained that the pleasure derived from music, from painting, or from sculpture is, due to implicit motor activities called forth by the stimuli; the observer projects himself into the motor activity suggested by the work of art; he has *Einfühlung* or *empathic* feelings in connection with the suggested activity.

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[4] Grant Allen, *Physiological Aesthetics*, 1877, 5-57.

But granting this importance of the *implicit* motor responses, why should the aesthetic value of the experience not be greatly enhanced if the observer of the work of art, instead of being content with the mere implicit reactions, actually assumes the posture of the figure in marble, or carries into effect the activity it suggests? Is the aesthetic value of the decorative dance in the muscular sensations of the dancer, or is it in the associative and formal value which it has for the observer?

Why is a graceful line or curve more pleasing than an irregular one? The answer of the motor theorists is that the movement of the eye in following the curve is smoother and therefore more agreeable than in following an irregular line. If we close our eyes, however, and attempt an easy rolling movement there is nothing particularly aesthetic about the experience. Furthermore, the well-known experiments of Dodge, Stratton, and others on eye-movements show that the eye in taking a curve does so by a series of jerky movements with intermediate rest periods.

Puffer,[5] after her subtle observation that balanced effect in a picture is in reality attained through the equal ability of both sides to hold the attention, interprets the expenditure of attention as "the measure of the *motor impulses* directed to the object of attention," and maintains that, because our organism is a bilateral one, we experience pleasure in a balanced picture because it fulfills the demand of our system for a "set of reactions corresponding to the organism as a whole." Doubtless, activity in which motor impulses are balanced with respect to the two sides of the body is more agreeable than otherwise (the musculature of both sides being equally developed), but to take this analogous or correlated event and reason to a causal relationship with the aesthetic experience is hardly warranted.

THE AESTHETIC EXPERIENCE AS PREPAREDNESS AND FACILITATION WITHIN CENTRAL REACTION MECHANISMS

(*A*) *Preparedness and facilitation.* In seeking a more adequate interpretation of the aesthetic we shall avail ourselves as much of the facts relating to music as of these derived from the experiments on visual forms. All art forms, it is believed, depend ultimately on the same principles.

That it is pleasant for the organism to react (mentally, overtly, or both) when the response mechanisms concerned are in a state of readiness, and unpleasant in the other event, is a commonplace. Yet this commonplace has much to do with our aesthetic feelings. This is particularly evident in music and in the art forms where the order and temporal spacing of the stimuli are essential to the production of the desired affective state. Where this is true the 'naturalness' and the desirability of the sequence must be due to the integration of successive elements—or progressive preparation in the reaction sequence—

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[5] E. D. Puffer, *The Psychology of Beauty*, 1905, 109 f.

each reaction predisposing the individual to the reaction which follows. In music the roughness of the sequence which "grates on one's nerves" as compared with the sequence which is smooth and pleasant must be due to the condition in which each successive element finds the brain patterns. If, in this progressive preparation, what comes is in accordance with one's expectations, it is pleasing, if not, it is displeasing and discordant. The unity of the melody as distinguished from a mere succession of notes would seem to arise from the fact that the hearer is prepared in the melody to react to each note that comes because it falls in with the reaction sequence already in progress. A similar explanation may be given for the *feeling of relatedness* experienced with the accepted intervals of the harmonic scale. Intervals deemed most agreeable or consonant are those which 'fuse' or 'blend' most readily. But in so far as the degree of consonance is known to depend chiefly on the coincidence of overtones, there must be a certain coincidence or overlapping in the reaction patterns by which the related tones are apprehended. This circumstance provides for the feeling of unity and relatedness and must be attended by facilitation within the reaction mechanisms.

Notwithstanding his strong inclination toward the motor concept, Bingham, at the close of his experimental studies, makes the following statement which is entirely in accord with the above:

> "When tone follows tone in such a manner that the hearer can react adequately to each, when the response to the successive members of the series is not a series of separate or conflicting acts, but rather in each instance only a continuation or further elaboration of an act already going forward, then the tones are not felt as discrete, separate, independent, but as 'related' to each other."[6]

Now, what we contend is that a similar facilitation, a similar smoothness and absence of conflicting elements, is equally important for other art forms. Observe, for example, architectural designs with marked beauty and harmony of construction and note if this harmony and unity is not dependent on similarities in the structural elements, or upon a *relatedness* of these elements, comparable to the relatedness of harmonious musical intervals. The leading notes and repeated intervals of musical design are here paralleled by the repetition and variation of some dominant motive in structural or spatial design. The presentation has unity, is 'pleasing to the eye,' because, as we turn from one feature to another, we are more or less prepared to react to each new element on account of repetition or overlapping of elements in the design. In a beautiful picture the congruency, coherence, and balanced relation of its parts, bear the same tendency.

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[6] Bingham, *op. cit.*, 87-88.

In the completion experiments the subjects showed a strong demand for repetition of the dominant element, for unity in variety, for balance and symmetry, with one part repeating yet reversing the arrangement of the other. The pleasure attending the completion and realization of some familiar or distinct melodic trend is here matched by uniform demand in the manner of spatial completion which will give the most aesthetic effect.

At this point the objection may be raised that sometimes a quite unexpected stimulus combination may be pleasant. This goes to show that not all cases of aesthetic experience are cases of *fulfilled expectation*. But it is doubtful whether there are any cases in which we are not in a measure prepared for the successive reactions when these are agreeably toned. How, in the other event, could the feeling of unity and harmony arise?

Why do we like regular linear combinations and smooth graceful curves? Our answer is that such regularity and smoothness is pleasing because of the facility with which the linear constructions are taken in perceptually. Irregular combinations are unpleasant because each part calls for a new adjustment. Symmetrical arrangements have agreeable associational value because of their suggestion of law and order. Any one portion, whether of a smooth curve or symmetrical figure, is a key to the rest, just as the leading note of a melody is the key to its construction and arouses a 'tonality attitude' or preparedness for the entire sequence.

May it not even be that our preference for the 'golden section' (division of a line so that the whole line bears to the greater part the same ratio as the greater part bears to the lesser part) is also dependent upon the *preparedness-facilitation* principle, since the mental transition from reacting to the whole line to reacting to the greater part prepares us for the further transition to the smaller part?

The enjoyment of fiction and of stage performance is even more clearly an example of enjoyment attending the exercise of processes brought into a high state of readiness. Fiction and the stage furnish the stimuli which vicariously allow for the exercise of drives restricted in their normal outlet. Such restriction of reaction mechanisms native to our constitution naturally results in increased preparedness and demand for activity. In the art of the stage as in the art of fiction, the first part of the performance furnishes the setting and arouses the drive. The greater the difficulties encountered by the leading motive, usually symbolized in the hero, the greater becomes the state of preparedness for the desired end-result, and, in turn, the greater the pleasure attending the climax or resolution of the *motif.* Good story writing must first arouse some dominant interest or drive and then, by skillfully holding the reaction mechanisms in restraint, force interest and suspense to higher levels, thus effecting an increased state of preparedness. Lindbergh's dramatic flight across the Atlantic would have failed to arouse such general acclaim and emotional fervor had it not been for its favorable setting. Had the intrepid flyer crossed before others had failed, or even immediately after his own entrance on the arena when the weather for days became a hindrance, the accomplished feat would not have come with such poignant effect, since the suspense, and with it the increased preparedness for the end-result, would have remained on a much lower level.

(*B*) *Habituation and practice.* Assuming the correctness of the prepared-facilitation principle, we should expect to find that artistic constructions within certain limits become more agreeable with repetition, since repetition has the effect of facilitation. This is, in fact, the case. The importance of habituation and practice in connection with music is emphasized by practically all the investigators of the subject. Only the influence of habit and custom can account for the difference in accepted intervals in Oriental and Occidental music, and for the gradual admission of new intervals in the history of European music. Valentine[7] furnishes experimental evidence indicating that with practice tonal combinations at first considered disagreeable may later be received as equally agreeable with accepted intervals. He also calls attention to experimental work showing that children who have had no special musical training cannot distinguish between decided concords and discords until the age of 12 or 13. A similar observation is made by Bingham[8] in connection with some work on quarter-tone melodies. The *Ss* found these quite disagreeable at first but after repetition were disposed to modify their judgment.

Valentine[9] contends that "familiarity is the essential element in the idea of beauty," and supports his statement with the extent to which his *Ss*' liking for certain pictures and certain pieces of music was influenced by familiarity. He states:

> "Even an expert musician often refuses to judge a new symphony on the first hearing. It is only when he has mastered the main themes, when he is so familiar with them that he can recognize them when presented in varying guise, when he can discover unity in variety, that he thoroughly enjoys the music."

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[7] C. W. Valentine, *The Experimental Psychology of Beauty*, 1913, 105 f.

[8] Bingham, *op. cit.*, 40.

[9] Valentine, *op. cit.*, 109.

From ordinary observation we might conclude that the chorus of familiar songs does not lose qualitatively as the result of greater repetition. Again, older people are known to find greater satisfaction in the songs and music of their more plastic years, the older and more familiar sequences being more easily reinstated.

(*C*) *Association.* Just as habituation and practice may be treated as subheads under the preparedness-facilitation principle, so, again, the influence of associations upon aesthetic evaluations may be referred to the same principle, since agreeable associations, like habituation and practice, have marked facilitating effects, while disagreeable associations have inhibitory effects.

Color preference is largely if not entirely a matter of association. Whatever the differences in the experimental results on this subject, there is at least general agreement that brighter and more saturated colors are preferred to dull, dark, shades and colors. But the former, it should be noticed, are uniformly associated with life and sunshine, growth, fertility, and florescence, the latter, with less desirable seasonal changes, with cold and winter, death and decay. The type of preference for brighter colors which can be demonstrated in infants need not have any aesthetic implications. According to Valentine, girls show a much stronger preference for white at from 5 to 9 years of age than later. This may be ascribed to the manner of dress and the frequent admonition to keep neat and clean. That during adolescence and with advancing maturity pinks and flesh colors should be given high aesthetic ratings and should be more frequently preferred in garments may also be an association phenomenon.

The most marked effects of association and habituation may also be observed in connection with changing fashions. What at first appears strange and bizarre is soon accepted as 'chic and smart.' Whatever is the vogue gains a new value, while earlier styles, or those of another generation, become aesthetically unintelligible. It is no doubt because of decreased adaptability that older members of each generation fail to see anything aesthetic in the preferences of the younger set.

To the above could be added many other examples emphasizing the importance of association. Preferences in sculpture and painting have a large associative element. Preferred constructions are all suggestive of activities and circumstances with pleasant connotations. Even the aesthetic value of music, particularly as it appears in the opera, must be attributed to its associative and redintegrative value in connection with dominant drives and emotions.

SUMMARY

The evidence that the principles of spatial design are familiar to those of musical design as well as of other art forms, suggests that the conditions providing for the aesthetic experience are fundamentally the same. These conditions we have discussed under the preparedness-facilitation principle. Typically, aesthetic feelings attend the realization of anticipated or expected relations, and in no case do they appear apart from a certain preparedness in the reaction sequence. Ultimately, the felt unity and harmony of relations in the elements of construction must depend upon the relatedness of the perceptual processes, the overlapping in the response series, and the facility with which successive reactions lead over into each other. Discordant and unrelated elements are so apprehended because they call for unrelated adjustments and redistributions of nerve currents. The significance of preparedness and facilitation is found also in the marked influence of habituation and practice, the importance attaching to pleasant associative values, the enhancing effect of increased familiarity where relatedness is a prominent feature, and, finally, the increased satisfaction and keener enjoyment of the latter part of a performance (as in music, drama, or fiction) when preparedness has reached a much higher level, partly through natural development of inner demands and partly through skillful restraint of the dominant motive.

PLAY INTERESTS AS EVIDENCE OF SEX DIFFERENCES IN AESTHETIC APPRECIATION

By HARVEY C. LEHMAN and PAUL A. WITTY, University of Kansas

Popular thinking assumes conspicuous differences between the sexes in most traits. The problem of sex differences in intelligence has long been discussed. Terman,[1] Hollingworth,[2] and Whipple[3] have presented and examined data secured by means of intelligence tests. These investigators have studied the cognitive processes principally. Conklin[4] suggests that the experimental methods employed by the above investigators are not well designed for the study of those traits in which sex differences are most commonly supposed to be evident, namely, the affective processes.

Thorndike[5] and Starch[6] have computed the percentages of males reaching or exceeding the median woman in a number of specific traits. The sex differences listed by these writers are interesting and suggestive, but the lists are of course incomplete. The data are meager and conflicting in regard to sex differences in certain personality traits.

There is a growing need for careful study of sex differences in various character traits. The following paper deals with one trait, or assembly of traits, namely, aesthetic appreciation. It reports only the extent to which boys and girls participate in play activities involving aesthetic appreciation and attempts to account provisionally for the differences revealed.

Method. Over 5,000 children were asked to check from a comprehensive and catholic list of 200 play activities *only those* in which they had voluntarily engaged during the preceding week. The directions were specific, the teachers were instructed in giving the tests, and the data were obtained from unselected children in order that the results of the investigation might be reliable. Data were secured from children in the following Kansas towns: Kansas City, Lawrence, Bonner Springs, and Moran. The older *S*s were for the most part students of the University of Kansas.

In order that seasonal differences might be taken into account the list was checked by the above groups on each of three different dates, Nov. 7, 1923, Feb. 20, 1924, and Apr. 30, 1924. The giving of the test three times served also to prevent obtaining of unreliable data in reference to nonseasonal activities. Moreover, unusual conditions which might attend one administration of the test were partially minimized by the three separate

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*Accepted for publication July 14, 1927.

[1] L. M. Terman, *The Measurement of Intelligence*, 1916, 68 ff.

[2] L. S. Hollingworth, Variability as related to sex differences in achievement, *Amer. J. Sociol.*, 19, 1914, 510-530.

[3] G. M. Whipple, Sex differences in intelligence test scores in the elementary school, *J. Educ. Res.*, 15, 1927, 111-117.

[4] E. S. Conklin, The scale values method for studies in genetic psychology, *Univ. Oregon Publ.*, 2, 1923, 15.

[5] E. L. Thorndike, *Educational Psychology* (Briefer course), 1915, 346 ff.

[6] Daniel Starch, *Educational Psychology*, 1921, 56 ff.

investigations. The total number of *Ss* from whom data were obtained is indicated in Table I.

Data were assembled according to age and sex. Tables and charts were prepared showing the percentages of individuals in each age group who had participated during the course of one week in each of the 200 activities mentioned in the Play Questionary.

TABLE I

NUMBER OF *Ss*, BY AGE AND SEX, INCLUDED IN THE INVESTIGATIONS

Ages	Date					
	Nov. 7,		Feb. 20,		Apr. 30,	
	Boys	Girls	Boys	Girls	Boys	Girls
8.5	98	100	90	97	80	99
9.5	169	174	161	139	144	144
10.5	182	215	169	199	160	176
11.5	187	235	167	222	184	220
12.5	249	326	201	289	176	266
13.5	280	269	231	235	259	263
14.5	274	301	252	282	238	278
15.5	230	261	247	244	247	256
16.5	210	251	181	223	193	235
17.5	145	182	130	208	146	193
18.5	115	120	170	174	130	167
19.5	95	101	119	115	102	93
20.5	50	73	73	124	59	76
21.5	53	41	57	85	43	66
22 and above	79	44	105	114	68	88

A survey of the sex differences may be obtained by examining lists of the activities in which the largest sex differences were found.

TABLE II

PLAY ACTIVITIES MORE COMMONLY PARTICIPATED IN BY BOYS THAN BY GIRLS AT PRACTICALLY EVERY AGE

Football: using a hammer, saw, nails, etc., for fun; wrestling; marbles; riding a bicycle; climbing porches, trees, fences, posts, etc.; throwing rocks and stones; playing cowboy; boxing; *whistling.

Shooting a gun; playing Indian; playing robber and police; basket ball; baseball with a hard ball; driving an auto; *watching athletic sports; smoking; jumping for distance; jumping for height.

Mumbly peg; flying kites; making or using a wireless or other electrical apparatus; snowball fights; pole vaulting; just playing catch; coasting on a wagon; rolling an auto tire; fishing; playing pool.†

Pitching horse-shoes; playing with bow and arrows; swimming; digging caves or dens; spinning tops; playing with toy trains, ships, autos, wagons, etc.; playing fire engine (or hook and ladder); running races; matching pennies; coasting on a coaster.

Playing bandit; playing soldier; playing with pet dogs; playing with an indoor or playground ball; horse-back riding; rolling a hoop; walking on stilts; hunting; building or watching bonfires; and building a dam.

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†In the case of this activity the sex difference is slighter than would perhaps have been expected because few members of each sex engaged in it.

TABLE III

PLAY ACTIVITIES MORE FREQUENTLY PARTICIPATED IN BY GIRLS THAN BY BOYS AT PRACTICALLY EVERY AGE

*Playing with dolls; visiting or entertaining company; playing house; *just singing; *sewing, knitting, crocheting, etc., for fun; *playing the piano (for fun); writing letters; jumping or skipping rope; dressing up in older folks' clothing; playing school.

*Social dancing; jacks; *playing movie actor or movie actress; London bridge; *stringing beads; *going to parties or picnics; *folk-dancing; just imagining things; *looking at pictures; *gathering flowers.

*Going to entertainments, concerts, and so on; playing with pet kittens; playing Sunday school; drop the handkerchief; *cutting paper things with a scissors; teasing somebody; just hiking or strolling; hop, skip, and jump; playing nurse; *listening to the victrola.

Sleigh-riding; reading short stories; telling or guessing riddles; telling fortunes or having fortunes told; hop-scotch; hide-and-seek; playing store; playing other make-believe games; *statuary.

"Here I come" "Where from?"; tin-tin; *other singing games; old witch; pussy wants a corner; *making mud pies, mud dolls, etc.; *painting with water-colors; *making a scrap book; *taking snap shots, and roller-skating.

Table II presents the play activities which were found to be much more commonly participated in by boys than by girls. The activities of Table II are arranged in order of merit, those showing the greatest amount of sex difference being listed toward the top of the table. Table III presents a similar list showing the play activities in which girls participated much more frequently than boys. At the time of compiling Tables II and III the present writers were engaged in writing a book on the psychology of play activities.[7] The present report was not then contemplated. The lists of activities presented in Tables II and III were therefore compiled without regard for sex differences in aesthetic appreciation.

The writers have recently attempted to identify in Tables II and III those activities most likely to involve aesthetic appreciation. It is of course true that almost any activity may involve more or less of aesthetic appreciation. However, it is probable that some activities are more likely than others to involve such responses. The activities identified by the writers as *most likely* to involve aesthetic appreciation are marked with asterisks.

Tables II and III show that only two activities judged by the writers to be of an aesthetic nature were more commonly participated in by boys than by girls, whereas 20 such activities were more frequently engaged in by the girls.

Since these activities were *voluntarily* participated in, it seems logical to infer that girls obtain greater pleasure than do boys from participation in activities aesthetic in nature. The differences between the sexes in aesthetic appreciation is clearly indicated in Figs. 1–4 inclusive.

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[7] H. C. Lehman and P. A. Witty, *The Psychology of Play Activities*, 1927, 83 ff.

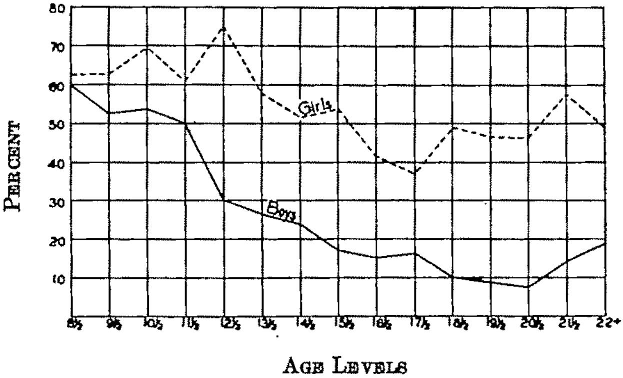

FIG. 1. PERCENTAGES OF GIRLS AND BOYS OF VARIOUS AGE LEVELS WHO REPORTED THAT THEY HAD GATHERED FLOWERS DURING THE COURSE OF THE WEEK PRECEDING APRIL 30, 1924

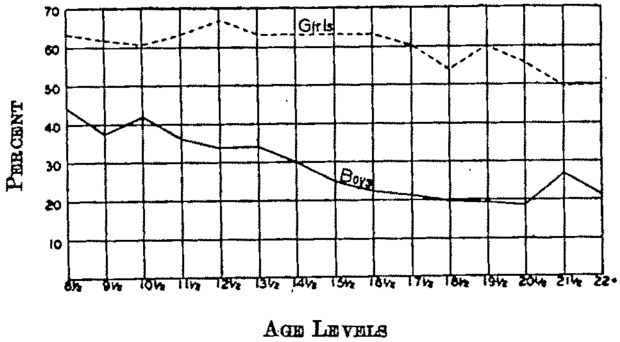

FIG. 2. PERCENTAGES OF GIRLS AND BOYS OF AVERAGE AGE LEVELS WHO REPORTED THAT THEY HAD ENGAGED IN PLAYING THE PIANO (FOR FUN) ONE OR MORE TIMES DURING THE COURSE OF ONE WEEK

Average of results obtained from three separate investigations

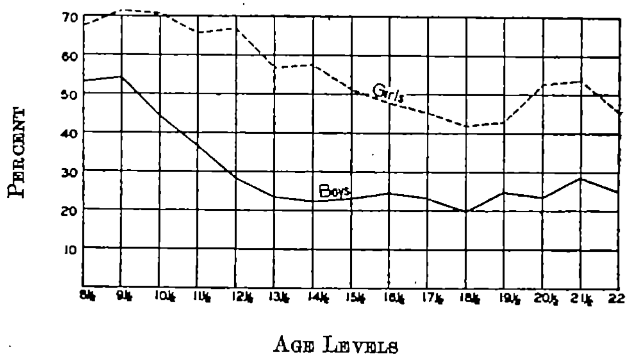

FIG. 3. PERCENTAGES OF GIRLS AND BOYS OF VARIOUS AGE LEVELS WHO REPORTED THAT THEY HAD ENGAGED IN 'JUST SINGING' ONE OR MORE TIMES DURING THE COURSE OF A WEEK

Average of results obtained from three separate investigations

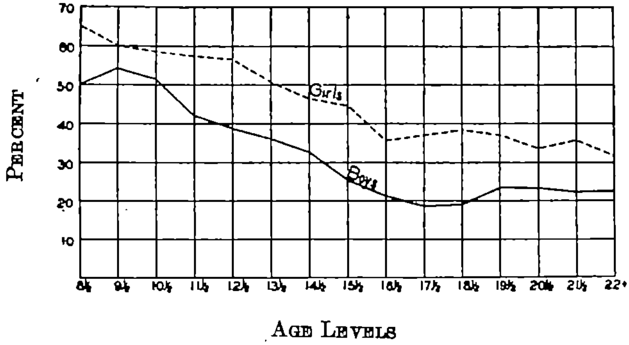

FIG. 4 PERCENTAGES OF GIRLS AND BOYS OF VARIOUS AGE LEVELS WHO INDICATED THAT THEY HAD SPENT TIME 'LOOKING AT PICTURES' DURING THE COURSE OF A WEEK

Average of results obtained from three separate investigations

Corroborative data are not wanting in revealing sex differences in aesthetic appreciation. C. F. Burk nearly 30 years ago found that the objects collected exclusively by girls usually involved aesthetic appreciation.[8] The objects collected exclusively by boys included toys, animals, minerals, and mechanical miscellanies. It is evident therefore that aesthetic appreciation affects the collections of girls, determining the direction of their interest.

TABLE IV

WORK INTERESTS AMONG FRESHMEN IN THE COLLEGE OF ARTS AND SCIENCES, THE UNIVERSITY OF KENTUCKY

	Percent	
	Men	Women
Entertaining people as musician, actor, speaker, etc....	7.8	11.1
Artistic skill as in decorating, window-dressing, millinery, costuming, handicraft, printing arts, etc....	2.8	8.1
Artistic creation, as in writing, designing, composing music, etc....	1.4	5.0

TABLE V

PERCENT OF BOYS AND GIRLS OF VARIOUS AGE LEVELS WHO INDICATED THAT THEY HAD ENGAGED DURING THE COURSE OF A WEEK IN FOUR SPECIFIC PLAY ACTIVITIES

Ages	Play Activity							
	Gathering flowers*		Playing piano for fun		'Just singing'		Looking at pictures	
	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls
8.5	60	63	45	63	53	67	50	65
9.5	53	63	38	62	54	71	54	60
10.5	54	70	42	61	45	70	51	59
11.5	50	61	37	63	37	66	42	58
12.5	30	75	34	67	29	67	39	57
13.5	27	58	34	63	23	57	36	51
14.5	24	52	30	63	22	58	33	47
15.5	17	54	25	63	23	51	26	45
16.5	15	42	22	63	24	49	21	36
17.5	16	37	21	61	23	46	19	37
18.5	10	49	19	54	20	42	20	39
19.5	9	46	19	60	25	43	23	38
20.5	8	46	18	56	23	53	23	34
21.5	14	58	26	49	29	54	22	36
22 and above	19	49	20	49	25	46	22	31

*During the course of the week preceding April 30, 1924. Average of results obtained at the three separate periods.

Miner has studied recently the special interests and abilities of freshman boys and girls in the College of Arts and Sciences at the University of Kentucky.[9] Some of Miner's findings are presented in Table IV. The work interests listed in Table IV are those which obviously involve artistic or aesthetic leanings. It is evident that the freshman girls display greater interest than do the freshman boys in those lines of work which are likely to demand aesthetic appreciation.

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[8] C. F. Burke, The collecting instinct, *Ped. Sem.*, 7, 1900, 187.

[9] J. B. Miner, An analysis of vocational interests, *School Rev.*, 33, 1925, 749.

Both Burk's and Miner's studies support the findings of the present writers. Aesthetic appreciation is displayed by girls more frequently than by boys in (1) collection interests, (2) work interests, and (3) play interests.

One logically asks, to what is this conspicuous sex difference in aesthetic appreciation due? A common answer two decades ago would have been, to differences in the instinctive equipment of the sexes. Today, we are less ready to attribute present behavior patterns to instinctive proclivities. Rather, we are inclined to trace carefully the origin and development of each trait under consideration.

Discussion

Origin of the aesthetic impulse. Various writers have emphasized the rôle of sex in originating different forms of the aesthetic impulse. That there exists a rather intimate relationship between the sex propensity and the appreciation of beauty, most careful students will grant. Indeed, Santayana finds in the sex impulse, the origin and drive to the appreciation of beauty in its multitudinous forms:

> "If any one were desirous to produce a being with a great susceptibility to beauty, he could not invent an instrument better designed for that object than sex. . . . . What more could be needed to suffuse the world with the deepest meaning and beauty? The attention is fixed upon a well-defined object, and all the effects it produces in the mind are easily regarded as powers or qualities of that object. But these effects are here powerful and profound. The soul is stirred to its depths. Its hidden treasures are brought to the surface of consciousness. The imagination and the heart awake for the first time. All these new values crystallize about the objects then offered to the mind. If the fancy is occupied by the image of a single person, whose qualities have had the power of precipitating this revolution, all the values gather about that one image. The object becomes perfect, and we are said to be in love. If the stimulus does not appear as a definite image, the values evoked are dispersed over the world, and we are said to have become lovers of nature, and to have discovered the beauty and meaning of things."
>
> "When love lacks its specific object, when it does not yet understand itself, or has been sacrificed to some other interest, we see the stifled fire bursting out in various directions. One is religious devotion, another is zealous philanthropy, a third is the fondling of pet animals; but not the least fortunate is the love of nature, and of art; for nature also is often a second mistress that consoles us for the loss of a first."[10]

According to Santayana, when love lacks a specific object, when it does not yet understand itself, or when it is sacrificed to other interests, the stifled energy is sublimated in various ways, susceptibility to the beauties of nature being one conspicuous form of sublimation. A similar point of view has been expressed by Langfeld, who holds that aesthetic appreciation may result from the thwarting of *any* impulse or desire.

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[10] George Santayana, *The Sense of Beauty*, 1898, 60 f.

> "In the many and varied forms of art each individual can find his particular refuge. The strong and the weak, the rich and the poor, the prude and the libertine, the prince and the peasant, can one and all select that form of art in which they can best realize a harmonious adjustment of the conflicting impulses which tend to torture and distort the soul. . . . So long as the human race is stirred by conflicts just so long will art itself endure."[11]

Buermeyer asserts that appreciation of art offers a more satisfying refuge from thwarting desires than the day-dream, since objects of art afford *tangible actualities* to satisfy sublimated desires.[12]

The above writers all agree that appreciation of art is to a large extent the outcome of sublimated desire. This hypothesis is in line with the present-day tendency to attribute the origin of much artistic creation to the attempt of the artist to secure compensatory gratification for precluded desires. Thus the actuation of the work of art as well as the appreciation thereof originates in the desire for substitute gratification thereby secured.

A possible explanation of sex differences in aesthetic appreciation. If the impetus to aesthetic appreciation is to be found in the fact that it affords gratification of precluded desires, how is one to account for the greater frequency with which girls and women turn to activities aesthetic in nature? The following provisional explanation is offered by the present writers as a likely one in this regard. Even among very young children, girls are obliged to restrict their range of participation in various activities to a greater extent than are boys.

The present writers found that girls of ages 8 to 22 inclusive engage in *fewer* activities than do boys of corresponding ages. Moreover, girls seem to be restricted geographically in their play behavior, being forced to confine their play to the home or nearby environs. Boys, on the other hand, engage in a larger number of activities and enjoy greater freedom in participation. They were found to participate much more frequently than girls in activities which require departure from home and a considerable geographical radius:[13] horseback riding, hunting, gathering nuts, fishing, and flying kites.

Girls, however, were found to indulge with much greater frequency than boys in activities within or near the home. The activities listed toward the top of Table III are illustrative.

The restrictions operate progressively as the children grow older. Girls are supervised and protected to a greater extent than are boys. These forces operate to effect a greater need for sublimation or redirection of desire in girls. Hence, the greater general aesthetic appreciation on the part of girls ensues.

A possible explanation of sex differences in aesthetic creation. If the preceding hypothesis be granted, namely, that the great frequency with which women participate in aesthetic activities is the outcome of sublimated desire, how is one to account for the fact that men more frequently produce outstanding works of art than women? The following hypothesis is offered as a likely one in accounting for this fact.

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[11] H. S. Langfeld, *The Aesthetic Attitude*, 1920, 28 f.

[12] L. Buermeyer, *The Aesthetic Experience*, 1924, 110.

[13] Lehman and Witty, *op. cit.*, 93.

Although women are forced to sublimate their desires more often than men and consequently display aesthetic appreciation more frequently, it may be that they react much less violently than men to thwarting. There is some evidence that females are able to stand thwarting to a greater degree than men. The fact that males are less able than females to endure thwarting has been observed by Cannon. He found that young male cats are more restive and excited when fastened to a holder than females.

> "In my earliest observations on the movements of the stomach I had difficulty because in some animals the waves of contraction were perfectly evident, while in others there was no sign of activity. Several weeks passed before I noticed that this difference was associated with difference in sex. In order to be observed with Röntgen rays the animals were restrained in a holder. Although the holder was comfortable, the male cats, particularly the young males, were restive and excited on being fastened to it, and under these circumstances gastric peristaltic waves were absent; the female cats, especially if elderly, usually submitted with calmness to the restraint, and in them the waves had their normal occurrence."[14]

It seems likely that the observation made by Cannon regarding animals holds also for human beings. The present writers have shown that girls are able to endure situations in their play behavior which would prove unendurable to boys because of the monotony involved.[15] Thorndike, employing data assembled by K. Pearson, concludes that only 38% of men reach or exceed the median woman in patience.[16]

The above findings are very suggestive. They indicate that the male's impulses may be much stronger than those of the female, and that this strength may be characteristic of the human species as well as of animals. In ordinary life men are apparently not required to suppress their desires as often as are women. This may explain the male's smaller amount of aesthetic appreciation. But in some instances thwarting of males inevitably occurs. When it does occur, perhaps the male becomes exceedingly restless and struggles to effect adjustment. When the circumstances are favorable, the result may be genuine artistic creation. In the thwarted female such artistic achievement is less likely to result, because of the absence of a comparable amount of drive. This may be one factor explaining the rarity of aesthetic achievement of a high order among women.

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[14] W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear, and Rage*, 1920, 14.
[15] Lehman and Witty, *op. cit.*, Chap. 7.
[16] Thorndike, *op. cit.*, 349.

THE INFLUENCE OF SEX AND AGE UPON THE ABILITY TO REPORT

By JOHN A. MCGEOCH, Washington University

The problem of the influence of age and of sex upon the ability to give a faithful report of observed data was for some time, in the early part of the century, a particularly interesting one to German investigators. The interest has somewhat waned, but the problem, in many of its aspects, remains.[1]

Many of the early studies were conducted in a fashion which rendered their results inconclusive in any general sense. They employed often either too few *Ss*, a restricted age-range, or only a single type of report material. Obviously, data from a few *Ss*, in this field particularly, permit no generalizations. Data from sufficient *Ss* over a limited age-range permit generalizations, but in a circumscribed manner. Quite possibly conclusions drawn from reports on one type of material may hold only for that type, and, unless the nature of ability in report is known, generalization is limited to statement in terms of the material used. Possibly, also, the intellectual status of the *Ss* might be the determining factor in their reports and conclusions might, therefore, be valid only for *Ss* of the specific mental status used in any given experiment. Such considerations are clearly germane to studies of both age and sex.[2]

The problems of *Aussagepsychologie* are significant both with regard to the body of knowledge about immediate memory functions and for the construction of an applied psychology of testimony. An attempt has been made in this investigation to avoid some of the conditions which have often rendered studies in this field inconclusive, and to study age and sex differences in report with a relatively large number of *Ss* of known intelligence rating, using three different types of report materials.

Report Materials and Method. The report materials used are fair representatives of those which have been described in the literature of *Aussagepsychologie.* They were the Binet object-card, a picture called *The Disputed Case,* and an event. These materials, the instructions given to the *Ss*, the methods used in gathering the data and the scoring of the results have been previously described.[3] The only deviations from the

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[1] From the Psychological Laboratory of the University of Chicago.

[2] The literature on these problems is too well known and too accessible to merit summary. Whipple's reviews in the *Psychological Bulletin* from 1909 to 1915, and J. Schrenk's *Einführung in die Psychologie der Aussage*, 1921, have summarised the better part of the literature.

[3] McGeoch, The fidelity of report of normal and subnormal children, this JOURNAL, 36, 1925, 434-445.

procedure there used were that there were 50 questions in all interrogatories, and that attestation on both narrative and interrogatory was asked of all *Ss*. The conditions were kept as standard as possible for all *Ss*.

Subjects. The *Ss* used in this study are children from four East St. Louis, Ill., schools. They range from 9–14 years old and from the 4–8 grade. The four schools are elementary schools of the same general class and the *Ss* appear to represent a fair sampling of the elementary school population of the city. The number of children at each age for each of the report materials is shown in Table I.

The number of cases is many times larger than that used in most of the preceding investigations of children's testimony. It is probable that many of the conflicting results in this field have been an outcome of the use of a few *Ss* only.

TABLE I

NUMBER OF CASES AT EACH AGE LEVEL

Age	Object-Card			Picture			Event		
	Boys	Girls	Total	Boys	Girls	Total	Boys	Girls	Total
9	43	49	92	54	56	110	34	39	73
10	87	75	162	80	60	140	58	60	118
11	83	63	146	68	56	124	74	69	143
12	64	52	116	65	46	111	58	58	116
13	38	59	97	27	53	80	31	42	73
14	25	25	50	29	21	50	33	24	57
Totals	340	323	663	323	292	615	288	292	580

Measures of Intelligence. To obtain a measure of intelligence, two tests were given: the National Intelligence Tests, Scale A, Form 1, and Army Alpha, Form 5. The scores of every *S* on the two tests were combined by adding the sigma standings algebraically and dividing by two, so that the combined score was expressed in terms of the mean sigma standing in the two tests. These combined scores were computed separately for each age from the mean and sigma of each age. In Fig. 1 are shown the lines of the means in intelligence score for each age (regression of intelligence on age) for the individual tests. Intelligence at any age has been expressed in terms of the sigma standing of the mean of that age computed from the mean intelligence score for all ages and the corresponding sigma. The results of this method were checked by drawing the lines for each test from the means of the raw scores. It was found that the two methods gave corresponding results. It will be noted that the lines for the two tests run very nearly together and that the result of their combination would be almost a straight line, except for a slight irregularity at age 12.

Results: (1) *Age Differences.* In presenting the data, the most important scores are items right and wrong, and the percentage of items attested. These will be given in detail and the tendencies of the secondary scores, total percent right, and percent attested and right will be stated briefly without presentation of the data. The tabular statement of scores for each level results in cumbersome tables, difficult of interpretation. The data are, therefore, given in graphic form. The means of items right and wrong on the narrative are shown in Fig. 2.

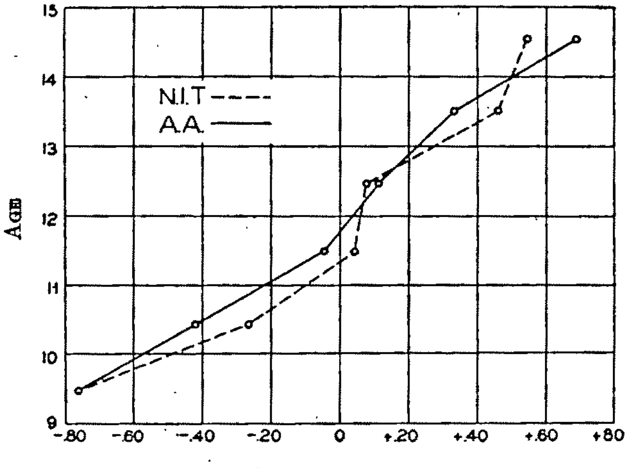

FIG. 1. INTELLIGENCE SCORES BY AGES

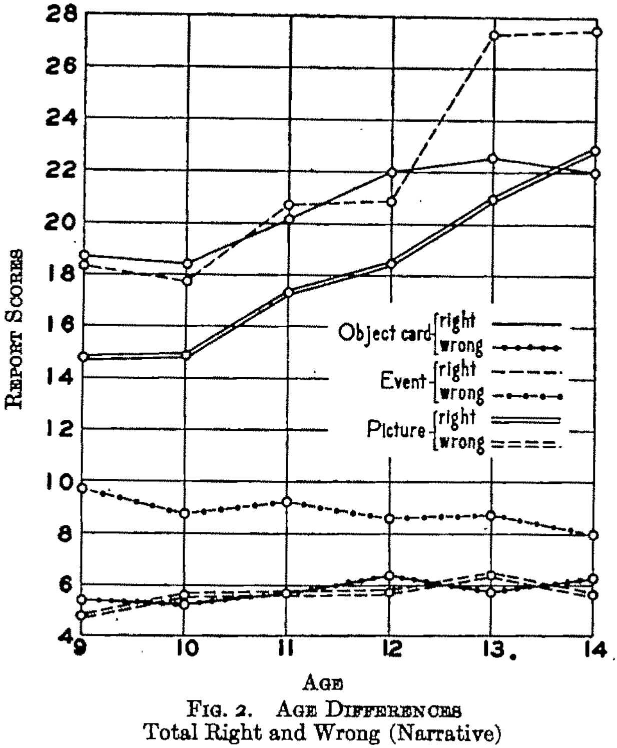

FIG. 2. AGE DIFFERENCES
Total Right and Wrong (Narrative)

The curves for items right remain roughly constant from 9–10, then rise throughout the remainder of the age-range, except that the curve for the object-card remains almost constant from 12–14. The object-card and picture error curves show no pronounced tendency to increase or decrease with age and only a slight tendency to minor fluctuations from one age to another. The event error curve falls very slowly from 9–14, but the amounts are hardly large enough to be significant. When the total percent right is computed from these scores, the curves show the same general tendency as the curves for mean items right. For the picture and the event, however, they have a much steeper slope than do the corresponding mean items right curves. The curves for percents attested and right follow courses almost identical with those for total percents right, but at a higher level.

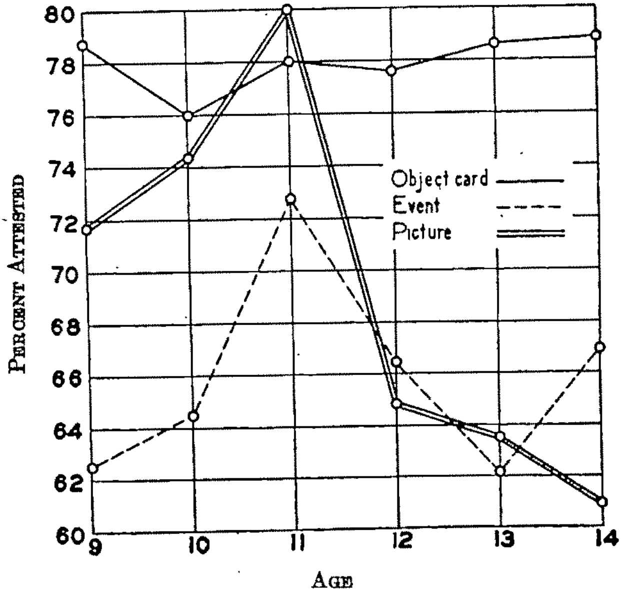

FIG. 3. AGE DIFFERENCES
Percent Attested (Narrative)

The data for percents attested on the narratives are shown in Fig. 3. This score represents the degree of certainty of the *S* with respect to the items recalled. In another section of this investigation it has been found that this score is unrelated to intelligence and probably represents a 'personality' factor. A glance at the curves shows that their irregularity is too great to permit any conclusion other than that there is no uniform age change in percent of attestation, though the amount of the attestation varies somewhat with the material upon which report is made.

The means of items right and wrong for the interrogatory form of report are shown in Fig. 4.

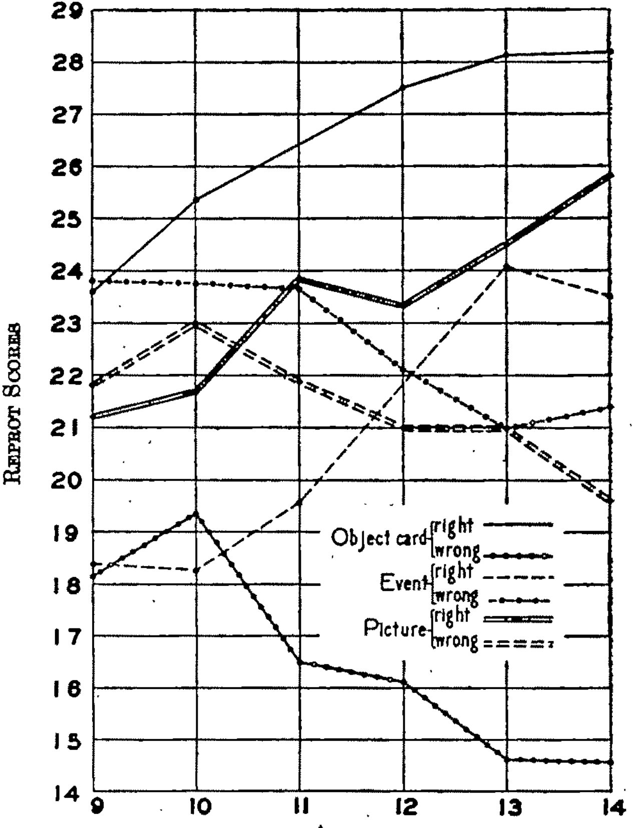

Fig. 4. Age Differences
Total Right and Wrong (Interrogatory)

The general tendency is for all functions to show an increase with increasing age. With the picture and event there is very little change from 9–10, a result the same as that found for the narrative of each. The error curves tend to fall with increasing age, particularly from 10–14. When these scores are used in the computation of total percents right and the results plotted, curves which rise rapidly from 10–14 are the result. The same is true for percents attested and right, save that these curves run at a higher level than do those for total percents right.

The curves for percents attested show the same irregularities found for this score on the narrative, and are not reproduced here.

TABLE II

DIFFERENCES BETWEEN SUCCESSIVE AGES AND THEIR SIGMAS

Report	Ages compared	Object-card		Picture		Event	
		D	Sigma	D	Sigma	D	Sigma
Narrative	9–10	− .1	.97	.1	.69	− .5	1.06
	10–11	1.7	.79	2.4	.72	2.5	.96
	11–12	1.9	.87	1.1	.80	.4	1.04
	12–13	.3	1.07	2.6	1.28	6.8	2.08
	13–14	− .3	1.27	2.1	1.85	.3	2.98
Interrogatory	9–10	1.6	.88	.3	.67	− .1	.84
	10–11	1.0	.74	2.1	.58	1.3	.68
	11–12	1.1	.91	− .5	.68	2.3	.77
	12–13	.6	1.05	1.1	.79	2.3	.99
	13–14	.2	1.38	1.4	.95	− .5	1.37

Thus far nothing has been said regarding the statistical reliability of these results. While it is highly probable that age-increases, when they occur consistently in three types of report material and with two different methods of reporting, are valid, the certainty of the conclusions will be enhanced by a statement of the reliability of the differences. In Table II are given the differences between the means at successive ages and the sigmas of these differences for items right, both narrative and interrogatory.

The outstanding fact revealed by Table II is that, in the majority of cases, the sigmas of the differences are almost as large as, or larger than, the differences themselves. This signifies that the difference between any two successive ages is of very doubtful validity. Certainly no more than the probable direction of the difference can be indicated. The means, the differences between which are in question, are reliable, being many times their sigmas. The differences between them, however, are too small to be reliable.

TABLE III

CUMULATIVE AGE DIFFERENCES

Report material	Ages compared	Narrative		Interrogatory	
		D	Sigma	D	Sigma
Object-card	9–14	3.5	1.23	4.5	1.36
Picture	9–14	8.3	1.60	4.4	.92
Event	9–14	9.5	2.41	5.3	1.23

Whether, over larger differences in age than that between successive ages, any significant differences in report-ability might occur, can be determined by computing the sigmas of the differences between the means at the extremes of the age range studied. If this difference turns out to be reliable, we may conclude that there is an actual increase in ability to report with increasing age, even though the increase from one age to another is not large. These differences with their sigmas appear in Table III.

If we take a sigma which is one third of its difference to indicate reliability, the differences between ages 9 and 14 are all reliable, with the slight exception of the object-card narrative. The increase is least on the object-card narrative and greatest on the event narrative. It is greatest on the event interrogatory and roughly equal for the picture and object-card.

Similar treatment of the error differences shows that the differences are, for the narratives, totally insignificant, while those on the interrogatory, though unreliable between successive ages, show a slight cumulative decrease. It appears, therefore, that there is no age-change in narrative errors and a slight decrease with age in interrogatory errors. When the reliability of cumulative age differences in total percents right is computed, virtually the same relations are found as those which are given in Table III.

The facts thus far presented warrant the conclusion that, while differences in report-ability between successive ages are small and unreliable, cumulative differences appear to such an extent that the differences between the initial and final ages of the range studied are reliable. This indicates a definite but slow increase in ability to report with increasing age. The amounts of the increase vary with the material upon which report is made. Inspection of the curves will show the greatest tendency to variation with age occurs with the event, while the picture and object-card follow in the order named.

TABLE IV

NARRATIVE: TOTALS RIGHT AND WRONG

Age	Object-Card				Picture				Event			
	B–R	G–R	B–W	G–W	B–R	G–R	B–W	G–W	B–R	G–R	B–W	G–W
9	18.5	18.4	4.9	5.8	14.3	15.3	4.4	5.0	15.9	20.4	8.4	11.1
10	18.0	18.9	5.1	5.5	14.6	15.4	5.6	5.5	17.5	18.1	8.2	9.5
11	20.8	19.2	6.1	4.9	16.8	17.9	5.2	6.0	20.3	20.3	9.0	9.2
12	20.9	23.3	5.4	6.7	18.0	19.0	6.0	5.4	18.4	23.0	7.2	9.6
13	22.5	22.2	6.2	5.7	19.5	23.2	5.7	6.3	20.5	32.7	10.1	7.7
14	21.5	22.4	6.0	6.1	19.3	28.4	5.8	6.1	23.5	33.6	7.2	9.0

(2) *Sex Differences*. The comparative performances of the sexes can be shown most compactly in tabular form. The narrative total right and total wrong scores may be found in Table IV. The symbols B, G, R, and W refer, respectively, to boy, girl, items right and items wrong.

Sex differences in object-card reports are small and irregular in direction. One can only conclude that neither in correctness nor in incorrectness do these reports show sex differentiation. With the picture the girls report consistently more right items than the boys, but for years 9–12 the differences are unreliable. The difference of 9.1 at year 14 with a sigma of 2.9 is the only difference that is statistically reliable. No consistent sex differences appear in total wrong items. There is a consistent tendency for the girls to report more right items on the event. The differences here are relatively large and several are reliable. There is also some tendency for the girls to report more wrong items. When the data of Table IV are used in the computation of total percents right for each type of material, similar tendencies appear, although less clearly.

Total items right and wrong in the interrogatory reports are given in Table V.

Reports on the object-card show no sex differentiation in either score. On the picture and event the girls consistently report more right items than do the boys. The differences, however, are small and most of them are statistically unreliable. Their consistency would, nevertheless, argue for their significance. Reports on the picture show no consistent differentiation in items wrong, while on the event the girls give fewer wrong items. The same general tendencies appear when total per cents right are computed.

TABLE V

INTERROGATORY: TOTALS RIGHT AND WRONG

Age	Object-Card				Picture				Event			
	B–R	G–R	B–W	G–W	B–R	G–R	B–W	G–W	B–R	G–R	B–W	G–W
9	24.0	23.5	18.0	18.2	19.7	23.0	21.1	22.4	16.5	19.9	25.3	22.5
10	25.3	22.7	19.5	19.1	21.1	22.5	23.4	22.4	17.8	18.6	23.9	24.5
11	25.5	27.5	17.0	16.8	23.0	24.7	22.3	21.4	19.2	19.5	24.2	23.0
12	26.7	28.4	15.5	16.6	22.7	24.3	21.0	20.9	20.5	23.2	22.7	21.5
13	26.8	28.0	14.2	14.7	24.1	24.5	21.9	20.6	21.5	26.1	22.4	18.7
14	28.4	26.1	14.7	14.4	22.6	28.1	19.9	19.3	21.7	26.4	22.2	20.3

The percent of items attested by each sex has been found, but no sex differentiation whatever appears. There are many fluctuations but no regular tendency. These data are not given.

CONCLUSIONS

(1) Ability to report in narrative form, measured in terms of total items right and total percents right, increases between the ages of 9 and 14 with each of the three report materials used. There is with narrative errors no definite tendency to vary with age, and only a slight tendency toward fluctuations.

(2) Ability to report in interrogatory form increases between the ages of 9 and 14 with each of the three report materials studied. This fact is indicated by the increase in numbers of items right reported and by the decrease in numbers of items wrong.

(3) Differences between successive age levels are, however, small and statistically unreliable, but they accumulate in such a fashion as to render the differences between the extremes of the age-range much larger and of a satisfactory degree of reliability. All of the increases with age are small, seldom averaging more than one or two points a year over the years which show an increase. Further, the increases are not strictly continuous.

(4) Reports on different materials show differential age increases. The event reports show the greatest tendency to yield an increase with age; the picture and the object-card follow in the order named.

(5) Sex differences in report ability are, in part, a function of the material upon which report is made. Object-card reports show no consistent differentiation, while reports on the picture, and even more on the event, show a fairly consistent, though slight, tendency for the girls to excel the boys. These differences are small, and few of them are reliable. Assurance, measured by percent of items attested, does not vary regularly either with age or with sex.

QUALITATIVE DIFFERENCES BETWEEN BINOCULAR AND UNIOCULAR IMPRESSIONS

By M. J. Zigler and Kathryn Ward, Wellesley College

Katz has recently reported that a dimly illuminated object,[1] *e.g.* the whitewashed wall at the rear of a deep room which is illuminated by windows on the opposite side only, presents three changes of appearance if there be a sudden shift from binocular to uniocular or from uniocular to binocular observation. (*a*) The shift from binocular to uniocular observation results in a darkening, and the reverse order of shift in a brightening of the wall. Katz states that the brightening effect attending the latter order of shift is more obtrusive than the darkening effect arising from the closing of one eye. (*b*) Small spots of slight unevenness or of slightly different coloring are less clearly perceptible in uniocular vision. (*c*) Certain entoptic phenomena, usually imperceptible in binocular vision, are clearly observable in uniocular observation.

Previous writers have reported the change in brightness as the only observable alteration attending the immediate shift from uniocular to partial or full binocular observation (or vice versa). Fechner was probably the first to note this change.[2] Fechner used two methods of observation. In the first place, he suddenly placed before one of the eyes during fixation of the sky a gray or smoked glass which absorbed less than one-half of the light rays entering it;[3] or he suddenly removed the glass after looking at the sky through it with one eye. In the second place, he observed the sky binocularly, naturally with one eye and through the gray glass with the other, and then suddenly closed the latter eye; or he suddenly opened the eye so as to look through the gray glass after observing the sky uniocularly. He found that a brightening of the visual field resulted in those instances in which the second phase of an observation, in comparison with the first, was made under increased degree of retinal stimulation, and a darkening effect appeared whenever the second phase had reduced total illumination. Aubert,[4] McDougall,[5] and Piper[6] found that the shift from binocular to uniocular observation (or vice versa) results in a perceptible change in brightness under conditions of low illumination only. The last named explained this alteration in brightness as a function of dark adaptation. Dawson,[7] employing a refined technique, found that smaller changes in brightness are detected binocularly than uniocularly, and that judgments of brightness are given more promptly, more consistently and more certainly under binocular observation. He also found that the uniocular impressions are less determinately localized than binocular.

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[1] D. Katz, *Die Erscheinungsweisen der Farben*, 1911, 229 ff.

[2] G. T. Fechner, *Abhandlungen der mathematisch-physischen Classe der königlich sächsischen Gesellschaft der Wissenschaft*, 7, 1861, 416 ff.

[3] With glasses which absorbed more than one-half of the light rays, Fechner observed a *paradoxical effect*, *i.e.* placing the filter over the eye produced a brightening and, in the case of opening the eye to look through the glass, a darkening of the visual field.

[4] H. Aubert, *Physiologie der Netzhaut*, 1865, 282.

[5] W. McDougall, On the relations between corresponding points of the two retinae, *Brain*, 33, 1911, 372.

[6] H. Piper, Über das Helligkeitsverhältnis monokular und binokular ausgelöster Lichtempfindungen, *Zsch. f. Psychol.*, 32, 1903, 176.

[7] S. Dawson, Binocular and uniocular discrimination of brightness, *Brit. J. Psychol.*, 6, 1913, 78.

EXPERIMENT I

Our experiments were undertaken in an endeavor to confirm the facts which Katz has set forth as distinguishing uniocular and binocular impressions. In the hope of securing, if possible, other observable differences, we also attempted to extend his work by making observations under somewhat brighter conditions of illumination than Katz used, as well as under the alleged optimal conditions in a dark room.

The preliminary work was performed in a large room which is lighted by a large central skylight and by windows on one side only. The *O* was seated with his back to the windows, facing and 4 m. away from the exposure apparatus, which was placed just beyond the center of the room under the skylight. A set of 15 cardboard stimulus-cards, 28 x 30.5 cm., was used. Ten were achromatic, varying in large fairly regular steps between white and velvet black; the other 5 were hues of green, fawn, yellow, brown and blue, every one of very low chroma. Most of these cards were plain; several had cross-lines and unevenness of surface in varying degrees of definiteness. The cards were presented in haphazard order in an exposure frame which held them in a position inclining backwards at an angle of 45° from the vertical. A cardboard exposure screen with an opening 12 x 18 cm., located 20 cm. in front of the lower edge of the card-holder, was operated manually by *E*. The four *Os*, who gave an hour per week (during the second semester of the 1924-25 academic session) to observation in this experiment, were Misses V. Berkey, V. Reinhart and K. Knight, undergraduates in an advanced laboratory course, and the senior author of this study. Every *O* had normal vision.

The *Os* were instructed as follows: "A card will be exposed for a period of 3 min. during which you are to view it alternately with one eye and with two eyes for the purpose of noting whether differences of appearance are correlated with the different modes of observation." The eyes were rested at least 3 min. between exposures until all after-images disappeared. The exposure period was soon reduced to one minute for three *Os* and to 30 sec. for the fourth to avoid serious complication of adaptation phenomena during observation. The *Os* were allowed absolute liberty as to the number and location of shifts. In the later stages of the work the *Os* were asked to close the eye other than that one which they naturally fell into the habit of closing in uniocular vision, so as to determine whether or not differences in the two eyes were at all responsible for any changes in appearance.

Results. After varying periods of training in these observations, all *Os* reported definite and immediate changes in the appearance of the cards at the moment of shift in mode of observation. The shift from binocular to uniocular vision resulted regularly in a darkening of the card for three of the *Os*. A clear reduction in chroma was reported regularly by two and occasionally by the other *Os*. The uniocular impression was characterized also by a hazy dullness. This was suggested somewhat less frequently than the other points of difference just given, nevertheless it was indicated by all *Os*. Every *O* remarked also the less determinate localization of these impressions. There was a strong tendency to see the opening of the exposure screen filled with a haze of color, and the fact that the card was presented some distance behind the screen and at a slant was imperceptible. These facts were never clearly perceived in uniocular vision. We failed to secure a single report in confirmation of Katz's statement that entoptic phenomena are more clearly perceived uniocularly.

The shift from uniocular to binocular observation resulted regularly in increase both in brightness and in saturation, in determinate localization of the stimulus-card, and in the realization that it was presented at a slant. The binocular impressions were also distinguished from the uniocular by a glossy hard surface and by the fact, which Katz has mentioned, that minor details of surface are more clearly perceptible.

Every one of the *O*s stated that the changes appearing with the shift to uniocular vision were just as obvious to the one eye as to the other.

EXPERIMENT II

The second and main experiment was performed in a dark room. The conditions of illumination were kept constant and at low intensity, where the binocular-uniocular differences are alleged to be most clearly observable.

An oculist's amblyoscope was adapted to the requirements of our experiment. The instrument was mounted upon a metal base, which was fastened near the edge of the table before which the *O* was seated in an adjustable chair. A brass hood, similar to that of an ordinary stereoscope, was fitted to the nearer end of the tubes. The darkroom measured 4.8 x 2.4 m., and the *O* was seated 1.5 m. from that end of the room at which one enters. A black curtain screen through which the hood of the apparatus protruded was stretched entirely across the room to prevent the *O* from seeing anything behind it except as the stimuli were presented in the instrument.

Pairs of essentially transparent stimuli (44 mm. sq.), both members of which were alike, were cut from viscoloid sheeting.[8] The series of 15 pairs consisted of 8 *uniformly colored* and 7 *figured and variegated* squares. The hues of the former were red, green, bottle green, blue, violet, cream, amber, and rose; of the latter, gray pearl, green pearl, rose pearl, jade, shell, Chinese mottle, and rainbow. A lamp, set 40 cm. behind the stimulus-holders of the instrument and at the mid-point between them, illuminated the two members of a pair equally at all presentations. During most of the work a 100-watt daylight mazda lamp was used; 50- and 75-watt lamps were also used for short periods; they failed, however, to produce any significant alteration of results.

The apparatus was stationed at an equal distance from the side walls of the darkroom. The *O* looked into the instrument and through the transparent stimulus-squares against backgrounds (in different experiments) of black, white, and neutral gray cardboard, which were fastened upon the side walls of the room. After discovering that the quality of the background had no crucial influence upon the qualitative character of the binocular-uniocular differences, we eventually settled upon the steady use of the pair of neutral gray backgrounds.

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[8] The sheets were selected from the stock which the E. I. Du Pont de Nemours Company of Boston manufacture.

Instead of the questionable procedure of allowing the *O*, as in Experiment I, to determine voluntarily the time of shift from one mode of observation to the other, we attached just behind one of the stimulus-holders a shutter of hard rubber, which *E* operated manually to close off the view of one of the *O*'s eyes. A stop aided *E* in making a sudden and complete shift in the position of the shutter.

Retinal rivalry appeared in the binocular observation of the *figured and variegated* stimuli; there was, however, no tendency on the part of the *Os* to confuse the phenomenon of rivalry with any of the distinguishing criteria of binocular and uniocular vision. With the *uniformly colored* stimuli, rivalry was not reported.

The shift in mode of observation was placed at the end of the 7th second, as measured by a silent pendulum, of the 15-sec. total exposure period. In one-half of the presentations observation began with the one mode, and in the other half with the other mode of observation. The *Os* were instructed to "observe carefully and report in detail any changes which take place in the field of vision during the period of exposure. Compare especially the presentation of the second half with that of the first half of the exposure."

The *Os* were Miss M. Davidson (D) and Mrs. A. B. Hoffman (H), graduate students in psychology, Miss A. Mathiesen (M), graduate assistant in psychology, and the senior author of this study (Z). All had normal vision and were well trained in psychological observation. M and Z had recently observed in a related visual experiment. Miss K. Ward, the junior author and an undergraduate majoring in psychology, served as *E* throughout. The *Os* gave an hour per week during the regular academic session of 1925-26 to this experiment.

Results. Adaptation changes played a much smaller rôle in these observations of 15-sec. exposure than in the previous ones of a full minute. The *Os* found little difficulty in distinguishing between these changes, which develop as a function of time under steady fixation, and those which took place suddenly with the shift in mode of observation.

The *Os* reported perceptible changes in brightness, saturation, localization, distance, perspective and objectivity, surface appearance and transparency. Some of these changes were reported more regularly than others, but changes in regard to all of these points were indicated with sufficient frequency and positiveness by every one of the *Os* to justify their inclusion as points of real distinction under our conditions of observation. Some of the stimuli favored the appearance of certain of these differences; some proved utterly unsatisfactory for the exhibition of certain points of difference. We shall take up these points of distinction separately.

(1) *Brightness*. The change in brightness is perhaps the most outstanding point of distinction between binocular and uniocular observation. This is the change which previous writers have regularly noted. Our *Os* confirm their finding that the shift to uniocular vision produces a darkening, while that to binocular vision results in a brightening of the visual field. This difference was noted in more than 90% of the presentations

to all *Os*. All of the colors used exhibited this change very consistently. Our *Os* tend to substantiate the observation by Katz that this change, notwithstanding the fact that it is sudden in initial appearance, is not a sheer momentary phenomenon.[9] They report that it persists throughout the second half of the observation. They also agree with Katz that the change which results in a brightening is more obvious and insistent than that which produces a darkening of the field. We offer as a plausible explanation of this fact the principle that a change in brightness is a more compulsory condition of attention than a change in darkness of the same magnitude.

(2) *Saturation.* The *Os* reported regularly that the saturation of these colors was perceptibly reduced in uniocular vision. The change in saturation was not reported quite as frequently as the change in brightness, yet it was for every one of the *Os* one of the most regularly reported alterations attending the shift in mode of observation. The increase in saturation in the shift from uniocular to binocular observation appeared just as positively as the reduction in the other order of shift. Two of the *Os*, M and Z, reported with marked assurance that this change is more significant with the more saturated colors than with those of lower chroma.

(3) *Perspective and Objectivity.* With the uniformly colored stimuli the *Os* manifested a marked tendency to perceive the binocular impressions as possessing depth or as standing out in definite perspective. These impressions frequently had definite objective reference. They were labelled as "a large clear almost transparent gum-drop," "a mold of cone-shaped clear jelly," "a ball of fairly clear water-glass," "a cone of milky white jelly," etc. The uniocular impressions, on the contrary, seemed to lack perspective and gave little suggestion of meaningful object. These impressions were characterized as flat, soft-like, dull colors which were often somewhat obscured or overshadowed by atmospheric or cloud-like haze effects. Aside from this overlay of haze or shadow, there is only slight suggestion of the realization of depth in uniocular impressions. In fact, all *Os* manifested surprise at their inability to realize perspective during the one half, when it was readily perceived in the other half of the observation. In the relatively few instances when perspective was reported in uniocular impressions, the degree of perspective was slight and the perceived object was imperfect and incomplete. There was little effort, and notable inability where undertaken, to assign objective labels to the uniocular impressions. The perception of an object in perspective was not always realized even in binocular

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[9] *Op. cit.*, 231.

observation. This seemed to depend upon an *object attitude.* Even under the object attitude, however, the uniocular impressions failed to mature fully as objects in perspective.

With the figured and variegated stimuli there was slight tendency toward the perception of a tridimensional object in the binocular presentations. There was rather a tendency for the *O*s to see certain spots and patterns of the stimulus field, in general those of brighter tint and of richer hue as nearer, and the others at variable distances. The variation in distance of the different parts of a variegated stimulus was also perceptible uniocularly. The amount of the distances was, however, much reduced in uniocular observation. Here the nearer parts appeared to be superposed upon the farther rather than to give the impression of any marked degree of inherent depth.

(4) *Distance.* In addition to the fact that in the uniocular observation of the figured and variegated stimuli the nearer parts and the farther parts are telescoped into close compass, so that the impression of depth is slight, there was a pronounced tendency in three *O*s to designate the uniocular perceptions as somewhat more distant. The amount of this change in apparent distance from the *O* in the shift from binocular to uniocular observation could not be determined, owing to the fact that the uniocular impressions could not be determinately localized, as we shall indicate below. This change in distance, although slight in any case, is more obvious in the shift from uniocular to binocular observation. In this order of shift the impression advances from an indefinite to a definite position, while in the shift from binocular to uniocular observation the impression recedes from a definite position to one of great indefiniteness; accordingly, the latter observation is made under difficult conditions.

(5) *Localization.* Every *O* declared that the uniocular impressions are less determinately localizable than those binocularly observed. The latter are always observed at a definite distance, while the former are seldom definitely localized. If the *O* attempted to estimate the distance and localization of uniocular impressions, the judgments varied widely from observation to observation; the attempt was often given up as impossible. It was very exceptional that an *O* designated the uniocular impression as easily localizable as the corresponding binocular one. The uniformly colored were less easily localized under uniocular observation than the figured and variegated stimuli.

(6) *Transparency.* The uniocular impressions seemed to resist visual penetration to a greater degree than the binocular impressions. They seemed to "cloud up somewhat" or to be-

come "slightly foggy." They generally gave the *Os*, appreciably less than the binocular, the impression that they were seeing the light through the colored stimulus. Even despite the fact that the binocular impressions acquired marked perspective, they presented a much more positive appearance of transparency. The less saturated of the uniformly colored stimuli seemed more clearly transparent, while the richer colors appeared more like thick jelly which gave less positive evidence of transparency. The figured and variegated stimuli seldom exhibited the difference of transparency in the two modes of observation. They tended rather to give the impression of a display of different colors at different distances in both cases.

Transparency was influenced on the one hand by the tendency to configuration and on the other by alteration in brightness. The binocular impressions were frequently reported as more configured than the uniocular. Where this tendency was pronounced, the uniocular impressions were designated the more transparent. When the binocular impressions were relatively free of configurative detail, they were regularly reported as possessing the higher degree of transparency.

Again, where the degree of brightness between the two impressions was pronounced, the brighter (binocular) was as a rule more transparent, unless there was at the same time a marked degree of configuration. Increase in brightness favors the appearance of configurative detail, and thus tends to reduce transparency; but if configurative details be lacking or relatively subdued, the brighter impression is usually the more transparent. Brightness is a factor which may throw the matter of transparency in either direction.

We have pointed out that the surfaces of uniformly colored stimuli under uniocular observation are overcast by a cloud-like or atmospheric haze or shadow, and that binocular impressions present a more definite limiting surface of separation. Our *Os* also indicate that in binocular observation of the figured and variegated stimuli the surface features, texture and details of configuration are somewhat more definite and hard. In uniocular observation the more minute characteristics are often reduced and sometimes even indiscernible, and the surface of the visual impression is characterized by a 'softness.' The tendency of the surface to soften, and especially to develop the shadow or thin cloud-like effect, is most pronounced in the case of the uniformly colored stimuli. This point of distinction does not hold for the figured and variegated stimuli. Although we did not make direct comparison of these impressions and the typical film color of Katz, it appears probable that the uniocular impression may under certain conditions represent, if not a typical film, some form of phenomenological intermediate between film and surface colors. Our two *O*s, M and Z, who had previously observed in a related visual experiment, as yet unreported, in which direct comparison of impressions under Katz's conditions were made, were convinced of the film-like characteristics of the impressions mediated by the uniformly colored stimuli in uniocular vision. They were both positive, however, that the figured and variegated stimuli did not fulfill the requirements of film even under uniocular observation, because they more regularly implied and aroused typical object meaning. It should be noted in this connection that Fechner[10] stated that certain *O*s see a light shadow hanging upon the surface of an object which is viewed uniocularly, and that Aubert[11] also reported a very delicate shadow spread over the visual field when one eye is closed.

SUMMARY AND CONCLUSION

A number of investigators have reported a change in the brightness or darkness of the visual field attending the sudden shift from uniocular to binocular observation or vice versa. A recent investigator reports in addition that certain minute details of surface are less clearly perceived in uniocular vision. Under bright indoor daylight conditions we have confirmed these points of distinction between the binocular and the uniocular perception of an object.

Furthermore we have added to the list the facts that uniocular impressions are characterized (*a*) by visible reduction of chroma, (*b*) by certain qualitative changes of surface appearance, and (*c*) by indefiniteness of localization. Under improved conditions of observation in the darkroom and by the use of transmitted light, we have found as further distinguishing characteristics of uniocular impressions, (*d*) the somewhat greater distance, as well as the indeterminateness of localization, (*e*) the lack of perspective and the tendency to lack objective reference, (*f*) the lesser degree of transparency, and (*g*) the reduction of hard-surface characteristics. These differences tend to be more pronounced in general with uniformly colored stimuli than with figured and variegated stimuli.

Unlocalizability, the tendency to take the frontal-parallel position, and the lack of definiteness of texture constitute three of Katz's distinguishing characteristics between film and surface colors.[12] The points of distinction between uniocular and binocular impressions as secured under our experimental conditions furnish a striking parallel. If our interpretation be correct, the stimulation of one eye, at least under certain conditions, represents an intermediate and incomplete step in the development of a perception. The nature of the stimulus, the conditions of illumination, and perhaps the mode of stimulation are primary conditions in arresting the process of perception in uniocular vision at this incomplete stage.

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[10] *Op. cit.*, 423.
[11] *Op. cit.*, 282.

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[12] M. F. Martin, Film, surface, and bulky colors and their intermediates, this JOURNAL, 33, 1922, 451 f.

HOW FORMULAE ARE READ

By Miles A. Tinker, Stanford University

The number of items that can be recognized and named from a single visual impression in one exposure whose duration is shorter than the reaction-time of the eye is termed the perception span for the type of material presented. The first important work on perception span in relation to reading appeared about 1885 when Cattell began publishing the results of his tachistoscopic work. From then to about 1910 there was much interest and experimentation on perception span for different kinds of material. Since 1910 there have been practically no investigations along this line. The purpose of this experiment is to determine the perception span for items in mathematical formulae.[1] It is hoped from this determination of the number of items read correctly in formulae to show whether there is any combination of such items into higher perceptual units and, if so, to point out some of the factors involved in the natural groupings.

Comprehensive summaries of the results obtained in experiments on perception span and word perception are given by such writers as Huey[2] and Whipple.[3] The general findings can be briefly stated. When scored for correct placement of right items, the perception span for unrelated letters lies between 2 and 3, and for digits between 3 and 4. Other investigations place the span for letters at 3 to 4, for digits and lines at 4 to 5, and for unrelated words at 2 to 3. When lines are in geometrical forms, and letters in nonsense syllables or words, and words in phrases or sentences the span is increased. Words are usually read as units though not always. The upper part of letters and words are more essential for word perception than the lower part and letter complexes which form the first part have more influence on correct perception than those in the latter part of words. Certain letters and letter complexes (usually consonants which project above the line)[4] are very influential in determining the recognition of words. These have been called "determining letters"[5] as opposed to the indifferent ones.

All of these experiments contribute something of importance for the understanding of the reading process but none of them explain all reading situations. The results obtained from studies related to perception in reading show that there is a natural tendency to combine the different elements of a visual impression into higher perceptual units whenever grouping is possible. This occurs in the perception of symmetrical groups of lines, dots or geometrical figures; in the perception of letters in the form of nonsense syllables or words; in the perception of words in phrases or sentences. No individual can be classified strictly as a certain type of reader, nor does any one person read by the same method in all reading situations. He may read largely by word wholes with certain material while with other more difficult copy his reading may be largely analytical.

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*Accepted for publication June 8, 1927.

[1] This study is part of a reading investigation carried out under the direction of Professor W. R. Miles. The expenses were met from the Thomas Welton Stanford Fund for Psychological Research.

[2] E. B. Huey, *Psychology and Pedagogy of Reading*, 1909.

[3] G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1914, 262-296, 305-329.

[4] Cf. O. Messmer, Zur Psychologie des Lesens bei Kindern und Erwachsenen, *Arch. f. d. ges. Psychol.*, 2, 1903, 190-298; Julius Zeitler, Tachistoskopische Versuche über das Lesen, *Philos. Stud.*, 16, 1900, 380-463.

[5] A. Goldscheider and R. F. Müller, Zur Physiologie und Pathologie des Lesens, *Zsch. f. klin. Med.*, 23, 1893, 131-167.

Method. The tachistoscopic method has been found useful for the determination of perceptual factors involved in visual reading. It has been the principal mode of procedure in studying configuration in reading and the facilitating factors. While short exposure reading is not a normal reading situation, many of the facts educed from this type of investigation have been shown to be intimately related to normal reading. Our immediate problem is to determine the perception span for mathematical formulae.

Five advanced students (R, M, De, Dr, and T), in the Stanford Psychology Department served as *S*s in this experiment. R, M, De, and Dr had completed the equivalent of two courses in high school mathematics, and T had a thorough training in calculus. They were all given the Snellen chart test for visual acuity. Those wearing glasses were allowed to use them during the visual test since they wore them during the experiment. They all made scores of 20/20 or above on the chart.

The stimulus material consisted of mathematical formulae and equations which were cut from algebra texts. The printing types in the various texts were approximately the same. These formulae were pasted at the center of cards about 8 x 9 cm. in size. The formulae were of various sizes and lengths. The smallest consisted of three items and the largest of 15. Fractions as well as linear formulae were used. The total number of formulae was 305 plus those used for the practice series. All were pure algebra except six which were from mechanics.

Apparatus. A Wirth revolving disk tachistoscope was used for exposing the formulae. The disk could be revolved by hand or with a motor. In this experiment it was turned by hand so as to synchronize with the complete swing of a pendulum once in 3 sec. After a certain amount of practice *E* was able to do this with a good degree of constancy. What small variation may have occurred does not affect our experiment since we desired to have an exposure which was less than the reaction-time of the eye and one that was approximately 0.1 sec. Opening the window on the disk to a width of 8.5 cm. and revolving the disk once in 3 sec. gave an exposure of 102 σ. A card carrying a fixation dot remained in the window prior to exposing a formula.

The *S*s sat at the the normal reading distance from the exposed material. This distance was measured at the beginning of every sitting and kept within the limits of 15–16 in.

Procedure. The experiment was conducted under constant artificial lighting. The illumination was 14 foot-candles per sq. ft.

Every *S* observed regularly twice a week. From 25 to 40 formulae were read at every observation period. The first sitting was a practice period. Every day, before any exposures were made, the *S*s read over the following instructions:

"The object of this experiment is to find out how large a formula can be read during a very brief given exposure. Formulae or equations of different lengths and of different types will be exposed in the tachistoscope window, while the time of exposure will be kept the same. After you have viewed a formula or equation, you are to write it down, reproducing the arrangement. You are not required to reproduce it in exact size. If you did not perceive all the material exposed, write as much of it as you can. Draw a circle around any part concerning which you are uncertain.

"Shortly after my 'ready' signal you will hear a loud click. 1½ seconds after this click the formula or equation will be exposed. It will appear in the exact region of the large dot shown prior to the ready signal."

At the end of every series of observations the *S*s made a written report in which they discussed the facility or difficulty encountered in making the observations, the place where difficulties occurred, and the degree of assurance that their responses were correct.

Sometime after this series of observations was completed, 4 of the *Ss* (the other had left the University) served in an experiment to determine the perception span for simple series of unrelated latters. The same apparatus and technique were used as with the formulae. The stimuli consisted of simple series of unrelated letters which were typed on the cards. Care was taken to avoid nonsense syllables. The series varied in length from 3 to 12 letters. In this latter experiment 180 different series were read.

Scoring. The formulae and equations were scored for the number of items correctly reproduced in the right order. Mathematical signs, division lines between numerator and denominator, radical signs, parentheses,[6] etc., were counted as separate items, as well as the letters and digits. Any misplaced item was counted as wrong.

The number of times a formula of any given length was read right or wrong was determined. The average number of items read at each exposure was calculated for the total number of formulae. The average amount of formulae read correctly, of those formulae incompletely read, was determined for formulae involving fractions and also for those not involving fractions.

Experimental Results. The results show a rather wide range of individual differences. For this reason the individual results as well as the averages are given. The mathematically skilled individual (T) was no better than two of the other *Ss* (Dr, De) who are somewhat faster readers than the average as determined on reading tests used in another part of this general investigation. T left before we could determine his reading rate, but we have his verbal statement that he is considered to be somewhat faster than the ordinary reader in speed. In keeping with the criticisms given above, the results of this experiment should be considered in the light of the small number of *Ss* used, and the conclusions with this in mind should be accepted as tentative and suggestive rather than final.

In Table I are shown the number of items read correctly per exposure for formulae and also for unrelated letters. In line 1 of the table is given the average number of items read correctly when only those formulae and equations were considered that were completely read. The average number of items read is 5.10 with a range for the different *Ss* of 4.81 to 5.70. The general average is slightly smaller (5.08) when all formulae are considered (line 5). If we compare this with the last line of the table, which shows the span for unrelated letters, we see that nearly twice as many items are read in formulae as in unrelated letters. For unrelated letters the average number read is 2.96 with a range for the different *Ss* of 2.71 to 3.68.

In line 2 is given the average number of items read when the formula is completely read and involves fractions. Line 3 gives the same for formulae completely read where no fraction are involved. If we now compare the results in lines 1, 2 and 3, we see that it is somewhat easier to read formulae in the form of fractions than linear formulae when they are within the perception span of the individual. The largest formulae read of those which involved fractions varied in size from 7 to 12 items; for linear formulae 8 to 9 items (not given in table). For the whole series, the average length of linear formulae was 5.73 and of fractional formulae 7.45 items.

The upper range for letter series varied from 5 to 7 letters. A difference between linear formulae or letter series and fractional formulae might be expected on two counts: (1) fractions provide a natural grouping schema which resembles the situation with geometrical figures and symmetrical grouping of lines; (2) the vertical axis of the fovea can be used especially in reading fractions, for Ruediger has shown that the area of distinct vision is usually an oblong space and though the horizontal axis is the longer the vertical spread is considerable.[7] For 2 *Ss* (De and T) the advantage derived from having the formulae in the form of fractions is about one item per formula. Further consideration will be given to the fraction-form as a configuration.

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[6] The two parts of a parenthesis were taken as separate items and the misplacement of either part counted wrong.

Line 4 gives the averages where no single formula was completely read. Most of these formulae were those which included a comparatively large number of items. In line 5 are the averages when all formulae read are considered. In comparing line 4 with lines 5 or 1 we find that for all but one *S* (Dr), there is a slight advantage derived from viewing formulae that are ordinarily longer than the span of perception. Whipple found a similar advantage in reading letter series where the longer series gave a somewhat higher average performance.[8]

TABLE I

THE AVERAGE NUMBER OF ITEMS CORRECTLY READ IN FORMULAE AND UNRELATED LETTERS WITH THE PROBABLE ERRORS OF THE AVERAGES

Items	Subjects					Average*
	R	M	Dr	De	T	
1. Complete formulae	4.82±.08	4.81±.08	5.06±.05	5.70±.08	5.44±.08	5.10±.07
2. Complete: fractions	4.80±.15	4.83±.17	5.33±.18	6.29±.17	6.18±.12	5.25±.17
3. Complete: no fractions	4.33±.13	4.81±.09	4.87±.08	5.32±.09	5.11±.08	4.83±.10
4. Incomplete formulae	4.95±.02	4.91±.07	4.73±.07	6.30±.24	6.18±.10	5.20±.10
5. All formulae	4.67±.07	4.87±.05	4.87±.05	5.90±.10	5.73±.10	5.08±.07
6. Unrelated letters	2.71±.05	2.71±.06	2.74±.07	3.68±.05		2.96±.06

*T is not included in average.

Finzi's study in which he exposed groups of nine letters for 16.7 σ is the only other investigation known to us where misplaced items were counted wrong.[9] The perception span for unrelated letters of 2.32 as determined by him is about half our perception span for formulae. Cattell's and also Erdmann and Dodge's perception span for letters is appreciably lower than our formula perception span.[10]

Perception span for formulae seems to bear some relation to speed in reading. Dr, De, and T, who are faster readers than R and M, show a larger span, especially De and T. It is hard to say whether it is greater relative span of perception or greater absolute span which is the influential factor here.

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[7] W. C. Ruediger, The field of distinct vision, *Arch. Psychol.*, No. 5, 1907.

[8] G. M. Whipple, Effect of practice upon the range of visual attention and of visual apprehension, *J. Educ. Psychol.*, 1, 1910, 249-262.

[9] J. Finzi, Zur Untersuchung der Auffassungsfähigkeit und Merkfähigkeit, *Psychol. Arbeiten* (Kraepelin), 3, 1900, 289-384.

[10] J. McK. Cattell, Über die Trägheit der Netzhaut und des Sehcentrums, *Philos. Stud.*, 2, 1886, 94-127; B. Erdmann and R. Dodge, *Psychologische Untersuchungen über das Lesen auf experimenteller Grundlage*, 1898.

If the formulae and isolated letter groups are scored according to the percent of times which a formula or letter-group of any given length is read right and wrong, approximately the same relationship is found as above. Table II compares the perception span for formulae to that for unrelated letters when formulae of any given length are scored right or wrong. Three *S*s (R, M and Dr) read formulae of 5 items correctly more often than wrong while the largest letter groups read in the same way are respectively 3, 4, and 4. The largest formulae read in this way (right more than 50% of the time) by De and T are those of 7 items. The largest letter group for De was 5 which was read right just 50% of the time. The upper range for size of formulae read correctly varied from 9 to 12 items, and for letters from 5 to 7 items. These results, like those in Table I, show that the perception span for formulae is materially larger than that for unrelated letters.

The average percent of the incompletely read formulae which were reproduced correctly and also the percent of the various kinds of items which were grasped correctly in these formulae are given in Table III. It is found, where all items are considered, that on the average 65% of the items were reproduced accurately and that 3% more of the items were grasped in linear than in fractional formulae. If we consider signs alone, however, 81% of the possible items were read correctly on the average and there was practically no difference when the formulae were in the form of fractions and when they did not involve fractions. For digits, 60% were reproduced in the right order on the average and here also there was no practical difference between reading fractions and non-fractions. Considering only the letters it is found that 58% were read correctly on the average and that 12% more were accurately reproduced in linear formulae than in fractional formulae. In linear formulae the items at the right end were usually digits. The fact that these items were frequently not grasped correctly may explain part of the 12% difference. On the average there were 1.61 signs, 2.16 digits and 1.94 letters per linear formula; 2.86 signs, 2.33 digits and 2.25 letters per fractional formula. In general, considering all kinds of formulae, there were 2.20 signs, 2.22 digits, and 2.10 letters per formula, *i.e.* about the same number of each kind of character.

The averages for the correct reading of the various kinds of items in Table III show that signs were correctly reproduced 21% more often than digits, 23% more often than letters and 16% more often than the average for all items. These results indicate that the mathematical signs of formulae as here defined, either in fractions or in non-fractions, are decidedly easier to read from very brief exposures than are either digits or letters. Total shape of the formulae seems to be an important factor involved here and will receive further consideration under the discussion of configuration. In comparing digits with letters it is found that they have about the same amount of correct readings.

Reports of the Subjects. An analysis of the reports of the *S*s throws some light on factors involved in reading formulae from short exposures. The factors tending to make the reading difficult were of five kinds and were mainly concerned with legibility. (1) Small sized type was considered very hard to read, especially in the form of exponents and fractions. (2) Another drawback was the lightness of type face used in many of the formulae. (3) Formulae which were very compact and also those which were complex in structure were obstacles to easy reading. (4) Italicized type, either numbers or letters, was considered difficult to read, especially in the smaller types. (5) Very long formulae tended to confuse *S* so that he usually perceived correctly a smaller number of items than when formulae of medium length were exposed. This is a common experience in all sorts of short-exposure reading.

The principle factors which tended to facilitate tachistoscopic reading of formulae were of four kinds. (1) It was considered easy to grasp short or medium sized formulae whose items were well spread out so there was considerable white space around each item. (2) Simple fractions facilitated ease in reading. (3) Signs of all sorts were said to be easier to grasp than either letters or digits—especially radical signs, parentheses, and lines dividing numerators from denominators. (4) Capitals and large or heavy-faced type were factors that facilitated the reading of formulae.

TABLE II

PERCEPTION SPAN FOR FORMULAE (F) COMPARED TO PERCEPTION SPAN FOR UNRELATED LETTERS (L). Score in percent of times read right and wrong

Formula or letter series length	Reading	SUBJECTS									Average	
		R		M		Dr		De		T*		
		F	L	F	L	F	L	F	L	F	F	L
3	R	95	65	90	80	90	80	95	90	100	94	79
	W	5	35	10	20	10	20	5	10	0	6	21
4	R	82	37	98	54	89	80	93	83	95	91	64
	W	18	63	2	46	11	20	7	17	5	9	36
5	R	66	13	72	20	66	30	83	50	89	75	28
	W	34	87	28	80	34	70	17	50	11	25	72
6	R	32	0	20	3	54	3	74	3	67	49	2
	W	68	100	80	97	46	97	26	97	33	51	98
7	R	28	0	29	0	33	0	68	5	57	43	1
	W	72	100	71	100	67	100	32	95	43	57	99
8	R	9	0	6	0	3	0	45	0	28	18	0
	W	91	100	94	100	97	100	55	100	72	82	100
9	R	3	0	3	0	17	0	40	0	31	19	0
	W	97	100	97	100	83	100	60	100	69	81	100
10	R	0	0	6	0	0	0	12	0	18	8	0
	W	100	100	94	100	100	100	88	100	82	92	100
11	R	0	0	0	0	0	0	13	0	11	5	0
	W	100	100	100	100	100	100	87	100	89	95	100
12†	R	0	0	0	0	0	0	28	0	0	6	0
	W	100	100	100	100	100	100	82	100	100	94	100

*The perception span for unrelated letters was not obtained for T.

†No *S* reproduced all the items in their correct order for formulae containing more than 12 items.

The assurance of correct response was high for short, simple formulae which did not involve exponents. For long formulae there was much more assurance of correctness for the items at the beginning and the end than for those in the middle. All the *S*s reported that they saw more than they could remember long enough to write down. They indicated lack of assurance as to correctness about certain items by drawing a circle about them. This was not trustworthy for the encircled items were more often right than wrong.

Factors in Configuration. The results of this experiment indicate quite clearly that there is a small degree of configuration involved in the reading of formulae. At least two different things can be cited as factors which are very probably involved in the combining of single items into higher units in the reading of formulae and equations. (1) Terry, in an eye-movement study, has shown that there is a tendency to read certain familiar numbers of more than one digit as a single unit.[11] Dearborn has also noted a tendency to grouping when comparatively small numbers were read.[12] In a previous study the writer has shown through the subjective reports of *S*s that very often a number of two or three digits tends to be read as a single unit.[13] There is no reason why this should not also occur sometimes in the reading of simple familiar combinations of a digit and a letter, such as '2x.' (2) The general total form of the formula is undoubtedly a factor in configuration. Goldscheider and Müller noted a decided tendency to grasp more lines exposed tachistoscopically wherever it was possible for the reader to group them into a somewhat regular pattern.[14]

TABLE III

RESULTS FOR INCOMPLETELY READ FORMULAE. The average percent of different kinds of formulae and different kinds of items read correctly, with the probable errors of the averages

Kind of Characters	Kind of Formula	SUBJECTS					Average
		R	M	De	Dr	T	
All items	General (av.)	60±01	64±01	61±01	74±01	72±01	65±01
	Fractional	60±01	59±01	60±01	75±01	72±01	64±01
	Linear	61±01	70±01	63±01	74±01	72±01	67±01
Signs*	Fractional	79±02	80±02	89±01	75±02	85±01	81±01
	Linear	79±02	81±02	92±02	73±03	84±03	82±02
	Av. of both	79±02	80±02	90±01	73±03	84±02	81±02
Digits	Fractional	58±02	48±02	68±03	58±03	63±02	59±02
	Linear	64±02	51±03	67±03	65±03	65±03	62±03
	Av. of both	61±02	49±02	67±03	61±03	64±02	60±02
Letters	Fractional	47±03	52±02	53±05	44±02	62±02	52±03
	Linear	55±04	55±03	77±03	65±03	69±03	64±03
	Av. of both	51±03	53±02	65±04	54±02	65±02	58±03

*The dividing lines between numerator and denominator, radical signs, parentheses, etc., as well as the ordinary arithmetical signs, were put in the category of signs.

The records of all the *S*s show consistently that the general form of the formula or equation was grasped even if none of the letters or numbers was read. Some of the facts involved in the general form are: the two parts of an equation separated by the equality sign, the dividing line between denominator and numerator, prominent signs as the radical sign and parenthesis, and the position of exponents and subscripts. The fact that all sorts of signs were read correctly much oftener than letters or digits (Table III), emphasizes the influence of signs as factors in grouping.

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[11] P. W. Terry, How numerals are read, *Suppl. Educ. Monog.*, 1922, no. 18.

[12] W. F. Dearborn, *Psychology of Reading*, 1906 (also as *Contrib. Philos. and Psychol., Columbia Univ.*, 14, 1906, No. 1).

[13] M. A. Tinker, Reading reactions for mathematical formulae, *J. Exper. Psychol.*, 9, 1926, 444-467.

[14] *Op. cit.*

SUMMARY AND CONCLUSIONS

(1) This study was undertaken to determine the perception span for mathematical formulae and to note any tendency toward configuration in the reading of the formulae.

(2) Individual differences were noticeable: the faster readers had a larger formula span.

(3) The average perception span for items in formulae of our 5 *S*s was nearly twice as great as for unrelated letters: 5.21 in formulae (4 *S*s: 5.08) and 2.96 in letter series. The largest formulae read correctly had 12 items; the longest letter series, 7 letters.

(4) For formulae within the perception span of the individual, those involving fractions are somewhat easier to read.

(5) Formulae read completely and correctly (see Table II) show about the same perception span advantage of formulae over letters that is indicated in conclusion (3).

(6) For formulae incompletely read, 3% more of the items exposed were reproduced correctly in the linear than in the fractional formulae.

(7) Signs in both kinds of formulae were found to be much easier to read than digits or letters.

(8) Only 1% more signs and 3% more digits were grasped in linear than in fractional formulae but 12% more of letters were read correctly where fractions were not involved. Part of this 12% difference was probably due to the letters being largely confined to the left part of linear formulae where the least number of mistakes were made.

(9) In general (see conclusion 3), nearly the same percentage of exposed material was grasped for both types of formulae, fractional and linear, although the number of items in the fractional was about 37% greater. This shows a strong influence of formula contour as an aid to perception.

(10) Five kinds of difficulties were noted by the observers in reading briefly exposed formulae. They were largely legibility factors and consisted of (a) smallness of type, (b) lightness of face of type, (c) compactness and complexity in formula structure, (d) italicized type, and (e) length of formula.

(11) Four different factors which facilitated the reading of formulae were found to be: (a) fairly short formulae whose items were discrete; (b) simple fractions; (c) signs of all sorts; and (d) capitals and large or heavy-faced type.

(12) Lack of assurance as to correctness in the reading of different parts of formulae did not agree with the objective reponse data.

(13) Among the factors influencing configuration or grouping in formulae are the following: (a) the tendency to read numbers of 2 or 3 digits as a unit; and (b) the general total shape of the formula.

(14) The results warrant the conclusion that the perception span for items in formulae is greater (about double) than for unrelated characters and that this larger span is due to a tendency toward configuration.

HEAT INTENSITY

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Though the study of heat which we recently reported was primarily devoted to the qualitative analysis of the experience[1]—we found that heat, aroused by paired temperature stimuli which singly are adequate only to cold and warmth, is psychologically "a simple quality that lies in the pressure-prick-pain continuum, near prick and closer to pressure than to pain"[2]—we nevertheless were able to show that a close relation exists between the intensity of heat and the differential of the stimulus-combinations.[3] Stimulus-combinations with large differentials gave more 'heats' than those with small, and the absolute as well as the relative number of 'mild' and 'intense' reports varied directly with the size of the differential.

Convincing as the statistical evidence may appear, conclusions regarding the correlation between heat intensity and stimulus-differential must be tentative until results are obtained from the direct comparison of different stimulus-combinations. The object of the present study was to obtain these results, *i.e.* to find, by making direct comparisons, how closely intensive gradations of heat are correlated with changes in stimulus-differential.

Apparatus and Procedure

Apparatus. Two temperature grills such as were used in the earlier study, one for each arm, and four constant temperature water-baths, two warm and two cold, were used in the present study. The same checks and controls as in the earlier work were also used in the present study.[4] The grills were placed on a table, 40 cm. apart, and were placed in frame holders, which were so arranged that the spatial relations of the grills could easily and quickly be interchanged. In one series of experiments Grill I was at the left and Grill II at the right; and in a second series Grill I was at the right and Grill II at the left. These two series of experiments were conducted in order to control the space-error. The temperatures employed in the earlier study (2°, 10°, 20°, and 30° C for cold; and 34°, 37°, 40°, and 43° C for warm) were again used in the cold and warm systems of each grill.

Method and instructions. We employed the method of paired comparisons.[5] Every stimulus-combination was presented with every other in the two space-orders (making 256 comparisons for every *O*). After *E* had brought the systems of each grill to the desired temperatures, he called the *O*s, who were waiting in an adjoining room, one at a time, and asked them to compare the intensities of the heat experiences aroused by the two grills. The following instructions were read to the *O*s at the beginning of every experimental hour.

"At the signal 'now,' place the volar surfaces of your forearms upon the grills before you. Relax and keep the pressure of your arms as constant as possible. Report which of the two grills, if either, gives the experience of heat. If both are hot, report which of the two is the hotter."

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*Accepted for publication May 1, 1927.

[1] N. C. Burnett and K. M. Dallenbach, The experience of heat, this Journal, 38, 1927, 418-431.

[2] *Ibid.*, 423, 431.

[3] *Ibid.*, 423-425.

[4] *Ibid.*, 419.

[5] E. B. Titchener, *Experimental Psychology, Qualitative, Student's Manual*, 1901, 92 ff.; *ibid.*, *Instructor's Manual*, 1901, 151 ff.

Observers. Our *Os* were Dr. C. A. Dickinson (Dn), honorary fellow in psychology; Mr. J. P. Guilford (G), assistant in psychology; Dr. Harry Helson (H), instructor in psychology; and the senior author (D). H and D had served in the earlier study.

Procedure. *O* seated himself at the grill-table as soon as he came into the experimental room, and at the signal from *E* placed the volar surface of his forearms on the grills. There was no time limit; *O* gave his report as soon as he had reached a judgment. All the *Os*, however, made their judgments easily and quickly. Equal judgments were permitted. Out of the 240 experiments in which different stimulus-combinations were compared, D gave 10 'equal' judgments; Dn, 1; G, 12; and H, 5. The relative infrequency of the equal judgments indicates the ease with which the judgments were given. As soon as *O* had given his judgment he left the experimental room, and one of the other *Os* entered. When they had all given their judgments upon a particular pair of stimulus-combinations, *E* interchanged the positions of the grills, and the experiments were repeated with the same stimulus-combinations but in the other space-order. None of the *Os* except D was aware of this procedure, which was adopted merely to save time. The adjustment of the temperatures of the different systems of the two grills was more time-consuming than the taking of the *Os*' judgments.

After the reports had been received from all the *Os* for the particular setting in the second space-order, the temperature in one of the systems of one of the grills was changed, and the grills—still in the second space-order—were again submitted to the *Os* for their judgments. After their judgments had been given for this setting in this space-order, the grills were shifted to the first space-order and again submitted to the *Os*. Alternation in space-order was thus continued during the entire experiment.

RESULTS

The results of the experiments are shown in the accompanying tables and figures. The crude data are shown in Table I. For the want of space, we give the results in Table I of but two *Os*, D and H. We present the data of these *Os* because they mark, as is shown in Tables II and III, the extremes of our *Os* in sensitivity to heat. H is the most sensitive,[6] and D the least.

The boldface diagonals in Table I give the results of the *Os*' judgments when identical stimulus-combinations were compared. The data below the diagonal show the results of the *Os*' judgments in Series 1, *i.e.* the first space-order, and the data above the diagonal show the results in Series 2—the second space-order.

(1) *Distribution of the intensive judgments.* The number of times that the different temperature-combinations were judged as 'hotter' by the *Os* is shown in Table II and in Figs. 1–3. These data prove that the intensity of the sensation is dependent upon the stimulus-differential. The greater the stimulus-difference, with any given setting of the warm or the cold systems, the more frequently is the judgment of 'hotter' returned. This relation is clearly shown in Figs. 2 and 3, in which the results are shown separately for the cold and warm temperatures. In Fig. 2, the judgments of 'hotter' are shown in relation to the cold stimuli; in Fig. 3, they are shown in relation to the warm stimuli. The curves of the 'hotter' judgments in Fig. 2 show a marked tendency to fall as the stimulus-difference becomes smaller; and in Fig. 3 they show an even more pronounced tendency to rise as the difference becomes larger. The correlation indicated in the earlier study is thus confirmed by a direct comparison of the intensities of the heat

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[6] Burnett and Dallenbach, *op. cit.*, 424 f.

TABLE I

CRUDE DATA FOR D AND H, SHOWING THE JUDGMENTS OF 'HOTTER' RETURNED BY EACH FOR THE TWO SPACE ORDERS

Bold-face diagonal gives the judgments of identical stimulus-combinations; data below diagonal show judgments of first space order, above diagonal of second space order

O		Stimulus-temperatures		Grill II															
				2°+34°	2°+37°	2°+40°	2°+43°	10°+34°	10°+37°	10°+40°	10°+43°	20°+34°	20°+37°	20°+40°	20°+43°	30°+34°	30°+37°	30°+40°	30°+43°
			No.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
D	Grill I	2°+34°	1	**no**	2	13	4	no	1	7	=	no	10	no	1	no	no	no	16
		2°+37°	2	2	**R**	=	=	2	2	2	2	2	2	2	2	no	2	2	2
		2°+40°	3	3	3	**=**	4	3	3	3	3	3	3	3	3	3	3	3	3
		2°+43°	4	4	4	4	**R**	4	4	4	4	4	4	4	4	4	4	4	4
		10°+34°	5	no	no	3	4	**no**	no	7	8	no	no	11	12	5	no	no	no
		10°+37°	6	no	2	3	4	no	**L**	6	=	6	6	6	12	6	6	6	6
		10°+40°	7	no	2	3	4	7	7	**R**	8	7	7	7	=	7	7	7	7
		10°+43°	8	8	no	3	4	8	8	8	**R**	8	8	8	8	8	8	8	8
		20°+34°	9	no	2	3	4	no	no	7	8	**no**	no	no	no	no	no	15	16
		20°+37°	10	no	2	3	4	no	6	7	8	no	**no**	no	=	10	10	=	16
		20°+40°	11	1	11	3	4	11	11	7	=	no	11	**L**	11	11	11	11	11
		20°+43°	12	no	2	3	4	12	12	=	8	no	12	12	**=**	12	12	12	12
		30°+34°	13	no	no	3	4	no	6	7	8	no	no	11	12	**no**	no	no	16
		30°+37°	14	no	no	3	4	5	no	7	8	no	no	11	12	no	**no**	no	16
		30°+40°	15	no	no	3	4	no	no	7	8	no	no	11	12	no	no	**L**	15
		30°+43°	16	16	16	3	4	no	no	7	8	16	16	16	=	no	16	16	**L**
H	Grill I	2°+34°	1	**=**	2	3	4	1	1	1	8	1	10	11	1	1	14	1	16
		2°+37°	2	2	**=**	3	4	2	=	2	8	2	2	2	2	2	2	2	2
		2°+40°	3	3	3	**R**	4	3	3	3	3	3	3	3	3	3	3	3	3
		2°+43°	4	4	4	4	**=**	4	4	4	4	4	4	4	4	4	4	4	4
		10°+34°	5	no	2	3	4	**no**	5	7	8	5	5	11	12	5	5	5	5
		10°+37°	6	6	2	3	4	6	**=**	6	6	6	6	6	6	6	6	6	6
		10°+40°	7	7	2	3	4	7	7	**R**	8	7	7	7	7	7	7	7	7
		10°+43°	8	8	2	3	=	8	6	8	**R**	8	8	8	8	8	8	8	8
		20°+34°	9	no	2	3	4	5	6	7	8	**no**	no	11	12	9	9	9	9
		20°+37°	10	10	2	3	4	10	6	7	8	no	**L**	10	12	10	10	10	=
		20°+40°	11	1	2	3	4	11	11	=	8	11	11	**L**	11	11	11	11	11
		20°+43°	12	12	2	=	4	12	12	12	8	12	12	12	**R**	12	12	12	12
		30°+34°	13	13	2	3	4	5	6	7	8	no	10	11	12	**no**	no	15	16
		30°+37°	14	14	2	3	4	5	6	7	8	14	10	11	12	13	**L**	14	16
		30°+40°	15	15	2	3	4	5	6	7	8	15	10	11	12	15	15	**L**	16
		30°+43°	16	16	2	16	4	16	6	7	8	16	16	11	12	16	16	16	**L**

sensations aroused by the different stimulus-combinations: the intensity of the heat sensation is dependent upon the stimulus-differential. With any given cold stimulus, the greater the difference in temperature between it and the warm stimulus (an 'intra-standard' comparison), the greater is the intensity of the heat sensation. For example, 2°-34° for D gives 3.5 'hotter' reports; 2°-37° gives 19, 2°-40° gives 27.5; and 2°-43° gives 29.5. Similarly, with any given warm stimulus, the greater the difference in temperature between it and the cold stimulus, the greater is the intensity of the resulting heat sensation. For example, 43°-30° for H gives 12.5 'hotter' reports; 43°-20° gives 18.5; 43°-10° gives 23.5; and 43°-2° gives 29.5. ('Equal' judgments were counted as 0.5 for each of the stimulus-combinations involved in a particular experiment.)

TABLE II

NUMBER OF TIMES THAT THE DIFFERENT TEMPERATURE-COMBINATIONS WERE JUDGED AS 'HOTTER'

Stimulus-temperatures			Observers												
			D			Dn			G			H			
Cold	Warm	No.	Space order		Total	Space order		Total	Space order		Total	Space order		Total	
			1	2		1	2		1	2		1	2		
2°	34°	*1*	1	2.5	3.5	1	1	2	0	0	0	1	7	8	
	37°	*2*	6	13	19	10	12	22	4	6	10	13	11.5	24.5	
	40°	*3*	14	13.5	27.5	13	15	28	11	12.5	23.5	12.5	14	26.5	
	43°	*4*	15	14.5	29.5	15	14	29	14	13.5	27.5	14.5	15	29.5	
10°	34°	*5*	1	1	2	0	4	4	0	0	0	4	7	11	
	37°	*6*	2	8.5	10.5	8	8	16	4.5	2.5	7	9	10.5	19.5	
	40°	*7*	9.5	9.5	19	10	9	19	7.5	8.5	16	9.5	9	18.5	
	43°	*8*	11.5	11	22.5	13	12	25	12.5	11.5	24	11.5	12	23.5	
20°	34°	*9*	0	0	0	0	0	0	0	0.5	0.5	0	4	4	
	37°	*10*	0	4	4	2	3.5	5.5	3.5	3.5	7	5	5.5	10.5	
	40°	*11*	7.5	6	13.5	5	7	12	7	7.5	14.5	8.5	8	16.5	
	43°	*12*	8	7	15	11	9	20	12	9.5	21.5	11.5	7	18.5	
30°	34°	*13*	0	0	0	0	0	0	0	0	0	2	0	2	
	37°	*14*	0	0	0	0	0	0	1	2	3	2	2	4	
	40°	*15*	0	2.5	2.5	2	2.5	4.5	8	8	16	4	1	5	
	43°	*16*	7.5	5	12.5	9	7	16	10	12.5	22.5	8	4.5	12.5	
Total			83	98	181	99	104	203	95	98	193	116	118	234	

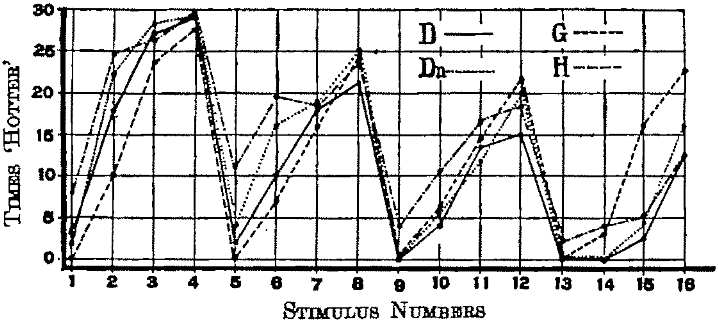

FIG. 1. Showing the number of times the different temperature combinations were judged as 'hotter'

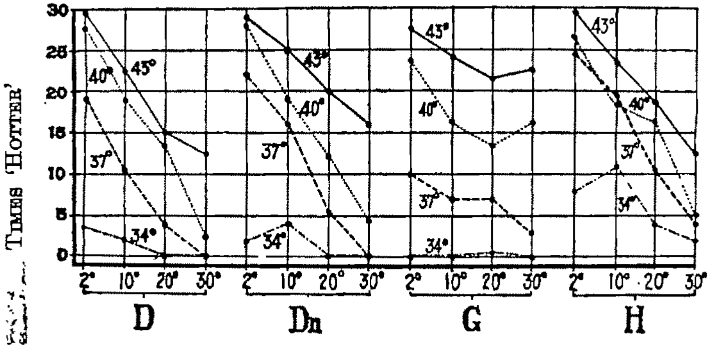

FIG. 2. Judgments of 'Hotter' shown in relation to cold stimuli

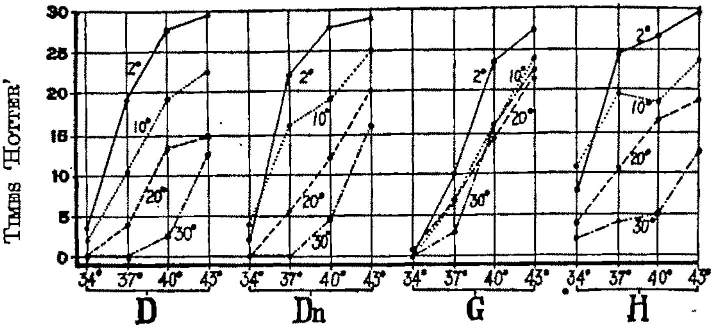

FIG. 3. Judgments of 'Hotter' shown in relation to warm stimuli

The relation between the stimulus-difference and the intensity of the sensation holds when 'intra-standard' differences are considered, no matter what the standard may be, whether warm or cold. It does not hold, however, when comparisons of 'inter-standard' differences are made, *i.e.* when we compare the number of 'hotter' reports in the case of stimulus-differences with different standards. For example, the difference between 2° and 34° is 32, and that between 30° and 43° is 13; yet the stimulus-combination 30°-43°—in spite of the fact that it has the smaller numerical difference—was judged as 'hotter' by all the *O*s more frequently than was the combination 2°-34°. These instances do not, however, invalidate our general conclusion—that the intensity of the heat sensation is dependent upon the stimulus-differential; they merely show that the stimulus-differential is not adequately represented by the numerical differences between the temperatures of the warm and cold systems of the heat grill.

TABLE III

NUMBER OF TIMES FOR THE DIFFERENT TEMPERATURE-COMBINATIONS THAT HEAT WAS NOT EXPERIENCED

Stimulus-temperatures			Observers				Total
Cold	Warm	No.	D	Dn	G	H	
2°	34°	*1*	17	17	16	2	52
	37°	*2*	6	1	9	0	16
	40°	*3*	0	0	1	0	1
	43°	*4*	0	0	0	0	0
10°	34°	*5*	17	8	16	3	44
	37°	*6*	7	2	10	0	19
	40°	*7*	1	0	2	0	3
	43°	*8*	1	0	0	0	1
20°	34°	*9*	18	10	16	6	50
	37°	*10*	11	9	7	2	29
	40°	*11*	4	3	2	0	9
	43°	*12*	3	0	0	0	3
30°	34°	*13*	15	13	13	4	45
	37°	*14*	14	10	11	1	36
	40°	*15*	12	7	3	0	22
	43°	*16*	4	2	0	0	6
Total			130	82	106	18	336

(2) *Cases in which heat was not experienced.* Our conclusion, that the intensity of the heat sensation is dependent upon the stimulus-differential, receives negative as well as positive confirmation from our data. The negative evidence is shown in Table III, in which the number of times that heat was not experienced at the different temperature-combinations is recorded separately for the different *O*s. The 'no' judgments in Table I, which presents the crude data for D and H, show the various experiments and the stimulus-combinations when they did not experience heat. Since two stimulus-combinations are involved in every judgment—one combination in each of the grills—every 'no' report must be counted twice: once across Table I for the temperature-combination used in Grill I; and a

second time down the table for the temperature-combination used in Grill II. The number of cases shown in Table III is, therefore, twice the number actually reported.

Table III shows, for 'intra-standard' comparisons, that fewer 'no' reports were returned by all the *O*s for stimulus-combinations with large temperature-differences than for stimulus-combinations with small temperature-differences. For example, taking 30° (cold) as the standard, Dn reported 'no' 13 times with the stimulus-combination 30°-34°; 10 times with 30°-37°; 7 times with 30°-40°; and 2 times with 30°-43°. Again, taking 43° (warm) as the standard, G and H reported heat in every experiment; Dn reported it in every experiment but one, and in that instance the temperature combination was 43°-30°—the smallest stimulus-difference used with that standard; while D reported 'no' 4 times with the stimulus combination 43°-30°, 3 times with 43°-20°, once with 43°-10°, and no times with 43°-2°. The 'intra-standard' comparisons show that the number of 'no' reports varies inversely with the stimulus-difference: the larger the stimulus-difference, the fewer the number of reports.

When we turn to 'inter-standard' comparisons we find, as we did in the case of the positive evidence, contradictory results. In a number of instances, as Table III shows, the number of 'no' reports varies directly with the stimulus-difference. For example, G gave 13 'no' reports at 30°-34°—a stimulus-difference of 4; and 16 'no' reports at 2°-34°—a stimulus-difference of 32. Such instances, however, do not (as we have said above regarding the apparently contradictory results of the 'inter-standard' comparisons) affect our conclusions. They merely indicate that the stimulus-differential is not represented by numerical differences. Thus the negative evidence corroborates the positive at every point.

TABLE IV

NUMBER OF TIMES, WHEN IDENTICAL STIMULUS-COMBINATIONS WERE COMPARED, THAT 'NO HEAT' AND 'EQUAL' JUDGMENTS WERE RETURNED, AND THE NUMBER OF TIMES THAT 'HOTTER' WAS REPORTED AT THE RIGHT AND AT THE LEFT

O	No heat	Equal	Left 'hotter'	Right 'hotter'
D	6	2	4	4
Dn	4	1	7	4
G	6	3	7	0
H	3	4	4	5

(3) *Space error.* As is shown in Table II, the difference between the number of 'hotter' reports given in the two space orders is so small that we may, at least for three of the *O*s (Dn, G, and H), conclude that the difference (5, 3, and 2 cases respectively) is due to chance, and that the space order is not effective in judgments of the intensity of heat that are aroused on the volar surfaces of the left and right forearms. In the case of the fourth *O* (D) the difference—15—is of the order that makes it significant. D seems to be slightly disposed toward judgment to the right.

Our general conclusion, however, that the space order is not effective under our conditions, receives confirmation when the results of the comparisons of identical stimulus-combinations are taken into consideration. Of the 16 comparisons in which identical stimulus-combinations were used in the two grills (the results of which are shown for D and H in the boldface diagonal in Table I) D reported, as Table IV shows, the 'hotter' experience 4 times at the right and 4 times at the left. These data indicate that D is evenly balanced as between the right and the left, and that if there is any disposition toward the right, as the results of the total experiment seem to indicate, it is slight. H reported the 'hotter' experience 5 times at the right and 4 times at the left—no indication of a space error there. The data of Dn and of G, on the other hand, show a slight disposition to the left. Dn reported the 'hotter' experience 7 times at the left and 4 times at the right; and G reported it 7 times at the left and no times at the right. The disposition of these two *Os* to the left in these experiments is offset, however, by their slight disposition to the right in the main experiments (see Table II).

(4) *An equation for the stimulus-differential.* The intensity of the heat sensation does not, as the results presented above clearly show, vary directly with the size of the numerical difference between the stimulus-temperatures. This result merely indicates, as we have already observed, that the equation $h \propto w - c$ (in which h equals the heat intensity; w, the temperature of the warm system; and c, the temperature of the cold) is not adequate to our results. The question we now have to consider is whether we can determine empirically an equation from our data that is adequate. We think we can.

TABLE V

NUMBER OF TIMES THAT 'HOTTER' REPORTS WERE RETURNED BY ALL THE *Os* FOR THE DIFFERENT COLD AND WARM TEMPERATURES USED IN THE VARIOUS STIMULUS-COMBINATIONS

O	Cold				Warm			
	2° *30	10° 22	20° 12	30° 2	34° *1	37° 4	40° 7	43° 10
D	79.5	54.0	32.5	15.0	5.5	33.5	62.5	79.5
Dn	81.0	64.0	37.5	20.5	6.0	43.5	63.5	90.0
G	61.0	47.0	43.5	41.5	0.5	27.0	70.0	95.5
H	88.5	72.5	49.5	23.5	25.0	58.5	66.5	84.0

*Stimulus-values, *i.e.* the difference between the stimulus-temperature and physiological zero.

We have no incontrovertible results regarding the difference limens of cold, warm, and heat;[7] consequently we shall be forced to make some assumptions which only the results of future experiments can test. We may safely assume, however, that the effectiveness of a temperature varies with its distance from physiological zero, and further that the greater this distance the greater proportionately is its effectiveness. We did not determine the value of physiological zero for the forearms of our *Os*, but it is certain from our results that it lies somewhere between 30°C and 34°C—the highest cold temperature and the lowest warm temperature used; and also that it lies closer to 34° than to 30°. This conclusion is based upon the results given in Table V, which shows the number of times that 'hotter' reports were returned by every *O* for the various stimulus-temperatures. Fewer 'hotter' reports were returned by the *Os* for 34° than for 30°. This must mean that 34° lies closer to the physiological zero than does 30°. We have consequently taken 32.5°C as the point of physiological zero, and have further assumed that the zone of indifference extends 0.5°C on each side of it, *i.e.* from 32°-33°. The stimulus-values of the different stimuli can, therefore, be expressed by the equations: $C = 32 - c$; and $W = w - 33$ (in which C stands for the cold stimulus; 32, for the temperature of the lower limit of the zone of indifference; c, for the temperature of the cold stimulus; W, for the warm stimulus; w, for the temperature of the warm stimulus; and 33, for the temperature of the upper limit of the zone of indifference).

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[7] For a review of the literature on this problem see Sydney Alrutz, Untersuchungen über die Temperatursinne, *Zsch. f. Psychol.*, 47, 1908, 182-202, esp. 262-267. Cf. also C. S. Sherrington in Schäfer's *Text-Book of Physiology*, ii, 1900, 959 f.

The stimulus-values of the different warm and cold temperatures, as they were computed by these formulae, are given in Table V under the different stimulus-temperatures. If now we simply add together the stimulus-values of the different temperatures used in the different stimulus-combinations, *i.e.* if we assume that $h \propto (32 - c) + (w - 33)$, we are back again where we were before, for $(32 - c) + (w - 33) = w - c - 1$ (1 being the extent of the zone of indifference).

It is obvious from our results that we must, in order to realize the relative effective value of the stimuli, weight one of the stimulus-values by a constant, k. If we knew what the difference limens of warm, cold, and heat are, we should have some indication of what to expect; but lacking those values, we can merely turn to the data in Table V in the hope of securing some hint regarding the weighting. An inspection of Table V shows that approximately the same number of 'hotter' reports were returned by all the *O*s for the temperature-combinations in which 43°C was used, as for those in which 2°C was used. D reported identically the same number (79.5) for each; Dn reported 90 cases for 43°, and 81 for 2°; G, 95.5 for 43°, and 61 for 2°; and H, 84 for 43°, and 88.5 for 2°. All the *O*s show that a temperature of 43° is as effective for heat as a temperature of 2°; that is, since $43° = 10\ W$, and $2° = 30\ C$, $10\ W \doteqdot 30\ C$. These results are confirmed by the comparisons of the 'hotter' reports received for the temperatures of 40° and 10°, and of 34° and 20°: $7\ W \doteqdot 22\ C$; and $4\ W \doteqdot 12\ C$.[8] We conclude from these results that the warm stimulus-value should be weighted, and that the value of the weight, k, is 3.

We have accordingly calculated the effective values, *i.e.* the stimulus-differentials, for the various temperature-combinations by the formula $(32 - c) + 3(w - 33)$, in which the values 32 and 33 represent the temperatures of the extremes of the zone of indifference, c and w the temperatures respectively of the cold and warm stimuli, and 3 the weight. These values, ranged in their descending rank order, appear in Table VI under the caption 'Stimulus-differential.' Opposite them, in the columns to the left are arranged the stimulus-temperatures from which they were computed; and in the columns to the right are arranged the number of times 'hotter' reports were returned by the *O*s for every one of the stimulus-differentials, together with the rank order of these reports.

Inspection of Table VI shows that the rank order of the *O*s' 'hotter' reports follows very closely the rank order of the stimulus-differential as that value was computed by our weighted formula. The correlation of the rank orders of the *O*s' reports with that of the stimulus-differential, computed by the method of rank-differences—$r = 1 - [6\Sigma D^2/n(n^2 - 1)]$—is given at the foot of the table for every *O*. The correlations are high—exceptionally high for D, Dn, and H—and lead us to offer tentatively the formula

$$h \propto (32 - c) + 3(w - 33)$$

as an equation of the stimulus-differential for heat.

We are, in the light of our results, forced to the conclusion that a change in the warm stimulus is more effective for changing the intensity of the sensation of heat than a change in the cold stimulus.[9]

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[8] The ratio does not hold for the lowest stimulus-values that we used—due probably to their proximity to the zone of indifference.

[9] Alrutz says, "die Kälteempfindung von grösserer Bedeutung für die Intensität der Hitzeempfindung zu sein scheint als die Wärmeempfindung" (*op. cit.*, 244).

APPARATUS

A SMALL SELF-STARTING SYNCHRONOUS MOTOR

By Willard L. Valentine, Ohio Wesleyan University

A small and inexpensive self-starting synchronous motor is now obtainable. It is described here because of its many uses in the psychological laboratory, and because it seems to be comparatively little known among experimentalists.

These motors satisfactorily drive kymograph drums with as much accuracy and less noise (in fact they are practically noiseless) than the more expensive, larger, and weaker spring motor generally provided. They may be mounted directly to the shaft as they are obtainable in several R.P.M.s and in either direction of rotation.

The starting torque is due to magnetic remanence in a very small, thin rotor located in a revolving field, and it is only slightly different from the synchronous torque so only a fraction of a sec. is required to reach synchronous speed.

There are three types and two sizes of motors. Every type and size has been designed to meet a certain, specific need; accordingly, the type B-2 motor is adapted for light duty; the type B-3 motor for small power uses such as in recording meters, time stamps, relays, etc.; and the type C motor for reasonably heavy power adaptations such as in heavy recording meters, unrolling paper and film at a uniform rate, etc. Each type of motor may be arranged to operate on any commercial voltage and frequency and to give various terminal shaft speeds. The use of standard speeds and frequencies will greatly improve the efficiency of the set-up.

There is a small bipolar field (A), in the accompanying Plate (Plate I), that is excited by a coil (B) taking power from a single-phase alternating current circuit. Each pole is split and has a number of copper shading coils (C) upon one of its arms. Located in the field and having a relatively large air gap is a combination of rotors (G), depending upon the type of motor. These rotors are of thin hardened magnet steel and have a high magnetic remanence. The shaft rotates at synchronous speed and, through a train of gears and pinions (F), the speed is reduced to the desired value at the terminal shaft (H).

The motor casing is made of a thin brass shell and cover that are drawn into shape, and these are soldered together after the motor has been assembled. Lubricating oil is applied through a hole in the rotor neck which is then soldered tight. In this manner the rotating parts are totally enclosed and protected from all dirt and dust, and require no further lubrication.

Another use to which these motors have been put in this laboratory concerns timing. If a small fiber disk is provided, grounded on one side by a thin brass plate, and presenting points on the other in the form of brass or silver wires introduced through holes so as to be in contact with the brass disk on one side and dressed flush to the surface on the other, a friction contact will then make and break the circuit any number of times depending on the R.P.M. of the motor and the number of studs which move under the contact. A 60-R.P.M. motor with two studs will make contact twice every sec. and may be substituted for the more clumsy electric metronome. These motors, together with a much more powerful and slightly more expensive one running at 1800 R.P.M., may be obtained from the Warren Telechron Company, Ashland, Mass., to whom I am indebted for the ratings above.

The specifications given in Table I show the various types of motors produced by this company, as well as the minimum synchronous torque ratings developed by motors of various shaft speeds.

TABLE I

MOTOR SPECIFICATIONS

Type of motor	Volts	Cycles	Watts rated	Minimum synchronous torque ratings expressed in inch-pounds, developed by motors of various shaft speeds				Overall dimensions of motors
				1 R.P.M.	2 R.P.M.	4 R.P.M.	60 R.P.M.	
B-2	12-250	60	2	0.125	0.06	0.030	None	2 5/16"x2 3/16"x2 3/8"
		50	2	0.104	0.05	0.030	"	
		40	3	0.080	0.04	0.020	"	
		30	3	0.060	0.03	0.015	"	
		25	3	0.050	0.02	0.010	"	
	250 up	all	4	Same as above				
B-3	12-250	60	4	0.375	0.19	0.10	0.0060	2 5/16"x2 3/16"x2 3/8"
		50	4	0.310	0.16	0.08	0.0050	
		40	4	0.250	0.13	0.06	0.0040	
		30	4	0.190	0.09	0.05	0.0030	
		25	4	0.160	0.08	0.04	0.0026	
	250 up	all	6	Same as above				
C-1	12-250	60	6	1.0	0.50	0.25	None	3 3/8"x3 3/16"x2 27/32"
		50	6	0.8	0.40	0.20	"	
		40	6	0.7	0.35	0.17	"	
		30	6	0.5	0.25	0.13	"	
		25	6	0.3	0.16	0.08	"	
	250 up	all	9	Same as above				

Motors can be furnished for operation on circuits of the following odd frequencies: 41⅔, 42, 59½, 61⅔, 62, 62½, 63⅓, 66⅔. Gear ratio for 59 cycles is not exact.

APPARATUS NOTES

PHOTOGRAPHS IN COMPLEMENTARY COLORS FOR THE DEMONSTRATION OF NEGATIVE AFTER-IMAGES

The writer uses color-photographs of the mosaic-screen type (Agfa Color-Plates) to demonstrate to classes (1) the mixture of colors by means of juxtaposition of small particles of color, each of which is too small to be seen singly, and (2) the production of all color sensations by the mixture of three fundamental colors. These photographs, which are transparencies upon glass, may be examined by means of a lens or they may be projected as lantern slides in order to show the granular construction of the screen.

By means of modification of the treatment of the color-plate after it has been exposed in the camera, a transparency may be obtained in which the object photographed is represented in colors complementary to its actual colors. If this transparency is strongly illuminated from behind, a negative after-image may be obtained in which the object appears in its actual colors. Faces may be recognized in the after-image which were not recognized in the complementary colors. If two pictures are made of the same object, one in actual and one in complementary colors, students may compare the two and thus study the relation of complementariness existing between various hues.

The method of making color-photographs is given in the handbooks of photography and in the literature of the Agfa Ansco Corporation. To get complementary colors, the method should be modified thus: after the first development, the plate should be put into a solution of plain hypo of the strength ordinarily used for fixing plates. After the plate has been thoroughly fixed, it should be washed in running water 10 min. and the process completed by drying it in the blast of an electric fan. If necessary, the edges of the plate may be dipped in melted paraffin before development in order to prevent 'frilling.' Since this process makes a negative instead of a positive image on the plate, the object photographed is rendered in complementary colors and brightness. The only difficult feature of color-photography is the determination of the proper exposure, upon which accuracy of rendition and the density of the plate depend. Out of doors some one of the more accurate exposure-meters should be used; under artificial light a few trial exposures may be necessary before the correct time is found. Ordinary exposure and ordinary development are likely to give too much green when the above modified procedure is employed, but this excess of green will not appear if the plate is slightly over-exposed and its development correspondingly shortened.

Harvard University FRANK A. PATTIE, JR.

CLASS-ROOM DEMONSTRATION OF DOUBLE IMAGES

The following method of demonstrating double images is recommended on account of its simplicity and instructiveness. On a card or sheet of paper draw a single straight line 15 cm. or more in length. Hold the line in a horizontal plane just below the level of the eyes with one end toward the tip of the nose. Move the point of a pencil slowly back and forth along the line and follow its movements with the two eyes. Two lines will be seen crossing at the point of regard. Although the moving pencil aids fixation, it is not essential. With the unaided eyes the point at which the lines cross varies with change of convergence. If the plane be turned gradually from the horizontal to the vertical position, the double images tend to disappear. The addition of parallel lines makes the demonstration even more striking. The writer has frequently used this demonstration in the class-room and recommends it in connection with the topics of convergence, double images, and the horopter.

University of Illinois PAUL THOMAS YOUNG

BOOK REVIEWS

Edited by Joseph Peterson, Peabody College

(I) *Die Methodik der Erforschung der bedingten Reflexe.* By N. A. Pidkopaew. Translated by M. Krich. München, J. F. Bergmann, 1926. Pp. vi, 64.

(II) *Die höchste Nerventätigkeit (das Verhalten) von Tieren. Eine zwanzigjährige Prüfung der objektiven Forschung bedingte Reflexe.* By J. P. Pawlow. Translated by G. V. Volborth. München, J. F. Bergmann, 1926. Pp. xi, 330.

(III) *Conditioned Reflexes. An investigation of the physiological activity of the cerebral cortex.* By I. P. Pavlov. Translated by G. V. Anrep. Oxford University Press, 1927. Pp. xv. 427.

The books listed above are certain to influence the trend of investigation in both psychology and physiology, for the simple reason that an imposing mass of data concerning animal behavior in relation to the structure and function of the brain is here, for the first time, made available in systematic form. Previous scattered publications of the methods and results of the investigation of conditioned reflexes by Pavlov and his more than one hundred colleagues have, for the most part, appeared in Russian, and they have, therefore, been practically inaccessible to the non-Russian reading public.

It is not only the wealth of data contained in these books but particularly the clear and detailed description of the method of the conditioned reflex which is certain to act as a powerful incentive to more careful experimental analysis of behavior and more critical theorizing concerning it in this country.

These books should be read in the order listed above. The first book on method, particularly Chapter IV on difficulties of the method, cannot be neglected if one is to obtain a true appreciation of the results presented in the two following volumes by Pavlov. Moreover, a reading of Chapter III concerning the formation of positive and negative conditioned reflexes will do much to remove the false notion of many psychologists that the method cannot yield results of great significance because of the artificiality of the animals' experimental environment and of the problems which it must solve. A precise technique is employed, to be sure, but it is the characteristics of the animal as an individual that govern the actual experimental procedure.

"Indeed every dog has its own individuality, *i.e.* a sum of the positive and negative unconditioned and conditioned reflexes which it has acquired during its life outside of the laboratory and which determines its behavior. . . . I shall now, in a general way, sketch a program to be followed during the first days of the procedure. 1st Day: . . . The experimenter himself leads the dog into the experimental room, pats him, calls him by name, and feeds him a small portion of meat powder out of his hand. 2nd Day: The same. Then he places the dog on the stand whilst fastening him by the collar only, without attaching the loops. Thereupon he feeds the dog several times, first from the hand, and then from the feeding device. After he has been on the stand 10-15 min. he is freed and given a bone" (I, pp. 30 f.).

The second book is a collection of offprints, 37 in number, dating from 1903–1925, printed in chronological order on the general problem of the conditioned reflex. Each chapter is an essay complete in itself and in reading them consecutively one follows the growth of the author's searching conception of the activity of the brain in controlling behavior.

The third book is a masterpiece of scientific exposition in which the presentation of data collected over a period of twenty-five years contributes to the building of a theory of behavior too profound to be either thoroughly systematic or complete.

This is not the place to summarize in detail his fascinating analysis of animal behavior. It is important, however, to comment briefly on the contents of the above books under the following heads: (1) the origin of the conditioned reflex concept; (2) Pavlov's view on the relation between the physiology of the nervous system and psychology; and (3) the relation between behaviorism and the study of the conditioned reflex.

The origin of Pavlov's working concept of the conditioned reflex was the natural outgrowth of twenty years of uninterrupted investigation concerning the activity of the digestive glands. In fact, the idea was forced upon him in the course of a highly technical research devoted to salivation. In no sense was it a product of leisurely speculation. He says, "At about the same time as Thorndike was engaged on this work, I myself (being then quite ignorant of his researches) was also led to the objective study of the hemispheres, by the following circumstance: In the course of a detailed investigation into the activities of the digestive glands I had to inquire into the so-called psychic secretion of some of the glands, a task which I attempted in conjunction with a collaborator. As a result of this an unqualified conviction of the futility of subjective methods of inquiry was firmly stamped upon my mind. It became clear that the only satisfactory solution of the problem lay in an experimental investigation by strictly objective methods. For this purpose I started to record all the external stimuli falling on the animal at the time its reflex reaction was manifested (in this particular case the secretion of saliva), at the same time recording all changes in the reaction of the animal" (III, p. 6). He adds, "The primary aim of our research was the accurate determination and tabulation of different phases of the cortical activity, the absence or presence of an inhibitory or excitatory phase, the exact conditions under which the intensity of the excitatory or inhibitory process varied, and the mutual interrelation between these processes. It is obvious that in its intrinsic nature our work is closely allied to the work of Sherrington and his co-workers upon the spinal cord, and it is impossible not to notice in how many points the different aspects of the nervous activity of the cortex correspond with those described in the physiology of the spinal cord; a fact which seems strong evidence of a similarity of the fundamental laws governing the nervous activity in the two cases." He further limits the range of his inquiry as follows: "It was, of course, obvious from the start that there was no immediate possibility of attacking the complex activity of the cortex from its physico-chemical aspect. . . . We have now come to see that we do not possess even yet a full descriptive knowledge of the various aspects of this activity. Our chief task in studying the cortical activity at the present time must therefore consist in reducing the tremendous mass of various separate observations to terms of a progressively diminishing number of general and more fundamental units" (III, p. 386).

His unit is, of course, the reflex. Concerning the use of this concept to explain behavior he says, "In the eighteenth, nineteenth, and twentieth centuries the conception of the reflex was used to the full by physiologists. Working at first only on the lower parts of the central nervous system, they came gradually to study more highly developed parts, until quite recently Magnus, continuing the classical investigations of Sherrington upon the spinal reflexes has succeeded in demonstrating the reflex nature of all the elementary motor activities of the animal organism. Descartes' conception of the reflex was constantly and fruitfully applied in these studies, but its application has stopped short of the cerebral cortex" (III, p. 4). It is precisely in the application of this concept to the activity of the cortex that these three volumes are concerned. The whole nomenclature follows that of the physiology of the spinal cord so that we find chapters on "Conditioned Reflexes" devoted to irradiation, inhibition, induction, etc.

Little need be said of Pavlov's view of the relation between the physiology of the nervous system and psychology. His attitude does not result from a philosophical bias but from purely practical (laboratory) considerations. He questions the validity of explanations of animal behavior which follow the application of concepts proper to the analysis of man's psychic life. Chapter IX on "Die Höchste Nerventätigkeit" is mainly devoted to this subject. A single quotation will suffice. "The comparison of results which have been obtained by the objective analysis of the most complex nervous phenomena with the results of subjective investigation is beset with unusual difficulties. . . . All our deliberations and our entire thinking are related to facts which have been obtained in a strictly objective manner and possess, therefore, a quite special character. We represent indeed in our case, all our facts in space and time. . . . That is what we call complete natural scientific fact. On the contrary, the facts of psychology are conceived only in the dimension of time. And it is quite understandable that such a difference in the manner of thought must give rise to a certain incommensurability between these two kinds of thought. . . . Psychological science and psychological investigation are indeed in a very difficult position. They deal with an unusually complex material, and besides that the psychical phenomena are accompanied by the most difficult conditions, which do not obtain in physiology and from which we do not have to suffer. One of the very unfavorable conditions of psychological investigation is the fact that the investigation is not concerned with a continuous, unbroken series of phenomena. The psychologist deals with conscious phenomena but we know very well what a variegated fabric psychic life weaves out of the conscious and the unconscious. It seems to me that the psychologist in his investigation is in the position of a man who wanders in the dark having but a tiny lantern in his hand which can illuminate a very short piece of the road. The psychologist would admit that it is difficult to learn the whole region with the aid of such a lantern. Everyone who has been in a like position is aware of the fact that the impression gained of an unknown region with the aid of such a lantern does not correspond at all to the impression secured in bright sunlight. In this respect we physiologists are in a more advantageous position. If you will take all this into consideration, then you will understand the divergence between the chances of objective investigation and those of psychological research" (II, p. 75). The attitude of the physiologist interested in problems of behavior toward the science of psychology is, according to Pavlov, essentially as follows, "It is logical that in its analysis of the various activities of living matter physiology should base itself on the more advanced and more exact sciences—physics and chemistry. But if we attempt an approach from this science of psychology to the problem confronting us we shall be building our superstructure on a science which has no claim to exactness as compared even with physiology" (III, p. 3).

The concept of the conditioned reflex, originating from an experimental difficulty has never become other than a useful tool of investigation for Pavlov. He has explicitly refrained from elaborating the philosophical implications of his investigations of the higher nervous activity in contrast to Bekhterev who originated a reflexology and to Watson who urges the necessity of a new philosophy-behaviorism. "It is obvious that these special peculiarities of the research are in many instances;" Pavlov says, "the cause of fallacies, especially since it is so tempting to adhere to different fancied analogies and plausible generalizations—a tendency which cannot be too much guarded against in the present state of the research."

Pavlov regards the work of the behaviorists as essentially physiological and hence as identical in aim with his own investigations. Two comments must, however, be made. In the first place, the behaviorists have formulated a philosophical system—behaviorism. Watson says, "If psychology can do without the terms 'mind' and 'consciousness'—indeed, if it can find no objective evidence for their existence—what is going to become of philosophy and the so-called social sciences which today are built around the concept of mind and consciousness? . . . While behaviorism was fighting for its existence it was afraid to answer this question. . . . Hence the behaviorist's one answer when approached in this way was to say, 'I can't let myself worry about such questions now. Behaviorism is at present a satisfactory way of going at the solution of psychological problems—it is really a methodological approach to psychological problems.' . . . Today the behaviorist can safely throw out a real challenge to the subjective psychologists—'Show us that you have a possible method, indeed that you have even a legitimate subject matter.'"

In the second place, the origin of behaviorism from psychology is apparent both in its terminology and in its methods. For example, Watson discards the term 'memory' but retains 'habit.' The behaviorists' animals solve problems and the principal problem is still the maze or labyrinth. The conditioned reflex is to the behaviorist a verbal weapon for polemical writing while the actual prosecution of research by *the conditioned reflex method* languishes.

Cornell University H. S. LIDDELL

Zur Psychologie der Philosophie und der Philosophen. By ALEXANDER HERZBERG. Leipzig, Felix Meiner, 1926. Pp. vi, 247.

Herzberg attempts in this book to answer the question: What mental conditions produce the great thinkers? His thesis is that the great philosophers have turned to the realms of thought because they have been hampered in the practical affairs of life. He endeavors to establish this theory by the empirical method, that is by referring to the biographies of 30 great philosophers.

Before considering his thesis, Herzberg gives in Chaps. I-VI his idea of philosophy. We are told that philosophy was at first undifferentiated religion, poetry, and science, and that in it lay the foundations of all the sciences. From this beginning philosophy developed in two directions: towards theory and towards practice. Theoretical philosophy is the science of the universe; its task is to give a complete portrayal of the world (p. 17). It is characterized by the fact that it is autotelic. The action of the theoretical philosopher is therefore hindered (39). Practical philosophy is the science of life; its task is the establishment, propagation, and exemplification of life's ideals (17). The ideals of various practical philosophers have a *quietistisches Motiv*, so the practical philosophers are also men in whom action is hindered.

In the second and main part of the book, Chaps. VII-XXVII, Herzberg turns to his thesis. He examines the lives of Socrates, Plato, Aristotle, Epicurus, St. Augustine, Bruno, Bacon, Hobbes, Descartes, Locke, Spinoza, Malebranche, Leibnitz, Berkeley, Hume, Rousseau, Kant, Fichte, Hegel, Schelling, Herbart, Schopenhauer, Comte, Fechner, Feuerbach, Mill, Stirner, Spencer, Hartmann, and Nietzsche, and seeks to find the anomalies of mental life, the inactivity and inhibition, that he anticipated to be those of the philosopher.

Herzberg limited his study of these men to their practical affairs. He concludes from his examination that they as a group were (1) *berufsscheu* or *berufsungeschickt*, *i.e.* they began their professions late in life, or had interruptions or disappointments in their careers; (2) unwilling or incapable of earning a livelihood; (3) *ehescheu* or *eheuntauglich;* (4) *umgangsscheu* or *umgangsuntauglich;* and (5) *politisch scheu* or *politisch untauglich*. These men as a whole were incapable of succeeding in the affairs of practical life. Not one of them succeeded in four, and no less than twelve were unsuccessful in all five.

The problem is, why do philosophers fail in the practical affairs of life. Before answering, the conditions of normal practical activity must be determined. These are, Herzberg thinks, (1) the existence of practical interests; and (2) a certain intensity and harmony of these interests. An examination of the lives of these 30 philosophers reveals that they had practical interests—in some cases the practical interests amounted almost to a passion. The practical incapacities of these men must, therefore, be due to one of two things. Either to the fact that the philosophical interests were of greater intensity than the practical, or to the fact that the thinker has certain inhibitions which keep him from succeeding in practical life and turn him to philosophizing—the *Hemmungshypothese*. There is evidence that the philosophers had strong inhibitions, or were supersensitive, or had weak constitutions. Depression was the retarding factor in the lives of many of them.

Art and religion are also outlets for a person whose practical activity is checked. The reason that a man turns to philosophy rather than to art or religion is that he has a critical intelligence and that he is systematic in his thinking.

The neurotic also suffers from inhibitions. The philosopher, the artist and the religious man are distinguished from the neurotic by their high development and by their capacity for sublimation.

In the third and concluding part of the book, Chaps. XXVII–XXIX, Herzberg sums up the points that have been made, and emphasizes the fact that the man who is checked in his practical affairs needs an outlet for his energy. Philosophy has furnished such an outlet in the cases of the men under consideration. Herzberg discusses in these chapters the kind of questions the philosopher considers. He also emphasizes the biological value of philosophy in that it enables the failure in practical life to create a beautiful, friendly world that is easy to dominate—a world that is, therefore, quite unlike the real world.

Although the book is entertaining, any reader of it must be convinced that, had the author been duly cautious in his statements, his book would have been a more valuable contribution to thought on this subject. He apparently disapproves of cautiousness of statement, however, for he criticizes Locke for his tendency to *nachdenken* and *reflektieren*. He attributes Locke's failure in practical life to pondering too much over practical matters. I shall give some examples of statements made by Herzberg which are quite false, or are without bearing on his thesis.

By attempting to adopt the empirical method, Herzberg hopes to render his conclusions plausible but he does not succeed in clearing his mind of an *idol* of the tribe enumerated by Bacon. That is, apparently his theory was already formed before he started writing his book and the only statements that attracted his attention were those which supported that theory. Thus, with regard to *Beruf*, which he defines as "eine dauernde Tätigkeit eines Menschen zu Erwerbszwecken (50)." Herzberg states that Plato and Aristotle each began his *Beruf* late in life. One established his school when he was forty, and the other when he was fifty. Yet everyone knows that these men received no pay for their teaching. Consequently they had no

Beruf, if we use that term with the connotation that Herzberg gives it. Contrary to Herzberg's idea, these men prove nothing for his theory.

The norm by which he judges the relation of the philosophers to money is this: in the case of the normal man "auf Gelderwerb ist ein grosser, ja bei den meisten wohl der grösste Teil ihres Denkens und Wollens gerichtet. Geld ist für ihn . . . eine der Haupt- und Staatsangelegenheiten des Lebens" (65). Because he is unable to discover that these philosophers under consideration were thus obsessed, he regards them as abnormal. It is quite apart from my purpose in this connection to show that they were anything but abnormal if the above statement is a criterion of normalcy. At the same time it is necessary to point out some erroneous statements in this chapter on *Geld*. Herzberg says that Spinoza could not make his living at grinding lenses. Consequently he received help from his friend De Vries and was made the sole heir of that friend (66). We have the statement of no less an authority than Frederick Pollock that Spinoza declined the gift of 2000 Gulden and that he dissuaded his frail friend from making Spinoza the sole heir. The income which Spinoza received from grinding lenses was sufficient for his slender wants. Hume is referred to as a *Millionär*, but the fact is that he never was wealthy. On account of his frugality he was able to leave an estate worth something more than £6000.

In connection with Herzberg's statement that most normal men marry but that only fifty percent of the thirty philosophers married, we would like to point out that such men as Plato and Spinoza were too much married to philosophy to care for an alliance that would subtract from their time for contemplation. There is no evidence that they turned to philosophy because they were disappointed in love. Futhermore, sixty percent of the philosophers who did marry seem to have lived happily ever after, according to the data that Herzberg has collected. If there is any truth in this data, the philosophers are to be commended for carrying out their marriage vows better than do many of Herzberg's normal men.

In order to show that the philosophers were *umgangsscheu oder umgangsuntauglich*, Herzberg writes, among other statements, with regard to Socrates: "aber er macht sich diese Menschen zu Feinden, indem er ihnen klarzumachen sucht, sie bildeten sich zwar ein, weise zu sein seien es aber nicht" (85). This, however, does not prove that Socrates should have tried to gain the esteem of these particular men. He had a great many more worthy friends. Plato is accused of having aroused the ire of the Syracusan 'tyrants,' but I know of no reason why he should have tried to become an intimate friend of theirs. Plato had many friends. These statements of Herzberg establish nothing with regard to the *umgangsscheu oder umgangsuntauglich* characteristic of these men.

Because most of the thirty philosophers were not great statesmen, Herzberg regards as untenable the Platonic idea that philosophers should be kings. He is unmindful of the fact that some of the thirty men whose lives he is examining would not have been regarded by Plato as philosophers. He might easily have chosen other great philosophers than such men as Rousseau or Nietzsche or Bruno—others who were more deserving of the designation *philosopher*. There have been great philosopher kings and statesmen. Herzberg here as elsewhere commits the fallacy of hasty generalization.

We could easily point out other fallacious statements and other 'snap judgments' by Herzberg which tend to vitiate the value of Herzberg's conclusions. Thus we might show that he has no adequate understanding of Neoplatonism. But enough has been said to throw a serious doubt over the adequacy of his attempt to establish his thesis that great philosophers have turned to philosophy because they were unsuccessful in practical life.

One might question Herzberg's idea of what is involved in being a practical man and offer the contrary thesis that the philosopher is the most practical of men. The real philosopher puts first things first. He is, in Plato's magnificent phrase, the "spectator of all time and all existence." Herzberg's practical man is a mere *a priori*-ist, out of touch with reality. He lives quite an abstract life remote from the truth and thus becomes the prey, in certain matters, of pseudo-scientists and healers who know little of medicine or philosophy. He lives, as Stevenson says, by catchwords. Sometimes he tries to get away from the narrowness of his existence by attending the theater. If he is fortunate, the drama will have been modified in accordance with the theory of Aristotle. Too often the spectacular or tawdry will hold his attention. In short, Herzberg's practical man is the one who lives for much of the time in a world removed from reality. It is the philosopher, the real philosopher, who is practical. In his search for the truth he is made free.

Randolph-Macon Woman's College MARJORIE S. HARRIS

The Abilities of Man. By C. SPEARMAN. New York, The MacMillan Co., 1927. Pp. vi, 415, xxxiii.

If one has followed the earlier writings of Professor Spearman, and especially the researches of his students, one will be immediately at home in this volume. The book is arranged in two parts, with fortunately no break in the chapter enumeration. The first part contains a presentation of the rival doctrines of the abilities of man leading up to the author's well-known conception of two factors, theories of the nature of *g*, and a notably clear presentation of the tetrad difference equation with examples of its use. The second part is devoted to a survey and summary of the literature, first for experimental support of the *g* factor and then for indications of other independent factors. This is followed by a long appendix of great value because in it appear many formulae, derivations of formulae, and examples of their application.

The literature on intelligence is scanned for basic conceptions of its nature and of the organization of human abilities. The different points of view are described as "monarchic," "oligarchic," and "anarchic." Spearman's own doctrine of the "two factors" is presented as the "eclectic doctrine." As most readers of this JOURNAL doubtless know already, one of these factors, designated as *g* or the "general factor," is thought of as varying from individual to individual but as remaining "the same for any one individual in respect of all the correlated abilities." But the presence of the two factors is now discovered and demonstrated by means of the criterion of "tetrad differences." A tetrad difference is found by first selecting in any table of correlation coefficients four coefficients the positions of which in the table would mark the corners of a rectangle. The difference between the products of the coefficients which lie diagonally opposite each other in this assumed rectangle constitute the tetrad difference, and in terms of the formula is equal to zero. Whenever this tetrad equation holds throughout a table of correlations, that is when each difference closely approximates zero, then every individual measurement of every ability can be divided into *g* and a second part called *s* which is the "specific factor." This *s* factor not only varies from individual to individual but may vary in any individual from one ability to another. The effect then of pooling a number of different tests is not to determine an average but to permit the specific factors to neutralize each other so that the result is a measure approximately of *g* alone. After reviewing a number of possible interpretations of *g* the author states his preference for an hypothesis of "mental energy" and of "engines." The mental energy can be used to explain *g* while the notion of "engines," energized or not, as the case may be, by this mental energy, explains the specific factors or *s*.

Obtained tetrad differences must inevitably be subject to the errors of sampling which affect the correlation coefficients upon which the tetrad differences are based. Thus they can be at best but approximate zero. Consequently the probable error of the tetrad difference is indispensable. For this Professor Spearman presents an original formula which is certain to interest every reader.

In order to prove the existence of *g* and *s*, a number of studies are examined for their tetrad differences. By graphic representation the close correspondence of the distribution of tetrad differences to the theoretical distribution, when the tests are measures of ability, is made strikingly clear. Equally impressive is the very opposite result, determined when the measures are of anatomical features. Where there are two varieties of the same test there is the same lack of correspondence between the observed distribution of tetrad differences and the theoretical distribution, indicating, by the author's interpretation, an over-lap in the specific factor.

Two long chapters are devoted to the examination of many investigations to determine if *g* is universal and to what extent *g* is a factor in different mental functions. The upshot of these is a contention for the universality of *g* and the conclusion that the amount of *g* in different functions may vary greatly, in some (sensory discrimination, for example) it is very small. Then the reader is led to the consideration of the special abilities for the discovery of which the tetrad equation is again used. But now it is the presence of tetrad differences in significant amount that is the criterion. Again a large number of studies are examined but this time with meager results. The only special abilities found are in reasoning and generalization, mechanical aptitude, arithmetical aptitude, and in judgments of social conduct.

While general ability *g* is interpreted as universal, as pervading all manifestations of ability, the author presents evidence for the recognition of yet two other universal cognitive factors which can be regarded as aspects of that same mental energy denominated *g*. These other two aspects are its inertia and its oscillation. Inertia is the familar perseveration. Correlation coefficients for tests of perseveration are so presented as to reveal one of the alluring features of the author's method. The criterion of tetrad differences is applied and a value approximating closely that of the probable error is obtained. Thus the correlations presented in the tables must be derived from one factor. This factor cannot be *g* because it had already been determined that the tests used gave an insignificant correlation with *g*. Garnett's establishment of *c* (cleverness) is reviewed and accepted as being the obverse aspect of perseveration. By oscillation is designated the familiar fluctuations in retinal rivalry, ambiguous perspective, and the fluctuations in tests of mental output. Again a table of correlations is presented which reveals low tetrad differences but when oscillation is referred to measures for *g* the tetrad difference jumps to nine times the probable error. The correlation between measures of oscillation and of *g* is very small. The correlation also between oscillation and perseveration is low but the tetrad difference when correlations for oscillation and referred to correlations for perseveration appears to be about eight times the probable error. This oscillation is conceived as an indication of a "certain facility of recuperation after effortful expenditure."

A chapter on the law of conation and the use of it in the summary will cause most readers to pause. A fourth universal factor is accepted. This is Webb's *w* or "consistency of action." In addition to the mental energy, the quantity of which is indicated by *g* there are those engines the operation of which reveals perseveration and oscillation; and "where energy and engines operate, there must further exist an engineer." This engineer is the *w* or conative factor.

To attempt the valuation of such a book as this seems to the reviewer decidedly premature. It is the kind of work that only time and much research can properly estimate. As a compilation of studies and programs of research it is a most serviceable contribution. Few psychologists can read it without being stimulated to rearrange or add to their present research plans so as to be able to attack some of the many alluring problems raised. But it is only after the reader has mastered the technicalities of the book and has put aside the fun of figuring tetrad differences and their probable errors and of day-dreaming about possible researches that the full significance of it all dawns upon him. Valuable as these technicalities and possibilities may be they are after all but tools and means. The greater value lies rather in the system of psychology presented. Here is a system, or at least a system in the bud, which is the direct outgrowth of research by the test type of research technique. Whether one be convinced of the system or not, one must recognize that a master mind has looked through the welter of tables and correlations and has seen far, far more than a test for this and a test for that. Few books in a year or in many years manifest the scholarship, the creative planning, the insight and the vision revealed here. It will long stand as an indispensable aid to the teacher, the experimental researcher, and to the systematizer in psychology.

Finally it must be noted, and with regret, that the book is marred by mechanical defects. Of typographical errors there are many, some of them apparently in the presentation of formulae and their applications. Although the index is long and apparently complete, working with the volume reveals in it many annoying omissions.

University of Oregon EDMUND S. CONKLIN

Practical Psychology: Human Nature in Everyday Life. By EDWARD STEVENS ROBINSON. New York, The MacMillan Co., 1927. Pp. xii, 479.

Robinson's book is written as a text for a short course taken by students, the majority of whom are seeking, first, a general but not too technical knowledge of the subject, and second, a grasp of its relations to personal and social life. In its selection and organization of material it follows rather closely the conventional plan beginning with the nervous system, then carrying the discussion through habit, perception, the higher thought processes, and feeling to personality and the measurement of individual differences.

The book makes very easy reading. Its style is simple, pleasing, and of even tenor throughout. Numerous examples from daily life are given by way of illustrating principles; paragraph headings in boldface, carefully worded, add much to the ease with which the content of the book can be grasped. Every chapter ends with an organized summary and a list of problems, the latter are cleverly selected and cannot help but stimulate the student and to lead him to think much about the subject matter of the chapter. Again, at the head of each chapter are presented two or three pertinent questions for the students to keep in mind while studying. All in all, the book is excellently written from a pedagogical standpoint.

None of the artificial and confusing problems so often thrust upon the beginning student by partisan instructors is found in this book. While the text does not aim to be systematic, it is not without order and logical sequence. Attention and sense qualities are treated under perception; memory, thought, and association are discussed under ideation (very little space is devoted to sensory processes). One reads nothing of the old duels between introspectionists and behaviorists, or the old problems of sensory attributes, meaning, the ultimate nature of feeling, and the like. While the terminology used in the book suggests a dualistic position and an instrumentalism, these points of view are not thrust upon the reader. Perceptions, ideas, and reasoning are said to prepare the individual for action either immediate or remote. Not least among the book's good points is a recognition of social conditions of behavior.

Practical Psychology is consequently a text almost, if not actually, suitable for use in high schools and certainly for Normal School use. Frankly, the reviewer feels that a text of this sort, admirably as it fulfills the purpose for which it is written, is not the ideal for a beginning College sophomore course. In spite of the fact that, as Robinson says, our beginning classes in psychology are made up largely of students who will go no further in their psychological studies, we need not to lower our teaching standards but to make our courses even heavier. With schools of education, pre-medicine, business, and journalism requiring psychology, the bulk of many of our beginning classes consists of students who ostensibly are going to make use of psychological facts in their professions. The best we can give them is none too good. Moreover, a beginning course which deliberately aims to be practical by being at the same time superficial cannot help but give the beginning student the wrong impression of psychology as a science. It will not, of course, prepare students adequately for advanced work in psychology and will necessitate a considerable amount of unlearning when they do take advanced work.

It is almost inevitable that an attempt to simplify the statement of psychological fact too much is achieved at the expense of accuracy. Robinson's book is not an exception. It is not strictly up-to-date in its selection of the few principles which it uses. The newer principles in biology, physiology, psychology, and the social sciences, all of which dovetail together and necessitate a radical re-presentation of practically all psychological facts receive no definite recognition. We refer to the organismic principle in biology and physiology, the practical abandonment of the concept of reflex action, the corresponding development of configurationism in psychology, and again a corresponding organismic view in the social sciences which latter views dispense with conventional explanations of learning, conventional views of habit and the like. Psychologists seem to be slow in recognizing progress not only in their own science but in borderline sciences. The present-day emphasis in all science is to begin with the complex and work toward the simple not only in methodology but in the presentation of subject matter. Robinson begins in the conventional way with the simple and leads to the complex, the exact reverse of our latest scientific developments.

University of Kansas — RAYMOND H. WHEELER

The Science of Mental Activity. By F. H. LUND. New York, A. G. Geiler, 1927. Pp. xx, 488.

Lund's book is an elementary text which well merits on the point of inclusiveness Hollingworth's statement (made in an introduction that he wrote for Lund) that it escapes the modern 'flux' in psychology. It aims at the preservation of the rich fruits of psychology since Aristotle. What an improvement this point of view is over that of the radical behaviorist who takes pride in disregarding everything in psychology older than ten years.

After an introductory chapter there are two chapters on the nervous system and its functions, then a chapter on emotion, another on feeling, and another on voluntary activity. Then follow three chapters on the sense organs and sensory phenomena. Attention, learning, habit, memory, intelligence, association, perception, imagery, imagination and dreams, language and meaning, thinking, belief and confidence, personality, are treated successively with a chapter devoted to each. Continuity is maintained between chapters by devoting the initial paragraphs of each chapter to orientation. There are sources and exercises for the various chapters. The illustrations throughout the text are unusually good; although, it appears that some taken from Lickley are not acknowledged.

This brings us to two major defects of this text. First, there is an apparent confusion in the author's mind (certainly the statements made are confused) on all topics relating to the enigma of body-mind relation. Not that we expect a correct solution, but a theory regarding it should not be flagrantly self-contradictory, and dogmatic. Thus it is roundly asserted (p. 7f.) that the physical world like the mental world exhibits the phenomena of memory, and that mind as an immaterial entity cannot be conceived. The widespread notion, says the author, that the mind is immaterial is due to the fact that mental processes occur without observable physical changes. Mental and physical phenomena differ in degree only (8). Mental activity is more 'private' (since the brain is concealed), less predictable, and less uniform. Here is a strange *impasse!* In order to make a difference between the mental and the physical and yet have only the physical (an impossible feat in any case), Lund sets the brain aside as a unique body. This is not to get rid of animism, but to extend it to the physicist's territory! The author sometimes speaks of consciousness and brain action as identical, then in the next sentence as separate. If mind and body are distinct, asks Lund, "how are we to conceive of their functional relationship?" (8). The student may well wonder how there could be a 'relationship' of any sort where there are not at least two things to be related. The statement, "consciousness and brain action are one" is followed immediately by the assertion that "a change in the one necessitates a change in the others." There can be no question of causal relationship between mind and body, because their changes are simultaneous. Now, this is precisely the instance, says Russell, in which causal connection is the most certain, because the lack of time interval between cause and effect prevents interruption of the causal *nexus* by outside forces.

This basic confusion of the author reappears throughout the text. Thus, on page 120 the student is told that purposive activity is unique only in the respect that end results cannot be immediately attained. If so, then the travels of Halley's comet are far more purposive than even the preparation of a text in psychology. In a bit of moralizing (126) the student is warned that the only safeguard against assaults of momentary impulse is to increase self-control. This is admirable, except that the very next page explains that the sense of efficacious effort and freedom are illusions.

Another type of defect in the book are misstatements of fact, and neglect or misrepresentation of other psychological sources. Thus, it is roundly declared (15) that insanity, delusions, hallucinations and multiple personality are the results of diseased or injured nervous systems. This rejection of mental diathesis, both Freudian and non-Freudian, is unwarranted. He states (17) that our present knowledge of learning and memory are in a large measure attributable to the animal psychologists! It is also stated that the nervous system contains about 12 billion cells: the true estimates range around 25 billion. In discussing the James theory of emotion, Lund seems to be unaware of the fact that James himself cites the statements (*Principles*, II, 464) from actors which prove that the voluntary mimicry of the emotions does tend to give rise to them. The author thus deduces what James "must have meant," when the pages of the *Principles* leave no doubt on the matter.

The chapter on Attention neglects the signal researches of the last decade, especially those by Dallenbach. The chapter on Memory is spoiled by a confusion of three notions of recognition: (a) recognition as the time and space localization of an item of memory, (b) as a feeling of familiarity and (c) as the function of identifying furnished facts, as opposed to *recall* where memory itself does the 'furnishing.' On p. 266 the author commits himself to definition (a) which does not square with the contention on page 268 that the range of recognition memory is greater than that of recall memory. Surely we cannot have more time and space associations for memory items than we have memory items themselves. On page 269 he explicitly commits himself to definition (c). In the chapter on Belief and Confidence, feeling of familiarity is confused with *confidence* in the cognitive sense. The degree of one's confidence, he says, increases with the ease with which the items (which challenge confidence) lead to familiar responses. If this were true, we could have no confidence that we are meeting with a highly novel experience, since the greater the novelty the greater the difficulty of 'redintegration.'

Wells College C. O. WEBER

Factors of Human Psychology. By L. W. COLE. Boulder, University of Colorado, 1926. Pp. 350.

Emerging as a text that has had some seasoning in mimeographed form this volume aims "to lay a foundation for business, industrial, and educational psychology." The author promises to avoid confusion between the facts of pure psychology on the one hand, and their applications on the other.

His introductory statement should be exemplary to the writers of modern textbooks: "It seems to the writer as undesirable to mingle a great deal of applied psychology with the introductory principles as it would be to discuss indiscriminately anatomy, physiology, and hygiene in the first year of medicine." It is disappointing, therefore, to note that the book proceeds unevenly with respect to the facts of modern psychology. There are two matters that are open to criticism: (1) the rather archaic interpretation of the introspective method, discussed in places to the point of confusion; and (2) the historical treatment accorded to certain topics in psychology, in contrast to the more modern treatment of others. Since the book advances no new systematic presentation of the subject-matter of psychology and since the author draws very heavily on secondary sources, the least that could be expected is an eclectic summary of the most recent concepts and data in each division of the field. The author appears to be most expert with his references in the chapter on the nervous system, in the discussion of instinct, and in the chapter on spatial perception; but the last chapter of the book, with the possible exception of the one on intelligence, leans heavily on material that is traditional without much attention to the more recent authoritative papers in the field.

One of the chief faults in the book is the exposition of the introspective method. In spite of all that Wundt has done to caution workers in experimental psychology against that form of observations which lacks objective control, the author repeatedly refers to introspection as a method which is charged with personal evaluation. He is right in starting with an emphasis on the observation of one's own mind, but he confuses the issue by making introspection equivalent to the sort of interpretive self-analysis that is epitomized by the phrase "What is the matter with me?" (24), or contained in the phrase "The criminal condemned to the gallows is often quite sure he has introspectively analyzed out the exact psychic causes of his downfall" (25). Unless one is indeed systematically convinced of a "self" psychology it is scientifically inaccurate to describe introspection as self-observation especially when it is "carried on under . . . ordinary . . . conditions" (22). On the other hand, without further exposition it is harmful to summarize one phase of psychological procedure by saying that "in the realm of sensation psychology has used successfully the methods of the physical sciences" (18) and then to go on to say that the methods change when the higher mental processes are investigated. It is doubtful, moreover, whether many psychologists, either in the pure or the applied field, will generally confuse intelligence with the thought processes as the author has repeatedly done.

The author tries his best to make peace between the two outstanding schools by defining psychology as *"the science of consciousness and behavior and their inter-relations"* (28).

After these introductory matters are dealt with we find about the usual proportion of the textbook devoted to sensation, but here again the author uses his concepts loosely. In the preceding chapter consciousness is confused with the control of the situation through ideas (45). Now, we find that every sense organ is represented by an area in the brain (53) and the skin is classified as a sense organ. In spite of much of the work that was done during the War on the semicircular canals, the writer speaks both of the "sensation of dizziness" (15, 112) and of the adjustments made from the semi-circular canals as reflex adjustments, adhering quite closely, therefore, to the debated Barany interpretation. The work during the War also brought out experimental investigations of sound conduction other than by means of "vibrations of air" (115, 117) since sounds were conducted through liquids and through the ground as well.

The Western Electric as well as the Seashore instrument for testing pitch is called an *audiometer*. The upper limit of pitch is given as being in the neighborhood of 30,000–40,000 d.v. The objection which the author voices aginst the color pyramid involves a complete misunderstanding.

The use of the text will be quite limited. It presents no new point of view nor a radically new treatment of the various sections of psychology. There are a few new illustrations but for the most part these are all taken from standard sources. Here and there new data are introduced and fundamentally the author is correct in his insistence on the ultimate basis of experience as the ground work of psychology. But why so many illogical counterblasts as, for example, the statement that "information of great value and certainty may be gained by the study of the structure or functions of the brain and nervous system" (22). Are these facts of value in physiology and neurology or are they psychological facts? Would the physicist say that information concerning the valence of the chemical elements would be of great value to physics? Of course, the point is that the physiological and anatomical substrate of mind yields many facts for the interpretation and explanation of mind but surely not facts as a basis for mental description.

University of Iowa CHRISTIAN A. RUCKMICK

Principles of Abnormal Psychology. By EDMUND S. CONKLIN. New York, Henry Holt & Co., 1927. Pp. vii, 457.

The scope of abnormal psychology has never been well defined. The abnormalities likely to receive systematic treatment are mostly on the minus side of the norm; functions more or less disturbing to harmony with the environment or to progress. It also includes functions quite normal, but not stressed in normal psychology, such as dreams. It is a much more vague term than pathological psychology or psychopathology, with their definite connotations of disordered function.

As Professor Conklin envisages the field, the focus is on disordered function more than on disordered personalities, taking up in order, disturbances manifested at a sensory level, the amnesias, delusion formation, the psychoses (essentially under the classifications of dementia praecox, the manic-depressive conditions, paresis, epilepsy), the psychoneuroses; then hysteria and the war neuroses, multiple personality. Two chapters are rather specially concerned with psychoanalytic procedures and claims. There are good critical accounts of hypnotic and spiritistic phenomena. Here follow a chapter on dreams and one on the effects of drugs, the latter relatively strong on the documentary side. It seems a pity in such a work to have made a single and necessarily sketchy chapter of feeblemindedness and genius. A concluding essay at a historical background deals with personalities who have influenced psychopathological thought; it would have been much stronger if the theme had been the history of cultural attitudes towards the phenomena. The style is straightforward and objective; there is no appeal to literary effects, nor, it may be added, to the various types of (journalistic) sensationalism to which the topic lends itself. In this last respect indeed, Professor Conklin has all but leaned over backwards.

The appeal is to an undergraduate course in the subject, and one taken from the standpoint of general culture rather than for introduction to more advanced work. Much more effort has been put into the task than appears on the surface, or may be appreciated by one not himself familiar with the field. The documentation could, and perhaps should, have been made much more detailed; this is one of the factors that limits the volume as an 'introduction.' Also there are speech forms not in good technical use, such as melancholiac, and paranoiac. Nietzsche's psychosis was hardly relevant to the question of genius and mental disease. The reviewer agrees heartily as to the use of the term 'extraversion,' but less well as to the rationalizing (p. 740). In a volume from the University of Oregon of all places, there is singular lack of reference to synaesthesis—the very word is not in the index.

For better or worse, the book does not far transcend the level of a compilation. The accuracy of reporting that one expects of a good compiler appears to be present, and must not be undervalued. At the same time, the advanced student must check these things; though the account be meticulously accurate one could hardly cite a work of this character as authority for, say, Clark Hull's studies of smoking. The regret must be repeated therefore, that reference to sources was (intentionally) not made easier. There is further evidence of indirect contact with the large portion of the field concerned with psychopathology. In no other way is it possible to account for the relative status in the book of Kraepelin and Bleuler.

With the undergraduate orientation of the volume, it may be pertinent to raise the underlying question of the topic's adjustment to undergraduate teaching. The natural attractions of the topic at this stage lie partly in the salve that the contemplation of others' weaknesses affords to one's own status-complex, and partly in thrills anticipated from more lurid aspects of psychiatric experience ("Now doctor, we want you to show us some of the very *worst* cases you've got" . . .). It is overloaded with nomenclature, and prone to substitute verbalism for experiment. Abnormal, *resp.* pathological psychology is not without content of direct undergraduate value, but it seems to fit better into the setting of mental hygiene, or the general introductory course, than as an independent discipline. The latter belongs to a more specialized level. Its content is assembled mainly from psychiatry, anthropology and literature. Need for a standard synthesis is not externally pressing but awaits, like other achievements, a genius adapted to it.

Boston Psychopathic Hospital F. L. WELLS

Interpretation of Educational Measurements. By T. L. KELLEY. Yonkers, World Book Co., 1927. Pp. iv, 355.

The justification for this volume lies in its last two chapters. In the first of these, the author, with the assistance of five other judges known in the field of measurements, has attempted to evaluate the numerous tests of intelligence and school achievement. In the last chapter, Kelley presents classified and graded lists of tests with reliability coefficients, population used in determining tests standards, measures of deviation, etc., as well as data on publisher, year, cost, and other facts of interest to the prospective user of the tests. In order properly to appreciate the statistical information here given, it must be assumed that the reader had perused the preceding chapters or their equivalent.

But in general it can hardly be said that this volume is suitable as a text for those wishing to study the problems of measurements. The historical survey (Chap. I) is inadequate for the beginner and adds little to the equipment of others who are not beginners. The second chapter touches in a very cursory manner on a number of theoretical questions, such as diagnostic value, endowment versus training, reliability, validity, and others. The new student is unprepared—in the psychology of tests or in statistics—to profit by the discussion. Still, the experienced student will probably find there a few challenging statements. The following chapters on the measurement of group and individual achievement, determination of individual idiosyncracies, and studies of certain inequalities of development are distinguished from the many others that have been written on these topics by Kelley's emphasis on the probable error of observed data, a point that might well be brought forcefully to the attention of users of tests. The topics considered in the chapter headed Elementary Statistical Procedure can better be studied in one of the recent texts devoted to the statistics of psychology and education.

In the eighth chapter Kelley sets out to prove that tests of intelligence and tests of achievement are measures of the same thing. His thesis is that because the two types of tests show a high positive correlation, we are therefore measuring not two functions but one. If he means that for practical purposes one may serve to predict performance in the other, then we have no quarrel with his view. But if we are to take him literally, then we must be ready to reject the concept that intelligence tests are significant in instances where specific school learning has played no part, or an unequal part in the performance of different individuals. But it is doubtful, whether many would go very far with Kelley in his belief, or that they would be justified in subscribing to his point of view. It would be necessary—if we were to agree with him—to conclude that because the number of household utensils correlates highly with the equipment in the living room the two are the same thing. It is to be noted also that in this chapter the author, in establishing the applicability of his formulas, makes certain assumptions which, while not extreme, are still very broad.

However, the student of measurements of intelligence and achievement will find the volume thought provoking, for Kelley has challenged some of the beliefs in this branch of psychology.

Cornell University FRANK S. FREEMAN

NOTES AND DISCUSSIONS

Demonstrational Experiments in Memory

For a number of years, during the lectures on memory in introductory courses in psychology, I have been using crude class experiments, which have yielded such definite and convincing results that a note about them now seems desirable.

There are four materials and presentations: (I) Ten nonsense syllables, for 12 repetitions and immediate reproduction. (II) Ten 3-letter familiar words, for 12 repetitions and immediate reproduction. (III) Five nonsense syllables, for one repetition and immediate reproduction. (IV) Ten nonsense syllables, for 4 repetitions at each of 3 successive lectures, and immediate reproduction on the third day.

The syllables (or words) were lettered in ink on charts and exposed on a frame by raising a roll-shade. The class read the series aloud with the lecturer, in trochaic rhythm, at a rate which the lecturer supposed to be one syllable per sec. The method was that of retained members. For reproduction the class wrote down the syllables immediately upon the drawing of the shade, and then corrected their own results. The class was polled by show of hands and the average amount correctly retained computed on the blackboard.

Experiment:	Percent retained (av. for class)											Av.
	a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*h*	*k*	
(I) 10 syllables, 12 repetitions	72	67	68	64	63	58	65	72	63	62	65	65
(II) 10 words, 12 repetitions	98	99	92	93	92	92	94	92	94	93	94	94
(III) 5 syllables, 1 repetition	—	76	73	76	82	78	88	79	86	83	82	80
(IV) 10 syllables, 3x4 repetitions	80	74	—	78	79	73	73	83	83	80	79	78

The table gives, as *a* to *k*, the results of eleven experiments of this sort. I do not attempt to give details, for my point is that the desired differences come out in spite of the crudity of the method, the obvious sources of error, and the great variability of conditions. The numbers in the groups range from 26 (*h*) to 131 (*c*). The groups were of men and women (*a*, *b*, Cornell, 1916-17), of men (*c-g*, all but one Harvard, 1924 *et seq.*), and of women (*h-k*), Radcliffe, 1924 *et seq.*). The materials in *a* and *b* were different from the later series (*c-k*). The distribution in IV was with two-day intervals within the week in *a* to *e*, *h*, and *i*; but with a five-day and a two-day interval in the other cases. This difference, however, does not seem to affect the results.

The base for comparison is (I), which is the least number in every column. It is true that practice favors the other three tests, but I do not believe that it is primarily responsible for the differences. The demonstrations are:

Meaningful material. Compare (II) with (I). The striking difference (av. 94% and 65%) is not all of the experiment, for every student realizes from his own experience how much more he has in memory after-image in (II) than in (I).

Length of series. Compare (III) with (I). The difference is less (av. 80% and 65%), but, although it is convenient in lecturing to have this difference, a difference is not really required if one is making Ebbinghaus' point that the number of repetitions necessary to master a short material may be decreased much more than the series is shortened. Of course 65% in (I) means 6.5 syllables, whereas 80% in (III) means only 4 syllables, and the demonstration is less clear cut if one talks simply about numbers of 'bonds.' This experiment, like the last, has also its introspective side: the student realizes that he has the five syllables of III for a moment within the 'range of cognition,' and the lecturer can refer to the laws of attention here.

Distribution of repetitions. Compare (IV) with (I). This difference is the least (av. 78% and 65%), yet in all these cases the difference has come out in the direction which simplifies exposition. The lecturer has to point out that forgetting tells against (IV) only, and it is convenient for him to note that, in spite of the great effects of forgetting, there is nevertheless some improvement on account of distribution. Introspectively many students will observe the greater degree of attention with which they come back to series (IV) after an interval, as against attention when the repetitions follow each other all at once in (I). Only occasionally will a student admit having thought of the syllables in the interval between lectures.

The value of demonstrations of this sort does not depend upon the lecturer's belief in the correctness of the associative law of frequency. The facts shown in these laws, like many others, can be presented as showing that the associationistic law is incorrect. I have once tried presenting them as *Gestalten*!

Harvard University EDWIN G. BORING

THE THIRTY-SIXTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The thirty-sixth annual meeting of the American Psychological Association was held at Ohio State University, Columbus, Ohio, on Wednesday, Thursday, and Friday, December 28–30, 1927. There was a total registration of four hundred and eighty-nine, which was ninety more than last year. This is a record attendance, and with the rapid increase which is occurring in the membership, we may be sure that the record will not stand long. This rapid growth of the Association, which is due principally to the broadening of the admission requirements for associates, will undoubtedly cause many changes in the arrangements for future meetings, for it will soon be difficult to find a banquet room which will seat all the members in attendance at the meetings, or to find lecture rooms large enough to hold one-half of those in attendance.

The number of papers submitted has not increased in the same proportion as the membership. Nevertheless, the Program Committee must be finding its task beset with many difficulties, and it is safe to predict that there will be radical changes in the program in the near future. The geographical distribution represented by the members attending was even wider this year than last. There were thirty-five states and six foreign countries represented, as against thirty-three states and three foreign countries.

Parallel sessions will continue to be necessary, unless the Program Committee decides to reduce the number of papers by at least one-half, or to introduce round-table discussions where numerous short papers can be presented. There were nine formal sessions, and two sessions for graduate students. At the former sessions, members and associates presented sixty-four papers, and at the latter sessions the graduate students presented twenty-one papers. The parallel sessions were not staggered as they were in the previous year, which seemed a wise change, as little is gained and often confusion ensues from such an arrangement.

There were two round-table conferences on Experimental Psychology, and one on the Analysis of Mental Traits. As usual, these conferences proved very valuable. Attendance at them was large, so large in fact that discussion was stifled in the experimental conferences until toward the end of the second meeting.

On the average, seven papers were presented at each meeting. An examination of the titles of papers which were read by members and associates reveals that there were fourteen papers on experimental psychology, thirteen on clinical psychology and mental measurements, nine on vocational psychology, seven on general psychology, seven on educational psychology, seven on animal and comparative psychology, and six on social and abnormal psychology.

The abstracts of the papers have already appeared in the program and also in the Annual Report of the Secretary, which has been published in the *Psychological Bulletin*, so that it is unnecessary to describe them in this place. It might be said, however, that the range of topics seemed to be greater than at some of the previous meetings. For example, in the experimental session there were papers in the sensory field, and on perception, attention, memory, reaction, and learning. The sessions for graduate students were not always as well attended as they deserved. Some of the papers were very good, and frequently aroused lively debate.

At the business meeting, twenty-nine new members and eighty associates were admitted to the Association. It was voted to hold the thirty-seventh annual meeting at Columbia University on Thursday, Friday, and Saturday, December 27–29, 1928.

About two hundred and fifty persons attended the annual dinner, which was as pleasant as such dinners usually are. After the dinner, Professor H. L. Hollingworth delivered the presidential address entitled "Sensuous Determinants of Psychological Attitude." It was witty as well as scholarly, and was greatly appreciated by the large audience.

Princeton University H. S. LANGFELD

THE TWENTY-THIRD ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The twenty-third meeting of the Southern Society for Philosophy and Psychology was held at the Virginia Military Institute, Lexington, Virginia, on April 6–7. The morning program on Friday was preceded by a warm address of welcome by General William H. Cocke, Superintendent of the Institute. In the first paper on the program H. C. Sanborn (Vanderbilt University) discussed the neo-realistic conception of music. From the point of view of the neo-realistic school, which originated with Herbart, music consists merely of the formal patterns, or *tönend bewegte Formen*, which are non-expressive. On the other hand the emotion of the reproducing artist, and that of his auditors as well, is taken—in contrast with the attitude of the composer—to be real. This subjective element, however, is viewed by the school as irrelevant to the work of art as such. In criticism it was urged by Sanborn that this view fails to note that the esthetic attitude is everywhere concerned not with *real* emotion, but with emotion that is objectified, projected, or felt into imaginary forms which serve as the medium of communication between the artist and those with whom he communicates. The neo-realistic view considers the work of art in abstraction from subjects as though pure forms really exist, whereas only in connection with subjects does the concrete work of art come into being, according to the view taken in the paper. This does not imply necessarily that the work of art, and more especially music, is not the bearer of a universal content. This content is not, however, specific emotions, or the definite ideas by which emotion in general is rendered specific, but rather the general background of psychic life with its indeterminate striving, emotional moods, and obscure indefinite ideas. The especial esthetic value of music is the expression of these aspects of life. Charles A. S. Dwight (Keuka College), in a paper on interaction of philosophy and psychology, contended that either philosophy does leak into psychology (in which case such admixtures should be clearly labeled as opinion) or it does not (leaving such questions as the self, purpose, character, etc. to be settled elsewhere outside of the psychologists' little court). In either case there should be frankness of discussion. Many of the statements passing as scientific findings are but mental constructs or inferences from facts, which should be clearly understood to be only individual visualizations of the data. While it is desirable for convenience to keep the two disciplines apart, actually they parallel, and will reciprocally influence each other. He deplored the exclusion by most psychologists of the soul, which is a red rag to the 'behavioristic bull.' A spiritual system of philosophy has as much right to 'psychologize' as any other. Philosophy was regarded by him as the queen in considerations of the question: What is knowledge? and yet the sciences are tending to build philosophies of their own without the guidance of the general theoretical background. The paper seemed to the reviewer to miss the real spirit of modern science, including experimental psychology, and to assume wrongly that psychologists are anxious to 'psychologize' (an unfortunate term) in the sense of entering into philosophical problems. If metaphysics is, as the paper defined it, the art of getting on with one's facts, it is clear that the scientist must not be deprived of this art within the limits of his own field, even though he may not care to go into questions of 'ultimates.' The transgression of these limits by some scientists is probably irrelevant. With two exceptions, the papers that followed were by psychologists.

H. M. Johnson (Mellen Institute, University of Pittsburgh) discussed the social significance of fatigue. While fatigue in an individual receives respectful consideration from one's fellows, contrary to the case of drunkenness, fatigue and intoxication differ chiefly in the manner in which they are brought about. Once produced, they are essentially alike. Every major symptom of intoxication which has psychological interest is counterfeited in fatigue. Small doses of alcohol or of an opiate bring about excitation, as does a moderate amount of exertion. That certain special tasks—miscalled 'tests'—can be performed more speedily and more accurately at the end of the day than at the beginning, is shown by carefully controlled experiment. Both moderate exertion and slight drinking are attended by euphoria, an estimate of personal well-being which onlookers may not share. With later stages of fatigue, as of intoxication, come grosser impairment. One of the earlier forms is a growing clumsiness, particularly of the more delicate coördinations. The coördination of the two eyes occasionally fails, at least temporarily, giving rise to double vision. Speech defects, not merely in the form of slovenly enunciation, but also in the loss of words, are common in fatigue. Disturbances in attention, which increase accidents in the factory and on the highway, are frequently noted as symptoms of fatigue. One form is the loss of normal distractability so that a person who is still able to perform a restricted task acceptably disregards the important demands of unexpected changes in his surroundings. Interference with normal activity may produce emotional outbursts which are not common when the individual is not fatigued. Social restraints become increasingly disregarded, so that the person becomes inconsiderate, overbearing, quarrelsome, or may impose on others by slacking. Profanity, obscenity, and the like are more conspicuous in fatigue than in the normal state; and are sometimes manifested at such times by persons who are normally decent. In more advanced stages the fatigued person may lapse into infantile behavior, such as talking to himself and addressing inanimate objects which he tends to personify. The later stages are sometimes marked, as in the Chicago experiments on deprivation of sleep, by hallucination and delirium—an effect which the dervish strives to obtain by dancing before the Lord. Finally, as in alcoholism and in drug-addiction, the tired person may exhibit an abnormal appetite for the agent of impairment, postponing the hour of retirement, seeking countless odd jobs to give the postponement an appearance of reasonableness, and reading long after he does retire. If others attempt to induce him to hasten to rest, he may show resentment as does the drunkard, who, long after he has passed the pleasurable stages of intoxication, insists upon continuing to drink and upbraids his friends who suggest that he go home. Verworn suggests that both fatigue and narcosis are essentially special forms of cell-asphyxiation. Moreover, the deprivation of oxygen often produces every psychological symptom common to alcoholism and fatigue. The possibility is not overlooked, that, as many products of incomplete oxidation of food materials of the body belong to the families of the alcohols and the ethers, it is possible that in fatigue the cell is actually intoxicated by products of its own manufacture. Certain social considerations were added and were discussed in the meeting. May the euphoria from alcohol, denied the individual by society, be obtained by the right amount of fatigue? To what extent may society regulate the activity of individuals which brings about the graver forms of abnormal behavior? Is longevity due to the regulation of fatiguing activity, sleep, abstinence from the use of alcohol, tobacco, etc., if such regulations can prolong life, purchased at too dear a price? Is one to be deprived of the thrills from the extremes of fatigue and recuperation, the 'ups and downs' of life? In the discussion it was suggested that fatigue may not be all of the same kind, as intoxication is not.

J. F. Dashiell (University of North Carolina) on the question "Are there any emotions?" defined emotions as visceral reactions, summarized some of the leading recent researches on such reactions, and maintained that they have yielded no such uniformity of results as to indicate innate emotional patterns. On the contrary, he maintained that there are only facilitating and inhibiting organic processes which become controlled and regulated by society, and which, because of the constancy of certain social stimuli, may take on a degree of uniformity. No original data were presented. In the discussion of this paper, Dorcus held that experiments like those by Todd of Western Reserve did show a constant drop in the position of the stomach and that the balloon method of recording stomach contractions was inadequate for showing what changes actually occurred.

Vivienne R. McClatchy (Florida State College for Women) reported preliminary results of an investigation of the relation between general intelligence and commonality of association responses in the Kent-Rosanoff association test. Significant correlations had been found earlier between ratings on originality and uniqueness of associative response, and Miss McClatchy had published high postive correlations between commonality of response and proficiency in certain linguistic tests. The present investigation with 48 college students resulted in a coefficient of −0.27 between commonality of response in a group form of the Kent-Rosanoff test and score on the Otis S-A Test, and in one of −0.29 between commonality in the Kent-Rosanoff and scores in the Otis Group Intelligence Test, Advanced Examination. It was reported that a frequency table of responses by college students to the Kent-Rosanoff list is in preparation.

Max Meyer (University of Missouri), in the last paper on the forenoon program, raised the question of standardizing the content of the general psychology course. He held that each psychologist teaches *a psychology* but not psychology; that a vote of psychologists should be taken on what really is psychology. This would not prevent each instructor from teaching what he pleases, but the result of the vote should be made known to the public. He suggested that sub-committees might do similar work on the different divisions or courses in psychology. By a vote after discussion the matter was referred to the Council.

In the afternoon program Roy M. Dorcus (Johns Hopkins University) presented results of a study of the effects of exhaust gases upon performance in certain psychological tests. Through a period of about 2½ mo. 6 *Ss* were exposed daily for some 2½–6 hours each day to exhaust gases from a gasoline engine in amount of 2, 3, and 4 parts of *CO* per 10,000 of air. During this time they were given the following tests: tapping, steadiness, dynamometer, arithmetic, location memory, substitution, and continuous reaction. Results were compared with performances under control conditions. No significant differences were obtained.

W. W. Rogers (New York University) reported preliminary observations on the behavior of five kittens toward white rats of the same age. Three of the kittens were each kept in an observation cage with a white rat 6–10 hours a day, and two were each with a rat only 15 minutes once a week. Postive manipulatory responses of the kittens toward the rats first appeared at the age of 11 days. Earlier than this a cessation of mewing and of other random behavior was noticed when the kittens came into contact with the rats. The manipulatory behavior became more aggressive as the animals increased in age, but up to 68 days (the limit of the observations) no kitten made any food-taking reactions toward a rat. When the kittens were 67 days old they were each placed in the cage with a gray mouse. Only one kitten (the one with the wildest mouse) caught and killed the mouse; but its reactions toward the white rat the following day was not changed. The mouse was probably attacked because of its wild behavior, thus increasing the stimulus to arouse the attacking impulse.

English Bagby (University of North Carolina) discussed the nature of traumatic experience as illustrated in the behavior of a senior student at Yale. The young man come in to take several psychological tests "to know his mental assets and liabilities." His hyperactivity led to an investigation as to its cause. It developed that he had a sister who had "gone insane" (dementia praecox) at 22, the age he was now approaching. There were other cases of insanity among his relatives. The situation was such as to increase his tensions, and his knowledge was not sufficient for him to face his problem frankly and rationally. Everything pointed to an eventuation for him which he feared and was powerless to avert, so instead of constructive thinking on the situation he was thrown into distractive activities and repression. Such episodes, or experiences, were defined as traumatic because they establish conditioned fear reactions which are maladjusting and persistent. When this subject was informed, from the psychological examination, that his mental condition was still excellent; that whereas his sister had developed her condition between the years 10 and 22 in a condition of tension at home and he on the contrary, as he was reminded, had not been subjected to such a condition, having been in the army, the situation was resolved and the threatening tensions removed. This treatment averted the danger and the young man graduated and was apparently adjusted. Discussion of the paper developed the fact that examinations were approaching at the time of the stress and were probably worrying the subject and that the tensions due to this situation would naturally relax when he passed the examinations and took up some less difficult and trying work after graduation. The reader of the paper was not willing to assert that the feared catastrophe would not have been averted even if the psychological study and adjustment work had not come in at that point.

A. E. Edwards (University of Georgia) announced and described somewhat a laboratory manual for beginning students in psychology, which he considered new in the combination of demands made upon the students. These are: minimum instruction and requirement of a maximum of insight and effort, the requirement of studying and applying scientific method, the completion of all graphs, tables, etc., and of the write-up of the experiment before the student leaves the class. The work is to be done as nearly as possible as it would be done for a journal article, so far as quality is concerned. Fifty experiments are included, each one requiring from two to three hours' work. 'Scientific method' was not defined or characterized in the paper.

James B. Miner (University of Kentucky), in an attempt to show how we think of the 'mind,' suggested that explanation is finding the relations among facts and that these relations might be conceived in non-spatial analogies. For example, one can think of mind and brain not spatially but in mutual relationships. In this manner 'red' may be connected with both the genetic relations (mind) and the neural relations (brain). Lovejoy, in the discussion, held that this analogy had no especial advantage over other analogies, that it is no solution of the difficult problem. The reviewer suggests that there is no advantage in using 'mind' to refer to conscious aspects of experience, as some psychologists use it. 'Mental' may well apply to certain potentialities of response to complex conditions. To say that one has a good mind is to recognize such potentialities.

Following the afternoon program the members of the Society and their friends were delightfully entertained with a reception and tea at the home of General William H. Cocke, Superintendent of the Institute, and were invited to witness a battalion review. In the presidential address, after the dinner on the 6th, Josiah Morse considered the relative merits of behaviorism and mysticism from a philosophical point of view, finding the mystic horn of the dilemma the more satisfactory.

In a second paper Miss McClatchy presented a theoretical and statistical critique of the concept of social intelligence and of attempts to measure such an ability. The George Washington Social Intelligence Test yielded negligible differences among three groups of women from which differences might be expected: a class group, a group of fraternity women who were chosen for their social adaptability, and a group of non-fraternity women. Moreover, a correlation of only .16 was found between the George Washington test scores and the ratings for social adaptability of the socially selected group. A coefficient of .53 was obtained between the scores of 27 persons on the George Washington test and their scores on the Introversion-Extroversion Personal Inventory issued at Colgate University. It was contended that Moss had not established his social intelligence factor.

J. M. Fletcher (Vanderbilt University) presented "an old solution to the new problem of instinct," arguing for a lapsed-intelligence theory of the origin of instincts. Conscious striving was conceived as an agent in the shaping of structures, and the transmission of these acquirements in heredity in the early forms of life was posited. No new data were presented. The discussion of the paper centered chiefly on the assumption of consciousness as a directive factor, but also on the assumption of hereditary transmission of somatic changes which would have to apply to other than single-celled forms.

William M. Brown (Washington and Lee University) discussing some aspects of personnel accounting, presented no data but urged the organization for personnel work in the colleges. Some commonly employed aids for such work were indicated. Miner emphasized the importance of personnel work in the higher educational institutions by psychologists, so that the work would not be taken over by registrars who are less well prepared to do it. Edwards and Lovejoy called attention to dangers in personnel work until better technique is developed.

Joseph Peterson (George Peabody College) presented data on a constant error in the prediction of scores in the course in general psychology. The psychology scores were assigned on the basis of eight to ten competitive examinations, and showed a reliability coefficient of .84 as compared with .82 for Alpha. Predictions were based on one, two, or three intelligence tests (Alpha, Otis S-A, and Atkinson, a short group, ingenuity test). Predictability by the best combination of the tests used in multiple correlation with the 'criterion,' psychology scores, ranged from .13 to .28 better than that based on pure guess; but there was about 15% loss even in this low predictability when regression equations derived from one class were used for the forcasting of success in other classes, the only case of real prediction. Moreover, it was found that a constant error of overpredicting poor students and the reverse with good ones, lowered still further the efficiency of the predictions. The unfortunate thing about this error is that it is non-correctable. This constant error is greatest in the cases of extreme abilities, just where the prediction would be of most service if accurate. These studies have been carried out on about 200 students, and show that the factors making for success in a course like psychology (which is probably similar in this respect to other college courses) are very complex and that their determination to a high degree of accuracy will continue to challenge the genius of scientists for some time to come. The constant error indicated seems to be present in all forms of prediction based on any sort of data.

I. M. Brett (University of Tennessee) read the final paper, on the social status of art among the Greeks. He challenged the validity of the generally accepted view by estheticians that art activities are uniquely non-practical and disinterested and that they are pursued for their own sake, independently of any instrumental value. This view of art, he held, is incompatible with the facts of the art life of the Greeks and the Romans and of the medieval period. Among the Greeks the ideal of a great and glorious state dominated every important art activity. Literature, especially in the drama, developed as a means of treating social questions; and sculpture took form as the language of a conscious endeavor toward a balanced, coördinated and harmonious individual and communal life. Beauty followed as the unobtrusive result. Civic service was likewise the primary characteristic of successful Roman art and the common driving force of medieval art-creation centered around the effort to save souls. The cathedrals were the supremely successful adjustment of means to an intensely desired end.

The new officers elected are: L. R. Geissler (Randolph-Macon Woman's college), president; Paul L. Boynton (University of Kentucky), secretary-treasurer; and Max F. Meyer (University of Missouri), Ethel Bowman (Goucher College), and Josiah Morse (University of South Carolina), members of the council. The meeting in 1929 is to be held at the University of Missouri, Columbia, Missouri.

Peabody College JOSEPH PETERSON

THE THIRD ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The third annual meeting of the Midwestern Psychological Association was held on May 11 and 12 in Bascom Hall on the campus of the University of Wisconsin at Madison. On the afternoon of the first day there was a symposium on the problem of insight (Einsicht) in connection with the theory of Gestalt. Those who were invited to present their own views with regard to this concept were H. A. Carr (Chicago), C. A. Ruckmick (Iowa), K. S. Lashley (Inst. for Juvenile Research), A. P. Weiss (Ohio State), and H. Helson (Kansas). While there was a divergence of interpretation of this concept, most of the speakers emphasized the fact that insight designated a quick readjustment of response for which there was no apparent period of learning but which nevertheless depended upon past experience. It was generally considered a useful term which might bridge the gap between perceptual and ideational learning.

The evening was given over to two separate sessions which immediately followed an informal dinner at the University Club. One session was in the form of a round table conducted by Professor Max Meyer, on the teaching of elementary psychology. At this time Professor Meyer read a short introductory paper summarizing the reasons why a large committee representing various points of view in psychology should investigate the question of the uniformity of elementary instruction which might directly or indirectly then have its affect on the introductory work in our colleges and universities. Thereupon a number of representatives from various institutions were given an opportunity to comment on such a program. While there was some diversity of opinion as to the amount of actual material available, and the present status of scientific psychology, almost everyone agreed that benefit would be derived from a thorough discussion of this pedagogical problem.

In another simultaneous session, under the chairmanship of Professor J. R. Kantor, the problems of social psychology were discussed. Papers were read by the following: "Comparison of students' estimates with grades received," by J. P. Porter; "The measurement of attitudes," by L. L. Thurstone; "Preliminary report on the measurement of esthetic sensitivity," by N. C. Maier; "Prospectus for a complete study of personality," by W. H. Sheldon.

On Saturday morning there were again two concurrent sessions, one devoted to a symposium in learning and the other given over to experimental papers. In the session on learning the following papers were presented: "An investigation of meaningless material among pre-school children," by L. B. Slater; "A study of the language development of the pre-school child with special reference to sentence formation," by Dorothea McCarthy; "A reaction time experiment in a learning situation," by G. S. Snoddy; "The relative significance of the law of effect in learning," by A. R. Markt; "A problem in learning," by M. H. Lewis; "Reasoning in rats," by N. Maier.

In the experimental session, the following papers were read: "A classification of tonal sensation," by C. A. Ruckmick; "Sensibility to odors and other phenomena during a fast," by J. A. Glaze; "Reflex conductive time in catalepsy and in the manic phase of manic-depressive psychoses," by L. E. Travis; "Results of further studies of sleep," by H. M. Johnson; "Measurement of beauty and ugliness in singing," by M. Metfessel; "Quantitative data on the psychogalvanic reflex," by A. R. Lauer.

At noon a small group of interested psychologists met for luncheon at the University Club to discuss the possibility of a national honorary psychological fraternity. Through correspondence with a number of institutions, it developed that there was a considerable amount of sentiment in favor of such a movement. At the beginning of the discussion it appeared, however, that there were a good many negative opinions. Those present felt, however, that on the whole such a development might add an incentive to graduate study in psychology. A resolution was unanimously adopted, therefore, to the effect that this group advocated the formation of such a fraternity whose members be selected on the basis of high standards of scholarship and keen interest in experimental research, and that steps be taken toward the formation of a larger committee to gather at the time of the next meeting of the American Psychological Association and to consider this question further from the standpoint of other laboratories not represented at this time.

During the afternoon session, laboratory reports were called for from the various institutions represented. In many cases, graduate students briefly summarized their own experiments while in other instances some psychologist in charge surveyed the program of research which was under way in his laboratory. At the close, Professor Madison Bentley made a unique contribution in his critical and suggestive analysis of an ideal experimental procedure.

Throughout the meetings, opportunity was afforded for the inspection of the psychological laboratory at the University of Wisconsin. Some thirty-two experimental procedures were set up for demonstrational purposes. There was also a display of new apparatus. At the dinner, which was held in the Crystal Room of Hotel Loraine, the retiring President, A. R. Gilliland, gave a scholarly address on the outcome of some recent investigations of personality traits. At the speakers' table three ex-presidents of the American Psychological Association were seated: Professors Bentley, Carr and Dunlap. A vote taken at this time resulted in the election of A. P. Weiss as president for the coming year, and E. Culler as new member of the executive council. At this time it was announced that the next meeting would be held at the University of Illinois, Urbana, Illinois. The assembled psychologists expressed, through a rising vote, their thanks to the retiring officers of the Association for the instructive program provided, to the department of psychology at the University of Wisconsin for the excellent facilities afforded for the sessions, and to the University for convenient accommodations and thoughtful courtesies extended.

It was the general feeling that these meetings were unusually successful. Although the sessions were much more fully attended, there was a registered attendance of 121. About 100 attended the conference dinner on Friday evening, and 81 heard the presidential address at the final dinner.

The following universities, institutes, and colleges were represented at the meetings: Antioch college; Indiana University; Institute for Juvenile Research; Iowa State College; Mellon Institute of the University of Pittsburgh; Northwestern University; Ohio University; Ohio State University; Stevens College; University of Chicago; University of Illinois; University of Iowa; University of Kansas; University of Michigan; University of Minnesota; University of Missouri; and University of Wisconsin.

University of Iowa CHRISTIAN A. RUCKMICK

WHAT IS WHITE?

Until a few years ago all quantitative studies of light were so uncertain that this was a question of little interest. Unless one were working specially on the blue end of the spectrum any light that looked fairly white to the experimenter was acceptable, though artists had already realized that there were difficulties, and had come to prefer the light of the north sky. Similarly, in selecting our clothing we checked our judgment made in the yellow artificial light of the store, by taking it out to the blue-white light outside the door.

More recently there has come to be a demand for a real standard. The first one to be adopted was naturally the north sky-light, already accepted by studio artists; but every one realizes that light coming from a source as obviously blue as the sky, must be too blue. More recently the Bureau of Standards has chosen the direct light of the noon sun; but I think that cutting down the sun's light to any point of brightness where the eye can see color efficiently, will convince the observer that the sun is too yellow. (The simplest way is to let sunlight fall on a ground glass.) Therefore, white obviously lies somewhere between these two values. May I suggest, as the next approximation to a true value, the light of a white cloud, illuminated alike by the sun and by the diffuse blue light from all the rest of the sky. The north light of a cloudy day, when there is not enough smoke or coarse water-droplets to give a yellow tint, should have about the same value.

I would also like to suggest a possible refinement of procedure, that may give a more accurate value for white than the eye can detect directly.

When the eye, adapted partially to darkness, is exposed to a bright light and then again darkened, there appears a series of colors. If the stimulus is short there is a single flash of color immediately after the light is cut off, the Hering after-image. If the stimulus is stronger, there is a series of colors, but the first one to appear with a given stimulus light seems to be constant, and the same as the Hering image for a light of the same hue. Now I find in my own case that this first after-image (under photopic conditions) is as near as I can judge, of the same hue as the stimulus-light, but of notably higher saturation. In this case, a light so near white that it is indistinguishible, but really slightly tinted, should be followed by an after-image with a visible hue, while an exactly white light should be followed by a white Hering-image. In my own case I find that the Hering image from the blue sky is a rich blue, from a tungsten electric light deep yellow, and that the first phase of the flight of colors is likewise blue or yellow. Then trying a blue-bulb electric light, the first phase is apple green, but notably quick to disappear and be replaced by the second color of the flight—this with a light that is indistinguishible from white to direct vision. Finally, the light from a white cloud produces a Hering image with no perceptible hue; and if the flight of colors is elicited by such a light, there is a substantial preliminary white phase before the usual color-display commences.

I do not know how general this phenomenon is; individual variability is the most striking feature of the colored after-images in general, but if a wide-spread phenomenon, it should give a promising method for selecting the true white. There is certainly a disturbing factor with a certain proportion of observers; at least at certain brightnesses the flash-image shows a strong bias for blue or violet. I think it possible this effect would be swamped with brighter lights, as it seems to correlate with the ability to see the 'blue arcs' recently discussed in *Science*, but the danger would have to be kept in mind.

Cornell University W. T. M. FORBES

THE GENERAL INDEX OF THE FIRST THIRTY VOLUMES OF THE AMERICAN JOURNAL OF PSYCHOLOGY

This volume of 200 pages marks an advance in the indexing of periodical literature in psychology. Several of the psychological periodicals (most of them German) have assembled their tables of contents for a period of years; but no thorough analysis of the literature comparable to the present work has been carried through. Besides giving a complete editorial history of the JOURNAL for the first thirty years (1887-1919), it lists by authors all articles and reviews. But the main part of the work (140 pages) is an analytical index under subjects. Here there are something like 20,000 entries under 3000 or more subject-heads. Since these references include not only articles published in the *American Journal* but as well a vast number of books and papers reviewed, the psychologist or lay reader will here find a cross-section of the entire field for more than a generation. This list serves, therefore, as a general means of entrance to the whole subject. While not so inclusive as thirty volumes of the *Psychological Index* (which gives only titles and authors), it is much more detailed, much more explicit, and much easier of reference. The two bibliographies are complementary works. The JOURNAL Index has the advantage of a single plan and a single editor. While the volumes were carefully carded by individual psychologists, the standardizing of the whole fell of necessity upon one editor. To this task Professor Dallenbach has devoted several months of the most exacting and painstaking labor. No one who pretends to an acquaintance with the literature of psychology will neglect to use the volume. It will be particularly useful as a part of the *Handapparat* of graduate and research students, who may well begin with the 41 entries (p. 153) under *periodicals*, proceed to *psychology, experimental psychology, apparatus*, and *textbooks*, and then go on to more specific subjects of research. The collegiate instructor who is responsible for courses, students' problems, and the laboratory will soon discover that the INDEX will readily and easily admit him to the history and the setting of almost any psychological problem or topic which appears in his teaching.

University of Illinois MADISON BENTLEY

TABLE VI

COMPUTATION OF THE STIMULUS-DIFFERENTIALS WHICH ARE ARRANGED IN DESCENDING RANK ORDER, AND CORRELATION OF THE *O*s REPORTS WITH THEM

Stimulus-temperatures			Numerical difference from physiological zero (32°–33°C)		Stimulus-differential $(32-c)+3(w-33)$	Rank order	Observers							
							D		Dn		G		H	
Cold	Warm	No.					Times 'hotter'	Rank order	Times 'hotter'	Rank order	Times 'hotter'	Rank order	Times 'hotter'	Rank order
2°	43°	*4*	−30	+10	60	1	29.5	1	29.0	1	27.0	1	29.5	1
10°	43°	*8*	−22	+10	52	2	22.5	3	25	3	24	2	23.5	4
2°	40°	*3*	−30	+ 7	51	3	27.5	2	28	2	23.5	3	26.5	2
10°	40°	*7*	−22	+ 7	43	4	19	4.5	19	6	16	6.5	18.5	6.5
20°	43°	*12*	−12	+10	42	5.5	15	6	20	5	21.5	5	24.5	3
2°	37°	*2*	−30	+ 4	42	5.5	19	4.5	22	4	10	9	18.5	6.5
10°	37°	*6*	−22	+ 4	34	7	10.5	9	16	7	7	10.5	19.5	5
20°	40°	*11*	−12	+ 7	33	8.5	13.5	7	12	9	14.5	8	16.5	8
2°	34°	*1*	−30	+ 1	33	8.5	3.5	11	2	13	0	15	8	12
30°	43°	*16*	− 2	+10	32	10	12.5	8	16	8	22.5	4	12.5	9
10°	34°	*5*	−22	+ 1	25	11	2	13	4	12	0	15	11	10
20°	37°	*10*	−12	+ 4	24	12	4	10	5.5	10	7	10.5	10.5	11
30°	40°	*15*	− 2	+ 7	23	13	2.5	12	4.5	11	16	6.5	5	13
20°	34°	*9*	−12	+ 1	15	14	0	15	0	15	0.5	13	4	14.5
30°	37°	*14*	− 2	+ 4	14	15	0	15	0	15	3	12	4	14.5
30°	34°	*13*	− 2	+ 1	5	16	0	15	0	15	0	15	2	16
			r =				0.954		0.935		0.749		0.943	

SUMMARY

The results of this study, in which the intensities of heat sensations (aroused by different combinations of warm and cold temperatures in a heat grill) were directly compared by the method of paired comparisons, may be summarized as follows.

(1) The number of 'hotter' reports varied, for all the *Os*, when any given temperature was taken as the point of reference ('intra-standard' comparisons), with the size of the stimulus-difference: the larger the difference between the temperatures of the grill-systems, the greater the number of 'hotter' reports.

This correlation did not obtain, however, when stimulus-differences were compared that were taken from different points of reference ('inter-standard' comparisons): the larger stimulus-differences frequently gave the smaller number of 'hotter' reports.

(2) The distribution of the cases in which the *Os* reported that they did not experience heat, *i.e.* the 'no' reports, corroborates the findings of the positive, the 'hotter,' reports in every detail. In 'intra-standard' comparisons, the 'no' reports vary inversely with the stimulus-differences: the smaller the stimulus-difference the greater the number of 'no' reports. In 'inter-standard' comparisons, however, this relation did not always obtain: the smaller stimulus-differences frequently gave the smaller number of 'no' reports.

(3) The space error, both in the comparisons of identical and different stimulus-combinations, is negligible for all the *Os*.

(4) The disparate nature of the results, obtained from 'intra-' and 'inter-standard' comparisons of the stimulus-differences with the number of 'hotter' and 'no' reports, indicates that the intensity of the heat sensation does not vary directly with the stimulus-difference.

(5) The distribution of the *Os*' 'hotter' reports among the different stimuli indicates that the stimulus-value of the different temperatures varies functionally with the distance from physiological zero, and further that the stimulus-value of the warm stimulus should be weighted by the constant 3.

(6) The correlation between the rank orders of the *Os*' 'hotter' reports and the stimulus-values computed by the weighted formula, $(32 - c) + 3(w - 33)$, is very high. We therefore offer that formula as an equation of the stimulus-differential for heat.

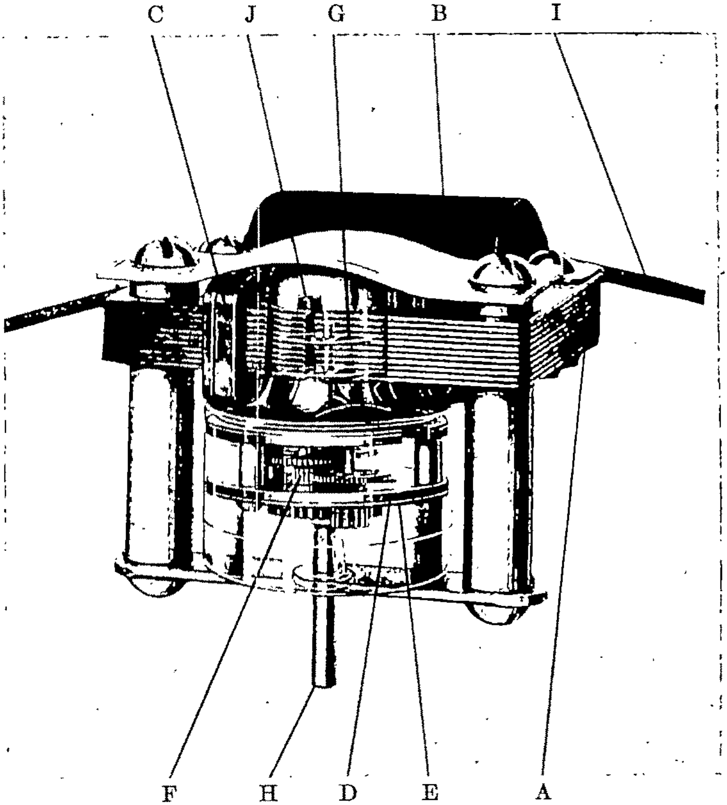

Plate I. A Small Self-Starting Synchronous Motor

A = Field
B = Field Coil
C = Shading Coils
D = Bearing Plate
E = Face Plate

F = Gear Train
G = Rotors
H = Terminal shaft
I = Flexible Leads
J = Oil Drainer

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A THEORY OF CIRCUIT INTEGRATION: A CRITICISM OF THE 'CENTRALLY AROUSED PROCESS'

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The supposition that there are centrally or cortically aroused processes has had a long and interesting history. It has received repeated but only apparent substantiation not only from experiments which have seemed to supply the necessary facts but from speculative deductions the plausibility of which has been accentuated not so much by facts as by the lack of contrary evidence.

With philosophical and epistemological distinctions between the subjective and the objective influencing psychological thinking from the outset and with a dualistic theory which associated mind with the former and matter with the latter, the way was paved at an early date for a persistent and vigorous faith in an 'inner world.' As soon as it was found that an injury or disease of the brain affected this 'inner world' there gradually crystallized the notion that subjective phenomena and the brain went hand in hand. So perfect has been the correlation, apparently, between mental and cerebral activities that the possible inadequacy of this fact as a scientific guide to a psychophysical theory has not been adequately inspected. Meanwhile we have been speaking of centrally aroused processes with almost as much assurance as a scientific verification justifies.[1]

It is not the intention of the writer to set forth too serious a substitute for the theory of centrally aroused processes. Sufficient evidence is not available. Rather it is the purpose of this paper to raise an issue and to offer a few suggestions toward

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[1] Cf. O. Kulpe, *Outlines of Psychology*, 1893, "the latter (centrally excited sensations) need no peripheral excitation. . . ." (p. 35). They may be correlated, however, with a co-excitation of peripheral organs (p. 85). See p. 184 for a defense of a theory of cortical sensations, and p. 272 for a central theory of feeling. Note also p. 441. W. B. Pillsbury, *Essentials of Psychology*, 1920, 141 f. Madison Bentley, *The Field of Psychology*, 1924, 213, 241, 252. H. C. Warren, *Human Psychology*, 1919, 148, 277.

what might be called a 'total integration' theory. Throughout it should be borne in mind that no effort is made to belittle the importance of integrations to which the cortex unquestionably contributes important features. Specifically it is the prevailing implication that the brain functions under certain conditions independently of any peripheral stimulation that is here subjected to criticism.

We begin with one of the major considerations fundamental to the central origin of mental processes, the problem of the localization of function. It is hardly conceivable in the light of such information as we have that images of a specific quality and widely differing from one another in quality are dependent upon any part of the cortex that may happen to be functioning at the time the images appear. Quite as difficult is it to suppose that the brain as a whole subserves images of all modalities independently of the periphery. Either assumption begs too many questions. Perhaps it is reasonable to assume that the margins of the different sensory areas subserve imagery for the modality in question. But such a theory is open to serious question, as will soon be pointed out.

The confused state of our present knowledge with reference to localization of function is due, in general, to the prevailing tendency in human thinking to interpret two contiguous events exclusively as cause and effect. To destroy a certain region of the brain and forthwith to lack a certain mental process has been regarded as evidence that the former was the seat of the latter. Another source of error in thinking is the tendency to look for something real in entities, structures, stuffs, and the like, which has shown itself in an anatomical bias in our discussions of the reflex arc. With mental processes construed as structural entities and a tripartite conception of the reflex arc, it naturally followed that one must search for an anatomical place of abode somewhere along the arc. One structural notion begets another.

It is not surprising, then, that we should be called to task for our mistakes. Results of recent experimentation upon the effect of brain injuries on behavior in animals, especially Lashley's elaborate researches,[1] compel us materially to alter if not to repudiate entirely the notion of localization of function. After all, why should we have concluded that, because destruction of the occipital lobe was correlated with a loss of vision, this lobe contained the seat of vision? One might as well conclude that because he cannot see without a retina the retina is the seat of visual processes, or that the loss of cutaneous sensitivity in the hand when a nerve in the fore-arm is sutured proves that the seat of the cutaneous sensitivity in question is the suture. Destroy a single link in a chain and the chain ceases to function.

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[1] K. S. Lashley and S. I. Franz, The effects of cerebral destruction upon habit-formation and retention in the albino rat, *Psychobiol.*, 1, 1917, 71-139; K. S. Lashley, Studies of cerebral function in learning: I. *Psychobiol.*, 2, 1920, 55-135; Studies of cerebral function in learning: II. The effects of long continued practice upon cerebral localization, *J. Comp. Psychol.*, 1, 1921, 453-468; Studies of cerebral function in learning: III. The motor areas, *Brain*, 44, 1921, 1-31; Studies of cerebral function in learning: IV. Vicarious function after destruction of the visual areas, *Amer. J. Physiol.*, 59, 1922, 44-71; Studies of cerebral function in learning: V. The retention of motor habits after destruction of the so-called motor areas in primates, *Arch. Neur. Psychiat.*, 14, 1924, 1-28; Studies of cerebral function in learning: VI. The theory that synaptic resistance is reduced by the passage of the nerve impulse, *Psychol. Rev.*, 31, 1924, 369-375; Temporal variations in the function of the *Gyrus Precentralis* in primates, *Amer. J. Physiol.*, 65, 1923, 585-602; Studies of cerebral function in learning, *J. Comp. Neurol.*, 41, 1926, 1-58.

Lashley's results suggest that the main axones from the lower visual centers to the cuneus are merely the normal anatomical connections between the lower brain centers and the cortex and that the cuneus is a visual center only because the visual nerve currents happen to reach the cortex by that route. That is, the relation of this area to vision is merely that of an entrance-way to the cortex. The visual functions, presumably, do not terminate in this region. How other parts of the brain take over the lost receptivity function is not known but presumably as more devious routes take on the conduction of currents from the optic nerve.

Moreover, when we assume that a visual consciousness is subserved cortically by the visual area alone, we are at once confronted with the task of understanding why, as nerve currents reach that area, the visual process comes into existence and why, as the currents leave the area, visual processes cease to exist. Where are the peculiar structures at the boundaries of this area that account for these striking events? It would seem more plausible to assume that visual processes appear and disappear when a peripheral stimulus ceases, or when the visual response, subserved by action-patterns extending through the cortex, is somehow inhibited.

Further, why are there not distinguishing features at the boundaries of the different sensory areas that would account for the qualitative differences in the mental processes, and why is there not something observable between a so-called sensory area and its margin that would help us explain the differences between sensation and the image? Not only are such cues lacking but such a central theory presents its logical difficulties. These were pointed out long ago by Dewey.[3] By what logical prerogative do we justify the notion that covert behavior[4] is sensory and dependent upon the sensory segment of the reflex arc and that it is not motor and not dependent upon the motor segment of the arc? There seems to be no evidence from a functional point of view why we should adhere so strictly to such anatomical terminology. Exactly where, in the arc, do the motor functions begin? Movements are caused by stimulation of the so-called sensory areas! We have no evidence that the motor areas function independently of the sensory.

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[3] John Dewey, The reflex arc concept in psychology, *Psychol. Rev.*, 1, 1893, 357 ff.; see also *J. Phil.*, 9, 1912, 664 f.

[4] R. H. Wheeler, Outline of a system of psychology, *Psychol. Rev.*, 30, 1923, 151-163.

If the brain as a whole or at least the major portion of the cortex functions as an integrating region in the performance of a single act, no matter from what source the stimulation, it would seem as if the same conditions held for mental processes if the latter have anything to do with behavior. Then the requisites for brain functioning at any time when mental processes are involved, are not only that the brain as a whole be intact but that a certain balance of potentials obtain within the brain and between the brain and the periphery. Shut off all afferent impulses in any one pattern and presumably that pattern would be inert. In other words sensory and motor segments are serviceable only in explaining alterations and overlappings of patterns. The pattern in any one case is to be regarded as a whole from sense organ to muscle whether the central regions involved be large or small, simple or complex, and no matter how much the latter contributes in the control of the resulting response, and no matter how much it surpasses in area or bulk the nerve material at the periphery.

We are evidently dealing with a problem in dynamics. While the brain contains an unmeasured amount of stored energy, it is a question, from the standpoint of energetics, whether this energy is ever available except as disturbing or catalytic agencies reach it from without. While facilitating agencies may reach it from the blood stream, such influences seem hardly adequate to the setting of nerve impulses in action. We might expect easy or more difficult conduction according to the condition of the blood, but we should hardly expect a nerve current to arise except as the nerve itself is excited somewhere along its course. As far as we know the normal place for such a disturbance is the receptor. Granting the theoretical possibility that, since energy from the blood is worked over into nervous energy, the circulation of the blood in the central nervous system may excite mental processes, we are still confronted with the very difficult problem how it is that these circulatory changes are occurring continuously at the right time and in the right place! The alternative, of course, is to suppose that when there is no peripheral stimulation whatever either over extero- or proprioceptive channels, the brain is capable of discharging energy into the muscles, or of working over its energy in such fashion as to maintain covert behavior. But why impose upon the brain the embarrassing task of lifting itself by its bootstraps? If the brain does function as a unit we should anticipate no expenditure of energy in the absence of a disturbance from without. Perhaps a little light is thrown upon the problem when we reflect that even in the absence of much extero-stimulation we have afferent avenues from the muscles, a situation which no doubt helps to supply the equilibrium or proper adjustment of potentials between the centers and the periphery. We know, also, that when we reduce stimulation of the sense organs and relax throughout the body, we approach a state of sleep.

Rather than to interpret the brain as a creator, the better working hypothesis would seem to be that of regarding it as a combined battery and amplifier which functions only when the circuit is kept open. While this analogy is intended only as a crude figure it may prove worth considering in the light of the discussion which follows.

We are informed that the sense organs are not only connected to the muscles of the body by way of the main sensory-motor routes but also by way of collaterals that form shorter circuits. Accordingly, upon the stimulation of a sense organ there is possibly a slight innervation of the musculature before afferent currents from the sense organs reach the higher centers.[5] Before the response takes place, whether it be covert or overt or both, there has presumably been a secondary sensory stimulation over proprioceptive channels. This may explain the presence of kinaesthesis in connection with all mental processes and harmonizes with the reflex-circuit concept as proposed by Dewey.

Results from experiments on the acquisition of skill point in the same direction.[6] In the formation of any overt habit not only are the pathways from the most used receptors to the most used effectors important but pretty much the entire nervous system. It is evident that in habit formation of practically any kind there is an adjustment of the whole nervous and muscular system. For example witness the disastrous effects of trying to shoot a rifle accurately while taking a different stance from that originally learned.

Couple this general consideration with the recent emphasis of the *Gestalt* school[7] that at any one time we are responding not to a single stimulus or to a limited group of stimuli even when we do nothing more than to look at a pair of gray squares, but that we are responding to a total situation. What we see is the direct product of a vast number of stimuli, and the response, considered introspectively or otherwise, is the product of a very general adjustment on the part of the central nervous system, an adjustment which depends upon relationships obtaining between widely distributed parts.

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[5] C. J. Herrick, *Introduction to Neurology*, 1918, 342 ff., refers to Kappers, *Bericht ü. d. III Kongress exper. Psychol.*, 1908; *Folia Neuro-Biol.*, 1, 1900, 507-532.

[6] John Watson, *Psychology from the Standpoint of a Behaviorist*, 1919, 293 f.

[7] Cf. K. Koffka, The psychology of perception, *Psychol. Bull.*, 19, 1922, 542; *Growth of the Mind*, 1925; Harry Helson, The psychology of *Gestalt*, this JOURNAL, 36, 1925, 342-370, 494-526; 37, 1926, 25-62, 189-223.

If the total integration theory not only has logical but anatomical and experimental support it is worthy of more serious consideration. Indeed it has a profound significance for systematic psychology for it reveals a possible mechanism for the multiple stimulation from differing sense modalities that is apparently essential to the existence of a mental process, if the simplest mental process is a complex.[8] Moreover, it furnishes us with a reason for the fact that the interpretive element in the percept always involves kinaesthesis and is dependent upon kinaesthesis,[9] and shows how there is always peripheral stimulation in the development of meaning no matter how rapid or attenuated that development is. It is no longer necessary to speculate blindly on the alleged origin of integrations in the central nervous system. It ties up the facts of covert behavior with those of overt behavior and this seems reasonable if man is a biological phenomenon. Finally, it is a constructive substitute for the tripartite concept of the arc. In this view since covert behavior depends upon the functioning of the complete reflex circuit the former is not exclusively sensory or motor but both and not exclusively aroused by forces at the periphery or in the center but both. There is always an adjustment of the centers to afferent currents and of the effectors to efferent currents.

We now pass to the second main problem, namely that of imagery, the stronghold of the advocates of centrally aroused processes. That the image is a centrally aroused process seems to receive confirmation from a multitude of sources. An image, it is said, is not dependent upon the stimulation of any sense organ. We may have visual imagery of a red disk when there is no red disk before us. We may have olfactory imagery of a certain odor when the nose is not being stimulated. If the nose were being stimulated the resulting process is a sensation, not an image. Further, it is possible, apparently, to have kinaesthetic imagery of a leg-movement after the leg has been amputated. Drugs, such as hashish, cause certain images to appear and this imagery possesses certain peculiar properties of behavior. The cause must be central. Moreover, imagery lacks the localization, clearness, duration, richness of quality and the intensity of peripherally aroused processes.

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[8] R. H. Wheeler, Persistent problems in systematic psychology: II. The psychological datum, *Psychol. Rev.*, 32, 1925, 261ff.

[9] R. H. Wheeler, The development of meaning, this JOURNAL, 33, 1922, 223-233.

All this evidence, however, may be misleading. Qualities and other properties of sensation apparently depend upon specializations of structure in the end-organs and upon certain properties of the stimulus. Evidently these sources are responsible for corresponding peculiarities of nerve impulses passing into the central nervous system from the various peripheral organs. If all such nerve impulses are alike how is the central nervous system affected differently by the different stimuli? Of what use are the sense organs, anyway, if their specialization fits only the stimulus and has no relationship with the nerve centers? We may choose to regard the sense organ as the seat of the sensation in question but this view has difficulties too obvious to mention. Assuming that nerve impulses from the retina differ in wave form from nerve impulses from other sense organs and that these differences account for the distinguishing features of visual sensations the problem arises, where are we to find equivalent specializations of structure to account for like differences between visual and other modalities of imagery. This difficulty vanishes if such brain patterns as subserve imagery include afferent routes from the sense organs. It might be, of course, that once a central pattern is thus set up by the action of a sense organ there is a functional specialization that accounts for these differences in imagery, or there may be a structural differentiation which neurologists have not yet discovered. But this interpretation meets with the objection heretofore mentioned that, functionally, there is no set region where an exclusively visual function must end and an auditory function begin and no position along the reflex arc at which it is conceivable that a sensory function ends and a motor function begins.

Evidence that there is no peripheral stimulation at the sense organ in case of imagery is inconclusive for the further reason that under normal conditions most if not all of the sense organs are continuously functioning. This means that specialized end-organs are affecting the centers constantly and that this effect is going on while an individual is having imagery. It is by no means clear, for example, that the retina does not supply peripheral stimulation for visual imagery. Those born blind never have visual imagery and the usual explanation has been to the effect that the retina has not supplied the appropriate 'traces' in the visual cortex. Quite as possible is the reason that the blind have no functioning retinas by means of which the visual patterns are constantly being excited. We do not know that the adventitiously blind who have visual imagery do not also possess functioning retinas and that visual imagery declines as the retinas degenerate. On the other hand we do know that many of those who have lost their sight are not strictly blind, though they cannot distinguish daylight and darkness, for they still see black or retinal grey and have entoptic colors. This correlation may mean nothing but it certainly sheds a new light upon the problem at hand, and the writer, at least, has not found an adventitiously blind individual still having visual imagery in whom it is evident, after investigation, that no functioning retina is left. If this situation should turn out to be true generally we can see how the brain, functioning as an amplifier and battery, makes over this crude 'material' from the retina with the aid of proprioceptive stimulation. Thus we have a possible physiological basis for imagery.

If resident sensations elicited by inherent retinal 'light' (and no one knows how long this light will last with a subject confined in absolute darkness) furnish the raw material for qualitative features of visual images, it may very well be that resident sensations in the other modalities function in like manner. We do not know that the adventitiously deaf do not lose their auditory imagery with total loss of hearing. We cannot trust the evidence from a single or even a small group of individuals, especially untrained observers, because there are many who are not deaf and insist upon having auditory imagery in which there is not an auditory factor which stands out as such.

Perhaps there is a cochlear 'tone.' Elaborate tests should be made on trained subjects whose organs of Corti are known to be destroyed; but unfortunately for science such observers are rare! Experiments in sound-proof rooms might furnish interesting results one way or the other, but unreliable results, since such rooms are seldom sound-proof. Moreover, the individual himself must breathe and his heart must keep going!

Genuine imagery of coldness and warmness should be very carefully studied under controlled conditions. It is much easier to image warm when the skin is warm, cold when the skin is cold. It may be doubted whether it is possible to have tactual imagery localized on an anaesthetic area. Do hysterical patients have tactual imagery localized on the 'insensitive' areas? If our thesis be true there would be no genuine tactual imagery from an anaesthetic area; and if our thesis holds for a few modalities it probably holds for them all.

We have additional clues. The types of imagery which prevail in mental life generally are from such modalities as are most obviously subject to continuous peripheral stimulation. Outstanding among these are the visual and kinaesthetic. This fact is quite as consistent with our proposal as with the view that the more frequent the peripheral stimulation the more ready to function are traces in the cerebral centers. Moreover, if imagery depends upon minimal stimulations of the sense organs we have one tangible reason, at least, for the observed differences between sensations and images. These stimulations are approximately liminal and hence the resulting processes are fleeting, vaguely localized, of weak intensity and lacking in detail unless, at the same time, the brain's elaborative function, and proprioceptive stimulation are at a maximum.

Facts recently brought to light in studies of *Eidetiker* show that the eidetic image arises most readily when the subject fixates a background, preferably the one on which the picture was originally exposed or one like it.[10] Dreams usually lack color, a fact which fits in with our thesis perfectly. The readiness with which different kinds of images may be elicited through suggestion points to peripheral stimulation and to a close relationship in general between central and peripheral processes. This brings us to a consideration of the conditioned reflex as a mechanism which may furnish the required secondary stimulus that controls the time of appearance, the form and perhaps other properties of the image. Presumably at least one of these secondary modes of stimulation is always proprioceptive.

By means of the conditioned reflex, presumably, motor attitudes, gestures, inner speech, etc., become supplementary stimuli in the arousal of visual, auditory, and other non-kinaesthetic modes of excitation, functioning together with the weak excitations from the relevant sense organs. Likewise, by means of the conditioned reflex, a visual stimulus may become adequate as one mode of peripheral excitation for auditory imagery or an auditory stimulus may be one of the conditions for the arousal of a visual image, and so on. We may suppose that by such a substitution of stimuli a visual response, for example, drains from a pattern connected with the ear. Weak excitations from the ear supply the material that is to be amplified. Visual and kinaesthetic stimuli furnish the conditions under which this auditory imagery takes on localization and form, and under which it possesses durative and clearness aspects.

Our theory proposes, therefore, that there are two kinds of peripheral stimulation in the case of every image, the first from the sense organ of the same modality as the image, and the second from one or more other modalities, one of which is always kinaesthetic. A given muscular adjustment may, in this view, be the adequate stimulus for a variety of images, depending upon the temporary alignment of patterns through the central nervous system. There need not be a muscular contraction of a specified amount, vigor and intensity for every specified property of the image.

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[10] E. R. Jaensch, cf. *Zsch. f. Psychol.*, volumes 84-92, 1920-1923.

So far we have stated our theory and have considered some of the objective evidence. We now turn to the evidence from introspection. While such evidence is still far from complete we submit that there is nothing in it that justifies a theory of centrally aroused processes. The sort of introspective evidence to which we refer, however, is furnished not by the verbal report method but by 'complete' introspection. It has already been pointed out that only the complete method furnishes data adequate for purposes of systematization.[11] We pointed out that the complexity of the simplest existing mental process has been overlooked. It is in this inadequately analyzed pattern that we may look for the introspective cues which point to a total-integration theory. One of these cues is the much neglected and at the same time much abused component, kinaesthesis. Another is the synaesthetic character of the simplest mental process. The first points to a peripheral factor, proprioceptive stimulation; the second, to innumerable possibilities in peripheral stimulation by way of the conditioned reflex. It is not improbable that the stimulus situation which arouses an auditory image, for example, is in part, a visual perception of an instrument, a singer, a word, a musical note or what not; or it might be a verbal image with its incipient movement and shifts of tension in the larynx. The combinations and possibilities are legion.

Evidence that all of the so-called centrally aroused processes contain kinaesthesis is becoming so significant that one would be justified in the case of negative results in looking for a substitute that owed its origin to the conditioned reflex. Visual synaesthesis is a case in point. The evidence, however, is scattered;[12] much of it is a by-product of other purposes and not properly checked. Few indeed have been the researches which can be considered reliable on this point. The chief reason for this is the fact that introspective observers have been slow to report kinaesthetic processes. It is not safe to conclude that, because some observers give negative results, there must be individual differences. The presence of kinaesthetic factors reported in a certain number of visual images, for example, is legitimate evidence pointing to the probable existence of such factors in all visual imagery in the same subject. There have been many observers who have failed to recognize delicate shifts and tensions in minor muscles, or who fail to recognize very weak and diffuse shifts more widely distributed over the body. Our muscles are never entirely relaxed and never absolutely quiet in waking life. When unrecognized these kinaestheses unquestionably play a part in the undifferentiated and unanalyzed background of the observer's experience. Thus, in case of visual imagery, incipient shifts of accommodation, changes in tension toward convergence and divergence—strains insufficient to produce observable eye-movement in any direction—have been overlooked either through lack of introspective training or for the want of the proper mental set.

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[11] R. H. Wheeler, Some problems of meaning, this Journal, 34, 1923, 185-202, especially 195 ff.

[12] R. H. Wheeler, Visual phenomena in the dreams of a blind subject, *Psychol. Rev.*, 27, 1920, 313-322; The synaesthesia of a blind subject, *Univ. of Ore. Publ.*, no. 5, 1920, 1-61; R. H. Wheeler and T. D. Cutsforth, The synesthesia of a blind subject with comparative data from an asynaesthetic blind subject, *Univ. of Ore. Publ.*, no. 10, 1922, 1-104; The rôle of synaesthesia in learning, *J. Exper. Psychol.*, 4, 1921, 448-468; Synaesthesia and meaning, this Journal, 33, 1922, 361-384; Synaesthesia, a form of perception, *Psychol. Rev.*, 29, 1922, 212-220; Synaesthesia in the development of the concept, *J. Exper. Psychol.*, 8, 1925, 149-159; T. D. Cutsforth, The rôle of emotion in a synaesthetic subject, this Journal, 36, 1925, 527-543. S. C. Fisher, The process of generalising abstracton and its product, the general concept, *Psychol. Monog.*, 21, 1916, no. 30, 1-213; S. W. Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, no. 117, 1-161; E. L. Woods, An experimental analysis of the process of recognising, this Journal, 26, 1915, 313-387; R. H. Wheeler, An experimental investigation of the process of choosing, *Univ. of Ore. Publ.*, no. 2, 1920, 1-59; H. R. Crosland, A qualitative analysis of the process of forgetting, *Psychol. Monog.*, 29, 1921, no. 130, 1-159.

In this connection it is easy to be led astray by objective methods that produce negative results. It is hardly safe to conclude that because we have failed to record eye-movements objectively the muscles of the eyes are not functioning in giving to visual processes the kinaesthesis to which a complete introspection testifies. It is quite as easy to make mistakes with an objective as with an introspective method when all of the conditions of experimentation are not controlled, and it is impossible to control them all in a psychological experiment. The ideal procedure of course, is to attack such a problem with both methods. That this is of paramount importance for accurate results is indicated by some recent work of Bott on the mechanics of continuous flexion and extension at the wrist.[13] The curve obtained by having a recording device attached to the fingers shows a gradual rather than a sudden shift from flexor to extensor movement. If flexion ceased before extension commenced movement of the hand would cease momentarily but this is not the case. Evidently, as Bott suggests, one phase of the movement begins before the previous phase ends; that is, extensor muscles begin to contract before the opposed set reaches its maximum of relaxation, and vice versa. This means that there is a tendency toward movement in one direction while observable movement continues in another. The writer has frequently observed qualitative differences in kinaesthesis which are presumably to be interpreted in just this way.

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[13] E. A. Bott, Some characteristics of voluntary wrist action, *Brit. J. Psychol.*, 14, 1923, 1 ff.

There is little doubt that objective records alone will not tell us everything that is going on in musculature during overt action. It is conceivable that in many instances and under a variety of conditions there will be tendencies toward movement strong enough to be perceived, as a result of proprioceptive stimulation, but not strong enough to change the direction of movement. Moreover, it is possible that complex movements, resulting from a combination of stresses and strains, yield objective records that could not possibly agree, on superficial analysis, with careful introspective reports. This pertains particularly to eye-tensions, where there are several sets of muscles functioning in a multitude of ways. While studies like Dodge's,[14] for example, are valuable for many purposes, they cannot be accepted as reliable evidence that kinaesthetic sensations from eye-muscles are illusory because no objective record is obtained.

At first blush the problem with respect to auditory imagery would seem more difficult but here again the complete introspective method may yield significant results. There are incipient strains characteristic of auditory attention. These may be in the throat or in the superficial muscles of the neck or both, and even in the chest. It is to be questioned seriously if the so-called 'surrogates' which 'accompany' auditory imagery are surrogates at all. Rather they are part and parcel of the experience called an auditory image and if the 'stimulus-error' be not committed too flagrantly they may turn out to be the most important part of the image. The majority of observers with whom the writer has been associated are either kinaesthetically or visually synaesthetic to a degree. The intensity, pitch, and volume of tone, even quality, is invariably perceived partly at least in terms of visual or kinaesthetic processes, and attempts to ignore the latter merely drive them into a synthesis in which the complex is perceived in unitary fashion as auditory. The peripheral cue, it would seem, is not hard to find in the kinaesthetic element of the complex or in the various eye-adjustments made in connection with the visual component and in a tendency to schematize and localize the sound. It is surprising how a naïve or stubborn observer will deny these associates and at the same time falter over his descriptions of the auditory experience. Until he discovers these so-called associates he simply cannot give an account of the imagery. Working under the theory of 'givens' or 'existentiality' a trained subject is likely to dodge the issue by saying: "I was immediately aware of the auditory quality as such." Then, perchance, he makes a complex out of it by enumerating its characteristics!

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[14] Raymond Dodge, Five types of eye-movement in the horizontal meridian plane of the field of regard, *Amer. J. Physiol.*, 8, 1903, 307-329; An experimental study of the ocular reactions of the insane from photographic records (in collaboration with Allen Ross Diefendorf), *Brain*, 31, 1909, 451-489; A mirror-recorder for photographing the compensatory movements of closed eyes, *J. Exper. Psychol.*, 4, 1921, 165-174.

In the case of olfactory and gustatory imagery there is again a wealth of visual and kinaesthetic associates which ordinarily passes unnoticed. The musculature of the nose may tighten; there may be slight shifts in the tonus or lie of the chest musculature, tongue movements, contractions at the back of the mouth and in the throat, eye-movements connected with visual imagery or with localizing the image in question, or what not. Likewise in dreams. We cannot be certain that because we remember only the visual, auditory and verbal contents of a dream, other components are not lost in the relatively undifferentiated experience.

The problem of identifying an image of a specific modality is of course a very difficult one. It is doubtful if much that is ordinarily regarded as imagery of a certain kind is really that kind of an image at all. Indeed there is very likely no pure imagery of any kind. The best one can do is to assume as analytical an attitude as possible and accumulate masses of data from the greatest possible number of subjects. Confusion may arise with embarrassing readiness in describing images that are elicited with great difficulty. A flashy and fleeting experience may possess the meaning cold, but when analyzed carefully there may be hardly any quality of coldness present, and the flashiness turns out to be a name for the visual component! The writer has known synaesthetic individuals who have lived for years unaware of the fact that their cutaneous images were more visual than cutaneous. The experiences meant cold well enough and in fact to them were cold.

Doubtless the so-called asynaesthetic individual will find himself to a degree in the same predicament with the exception that he can pick out the tactual feature more readily because the interpretive or recognitive phase of his awareness has become integrated with the tactual part of the complex as well as with the visual and other 'associates.' But when it comes to a definite description of this tactual component, as such, he is quite as much at a loss as the obviously synaesthetic individual. He inevitably uses the associates and certain of these are bound to be easily recognizable as sensory.

Indeed, our treatment of imagery confessedly makes of it a weak or partially aroused sensation.[15] But now we must not consider that, because of this, images depend upon peripheral stimulation exclusively. Even in the case of sensation it is plain that central integrations are essential. The fact that sensation is conditioned by peripheral stimulation does not make of it a peripherally aroused process and nothing more. Sensation is quite as much a central as a peripheral process. While this may seem to be a trivial point it is necessary, for the sake of clearness, that we carry the logic of our position to its conclusion. Proportionately there should be no more peripheral stimulation in the case of sensation than in the case of imagery. Conversely, we may doubt whether in the one case there is proportionately more central activity than in the other.

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[15] R. H. Wheeler, Outline of a system of psychology, *Psychol. Rev.*, 30, 1923, 157 ff.

We should not leave out of account the various abnormal mental phenomena which the critics of a total-integration theory are likely to mention as damaging evidence. How, for example, does post-hypnotic suggestion depend even in part upon peripheral stimulation? Certainly the person who made the original suggestion need not be present at the time the instructions are carried out. Let us suppose that a patient is told, under hypnosis, that he will arise from the dinner table while at dinner, put on his hat and coat, and leave the house. A possible source of peripheral stimulation has already been mentioned—the dinner table. Presumably it would not have functioned as the peripheral stimulus had there not been the proper nervous set. A total-integration theory takes this into account. It supposes, however, that nervous sets of any kind never result in action—indeed, do not exist—in the absence of an adequate objective situation. Any detail of the patient's environment at the time he fulfills the suggestion, duplicated in the situation in which the original suggestion was given, may, through a conditioning process, function as an adequate peripheral stimulus. We have no way of measuring the far-reaching influence of conditioned reflex phenomena, or the extent to which two apparently different situations contain stimuli in common. If it is true that every time we respond we are reacting to a total situation, including proprioceptive stimulation, it is bewildering to reflect upon the number of possibilities whereby an apparently irrelevant item may be playing an important rôle in behavior. Let a single contributing stimulus be represented twice, in two successive responses, and we have the possibility of the peripheral cue for which we may be hunting. The horse that shied apparently for no particular reason, while being driven along a country road, was responding to a substitute stimulus present in the original setting when a newspaper blew across the road.

We have touched upon only a few of the problems confronting the advocate of centrally aroused processes. Those who, in the earlier period of experimental psychology, upheld the innervation sense, relied upon superficially analyzed and misleading evidence. The theory that the *Aufgabe* and its determining tendency represent central innervation mechanisms rests upon a foundation equally questionable. The writer has already suggested,[16] upon the basis of introspective data, that a mental set is essentially a peripheral innervating mechanism the evidence for which lies in the presence of kinaesthesis. The *modus operandi* of the determining tendency is a definitization and elaboration of an initial and relatively undifferentiated motor adjustment, the stages through which the definitization and elaboration pass being dependent upon supplementary stimulation.

Let a subject attempt to hold a visual image before him and he finds that secondary expedients are necessary, such as moving the image about, changing its shape, using verbal suggestions and the like. In the voluntary control of movement one's attention is centered upon the object and upon his muscles. We are forced on every hand, therefore, to conclude that voluntary phenomena are quite as much peripheral as central.

The problem of attention, like the problem of voluntary control, should not detain us long. So-called motor accompaniments of attention have long been recognized. Why they were defined as accompaniments rather than as genuine phenomena of attention is traceable to the dogma of parallelism and the doubtful statement of fact that attention precedes the motor accompaniments. The latter declaration assumes that a process appears and is attended-to afterwards, a statement that is neither consistent with the facts nor logically pertinent. It is both in the adjustment to attend and in the act of attending that we find kinaesthesis. Every shift of attention involves motor adjustments. The evidence is well worth considering, therefore, that one of the essential conditions of attention is peripheral stimulation. Then, of course, if our previous suggestions with respect to perception and image are correct it follows automatically that attention is a peripherally as well as a centrally controlled adjustment.

Beginning with Plato, if not earlier, theories of memory have repeatedly been couched in terms of central traces or residues. From the time of Aristotle it has been customary to speak of association of ideas as if one idea literally evoked another. Either this implication has been forestalled by a guarded qualification to the effect that the real dynamics of the process is to be found in the brain, not in the idea, or the issue has been clouded by a reference to the subconscious. As a result of this long history a set of terms such as recall, revival, reinstatement, and the like, have attained the status of common usage, yet there is nothing in these terms that indicates the nature of the actual processes involved. Such a begging of the question is not necessary under the total-integration view. Images are incomplete sensations; ideas are incomplete perceptions. Association of ideas becomes a highly modifiable and easily inhibited seriation of sensory-motor reactions, one reaction furnishing a peripheral stimulus, through proprioceptive stimulation, for the next. The direction in which a course of ideas may lead obviously depends upon the interrelations of action patterns but we are at a loss to know how these patterns function in the absence of peripheral influences.

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[16] R. H. Wheeler, An experimental investigation of the process of choosing, *Univ. of Ore. Publ.*, no. 2, 1920, 1-59.

The problem of affection has played a prominent rôle in the development of cortical theories of mental processes. We can touch upon the matter only briefly. Suffice it to say that everything said up to date about affection can be said of kinaesthesis. It remains for more careful introspection to settle the question. It is claimed, for example, that affection lacks clearness.[17] So does a diffuse kinaesthesis or organic process or for that matter any mental process a complete cognition of which is prevented by an absence of the proper cues or setting. We are here involved in the distinction between attributive and cognitive clearness which we have already commented upon.[18] Affection may lack localization. So may kinaesthesis. We have qualitative opposites in affection, likewise in kinaesthesis—strain and relaxation. It has been suggested that affection may depend upon certain undifferentiated nerve endings in the body. These may be classified as kinaesthetic. Affection is characterized as subjective. What could be more subjective than a vague, diffuse and unlocalized kinaesthesis? We do not wish to imply that every experience of pleasantness and unpleasantness can be reduced exclusively to kinaesthesis. No complex experience can. Certain observers may find that pressure or some organic complex may constitute affection under certain conditions. It all depends upon the observer's mode of interpretation and upon the history of pleasantness and unpleasantness in his particular case. Moreover, there are many reports of affection that turn out to be nothing other than judgments of like and dislike, attitudes of acceptance and rejection, for the most part verbal and otherwise motor in character. These responses possess no content which, in any other setting, would mean pleasantness or unpleasantness. We are inclined to hazard the suggestion, therefore, that there is nothing known about affection that is incompatible with the thesis of this paper. The case of emotions is too obvious to require comment.

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[17] E. B. Titchener, *Text-book of Psychology*, 1909, 213.

[18] R. H. Wheeler, Some problems of meaning, this JOURNAL, 34, 1923, 196; Persistent problems in Systematic Psychology: II. The psychological datum, *Psychol. Rev.*, 32, 1925, 451 ff.

To recapitulate, we have raised the systematic issue in this paper whether centrally aroused processes exist and our negative conclusion rests upon indications from experimental work on the brains of animals, the circuit concept of the reflex arc, the continuous functioning of the sense organs, the prevalence of kinaesthesis in mental life, the principle of the conditioned reflex and the method of complete introspection, and the evidence that when we respond overtly or perceptually we are reacting to a stimulus-situation. We have brought to bear upon the problem the difficulty of describing functions in terms of fixed and stationary structures in such a way as to imply that the function is derived from broken continua, and the difficulty of implying that energy is created in or discharged from the brain without outside help.

The view here suggested assumes that integrations do not arise in the cortical centers but are functions of complete reflex circuits and of continuous peripheral stimulation from more than one sense modality, one of which, however, is always kinaesthetic. This view does not minimize the importance of brain centers but implies that they do not function independently of receptors and effectors. The brain is regarded as an amplifier and battery whose activities depend upon an open circuit. The sense organs are regarded as necessary for the supplying of those differentiated energy-patterns which result in the appearance of images of different qualities. Substitution of stimuli and conditioned reflex phenomena, operating especially through the medium of proprioceptive stimulation are suggested as the conditions for the time of appearance, the duration, form and 'behavior' of images. Because all such integrations as presumably subserve mental life in any of its forms are neuro-muscular integrations, based upon complete reflex circuits, we have called our hypothesis a total-integration theory of mental processes. It is strictly opposed to the prevailing conception that certain mental processes are cortically or centrally aroused.

THE INTELLIGENCE FACTOR IN THE SOLUTION OF SPACE PROBLEMS WITH THE TWO STORY MAZE

By Catharine M. Cox, Stanford University

I. Problems and Approach

Is the adequacy of adjustment or procedure in situations involving new motor space experiences conditioned to a greater extent by imagery type or by intelligence? What is the rôle of each in solving problems in motor space? The two-story maze offers an opportunity for the observation of individual differences in methods of attack on motor space problems.[1] Variations in time and error scores and in cue pattern on a series of five motor space problems have been observed, and in connection with the gathering of these data the parts played by visual and kinaesthetic factors have been examined to some extent. Learning was not primarily involved in the problems presented, but by reason of the serial method of presentation the learning factor inevitably entered in.

The extent to which the intelligence of the *S*s conditioned the building up of successful procedures, and the relation of intelligence to (1) speed in solving double-maze problems, (2) number of errors, (3) type of cues and imagery, and (4) number of components in a cue scheme, have been considered and some results are available on these points in answer to our double question.

Apparatus. The Miles two-story duplicate maze has been found peculiarly adapted to the investigation of motor space procedure. It presents a situation in which visual and kinaesthetic experience are combined, but not in the usual manner, for here visual guidance is indirect. The proportion of the relative participation of visual and kinaesthetic cues may here be altered from problem to problem by changing the position of one of the mazes. The same maze pattern is retained throughout the experiment. The apparatus consists of a combination of two identical stylus mazes set one directly above the other The upper one is seen and traced with one hand, while the lower one is traced by the other hand out of view. In Plate I an *S* may be observed in position at the maze. The picture was taken from the position of *E*.

The motor space problem series. Five positions of the lower maze, the upper remaining fixed, formed the series of problems presented for solution to 67 university students, members of a class in elementary psychology.

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[1] Walter Miles, The two-story duplicate maze, *J. Exper. Psychol.*, 10, 1927, 365-377.

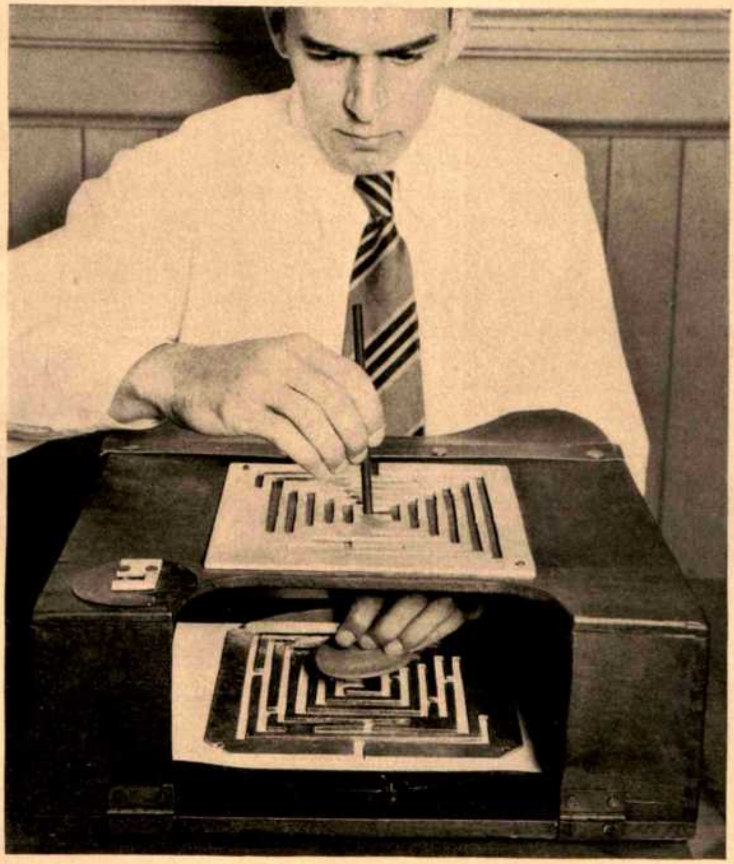

PLATE I. AN S SOLVING PROBLEM 2 ON THE MILES TWO-STORY DUPLICATE MAZE

Problem 1. 0°. The lower maze in register, *i.e.* in the same position as the upper (visible) maze.

Problem 2. 180°. The lower maze revolved half way, so that the side that was nearest the *S* is now farthest from him.

Problem 3. 90°. The lower maze is turned one quarter so that its paths are at right angles to their original position and to those of the visible maze.

Problem 4. 180°. (reversed) The maze is turned over on its face so that the side nearest the *S* is now farthest from him, and the maze offers a mirror presentation of the pattern.

Problem 5. 90°. (reversed) From the position as in Problem 3, the maze is turned over on its face. Also a mirror position.

This series was not intended as a learning experiment, but the learning factor entered in largely in proportion to the length of time required by the *Ss* to solve the problems. Those whose method of procedure was sufficiently successful (and adequately combined with motor quickness) completed the whole series so promptly that the learning factor was slight. Those who proceeded slowly on the first problems learned, or partially learned, the maze pattern, and so their performance on the later problems was proportionately faster.

In the course of the investigation striking differences appeared: (1) in the time required for the solution of the various problems in the series; (2) in the type of motor-sensory guidance which the *S* believed he was most dependent upon, and in the type of his imagery; and (3) in the extent to which he employed logical or rational processes or guides, and the nature of these. In another place these three kinds of differences are discussed for the group as a whole, and the time scores for two sub-groups, the two sexes, are compared.[1]

Variations in all three of the aspects of behavior observed in this motor space problem situation, *i.e.* differences in the time scores, in motor-sensory cues and imagery, and in rational analysis, brought up the question as to the extent to which the presence and influence of the variable factor of intelligence was operative.

The Intelligence Element. The purpose of the present study therefore has been to discover the relation of intelligence to procedure in a series of five motor space problems.

The intelligence of our *Ss* was measured by score on the Thorndike college entrance test. A further criterion of ability was also available: the term grade of each of the *Ss* in elementary psychology. On the basis of the Thorndike scores the *Ss* were classified into three groups. The behavior of these groups described in terms of procedure in the maze problem situation and certain characteristics of the three intelligence levels are presented. Time and error scores are compared with intelligence rating, and the types of imagery and other cues employed by the different intelligence groups are considered.

II. OTHER STUDIES

A number of investigators have considered the effect of intelligence (1) in motor space problem learning, and (2) in problem solving of various types, but not, so far as the writer is aware, in motor space problems like the present ones that are not primarily problems in learning. Gould and Perrin concluded from their study of the maze learning of human adults and children that behavior in the initial attack on the maze problem gave "a graphic representation" of intelligence measured by chronological age.[3] The adult of 21 they found superior to the child of 11 in learning the pencil maze, when scores on errors, time, and distance were compared. But they concluded further, that "all distinctions found to hold between the child and the adult are extremely relative. Some of the children seemed to compare in every respect with the normal average adult" and the reverse was also found to be true. Hicks and Carr compared three intelligence levels, those of rats, children, and adults.[4] They found that certain features of the numerical expression of the learning processes in the maze could be isolated to serve as indices of intelligent behavior. An inverse relation appeared when degree of intelligence as shown by the three groups was compared with surplus time and error elimination.

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[1] C. M. Cox, Comparative behavior in solving a series of maze problems of varying difficulty, *J. Exper. Psychol.*, 11, 1928, 202-218.

Gopalaswami found that the success of 10 *S*s in learning the solution of new motor space problems correlated with teachers' estimates of intelligence, 0.81 in one case, 0.05 in another.[5] (The author notes that the groups are too small for correlations to be particularly significant). The problem was mirror tracing of the star pattern: in the first instance the usual simple method was used, in the second an apparatus was employed that eliminated the *S*'s knowledge of his errors. Learning in this case showed practically no correlation with estimated intelligence.

Hunter compared maze performance with scores on the Otis intelligence test for 59 *S*s.[6] Various measures of time (total time, time of one trial minus another, time of each of ten tenths of all of the trials), measures of total trials, and total errors were correlated with Otis scores. All of the correlations were negative, thus showing correspondence between speed in movement, speed in learning, and brightness. The correlations range from -0.02 ± 0.10 (intelligence and time on the 10th tenth) to -0.45 ± 0.08 (intelligence and total time). Fairly comparable with the present study (because they indicate the relation of intelligence to initial problem attack) are the following correlations on early performance in maze learning: intelligence and time of the first trial, $r = -0.22 \pm 0.06$; intelligence and time of the best of the first three trials, $r = -0.33 \pm 0.087$. Total time appeared to be the best single indicator of ability ($r = -0.45 \pm 0.08$), considerably exceeding total trials ($r = -0.17 \pm 0.094$) and somewhat exceeding total errors ($r = -0.34 \pm 0.087$) in correlation with Otis score.

The four investigations referred to are illustrative of those primarily concerned with the relation between maze *learning* or puzzle solving and intelligence, and yet they have thrown, as well, some clear light on the question of the relation between intelligence and *attack* on a maze problem, *i.e.* procedure in the early trials before learning is largely involved. Particularly significant for us is the finding that behavior in the early trials did give a 'graphic representation' of intelligence and that the time on early trials as well as total time, showed a definite negative correlation with the intelligence score.

A second group of studies that concern us are those which consider the relation of intelligence to problems of various sorts that are not primarily learning problems. From investigators of performance tests of intelligence we have Pearson's correlations between group intelligence scores and time scores on puzzle problems. Somewhat comparable to our problem series in that they present novel puzzle situations are the Ruger wire puzzles, successful solutions of which were found by Camp Dix examiners to correlate 0.37 with Army Group examination Alpha.[7] Porteus' pencil mazes (Camp Dix results) correlated 0.46 with Alpha.[8] Correlations between puzzle problems and individual intelligence test scores (mental ages), or with performance test scores, are often considerably higher. The Porteus maze is an example of a single performance test that correlates as high as 0.46 with a group test of intelligence (the correlation between Porteus and Stanford Binet was 0.66, Army results), and has a distinct contribution to make to a more extended performance test scale (as in Beta).[9]

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[3] M. C. Gould and F. A. C. Perrin, A comparison of the factors involved in the maze learning of human adults and children, *J. Exper. Psychol.*, 1, 1916, 122-154.

[4] V. C. Hicks and H. A. Carr, Human reactions in a maze, *J. Animal Behav.*, 2, 1912, 98-125.

[5] M. Gopalaswami, 'Intelligence' in motor learning, *Brit. J. Psychol.*, 14, 1924, 274-290.

[6] W. S. Hunter, Correlation studies with the maze in rats and humans, *Comp. Psychol. Monog.*, 1, 1922, 37-56.

The Miles double-maze test has not been standardized as a performance test of intelligence, but the results obtained in the present study suggest that one of the 5 problem positions with a preliminary practice position might make an interesting and serviceable addition to the available group of performance tests.

III. INTELLIGENCE SCORES OF *S*s

During the progress of the experiment the Thorndike scores were not known to *E*. They were not considered until after the other data had been evaluated. The Thorndike scores were found to cover a range of 70 points from the lowest score, 37.29 to the highest 109.90. The mean score was 72, median 72, standard deviation 16.50.

TABLE I

CORRELATION OF INTELLIGENCE SCORES WITH TIME SCORES ON MOTOR SPACE PROBLEMS

Problem	Correlation Coefficient
1, 0°, register	−0.26 ±0.06
2, 180°, one-half revolution	−0.59 ±0.05
3, 90°, one-quarter revolution	−0.40 ±0.07
4, 180°, reversed	−0.11 ±0.09
5, 90°, reversed	−0.09 ±0.09

IV. INTELLIGENCE, TIME AND ERROR SCORES

When individual time scores on the separate problems were correlated with Thorndike scores, the results in Table I were obtained.

The first three positions show a fair negative correlation between slowness and brightness. It may be seen that the time score on Problem 2 appears to compare favorably, as an inintelligence measure, with some performance test problems in general use. The five correlations are all obviously influenced by factors of difficulty and position of the problem in the series.

Each of the five motor space problems presents a different situation as far as intelligent behavior is concerned. We have attempted to analyze each from this angle, and to point out some of the elements common to two or more of the problems that do not appear in the others. Table II, a table of correlations, shows these comparisons.

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[7] Psychological examining in the United States Army, *Mem. Nat. Acad. Sci.*, 15, 1921, 363.

[8] *Loc. cit.*

[9] *Op. cit*, 404.

Problem 1. The two mazes are in exactly the same rotation position. One might expect this problem to be too simple to bring out differences in intelligence in college students. Probably acuteness of visual imagery, general liking or disliking of puzzles, simple manual dexterity, the kind of simple coördination in similar movements common in many every-day activities are measured here rather more than intelligence. Still there is a low but significant agreement between speed and brightness. (Average time for 67 *S*s equals 96 sec.)

TABLE II

INTERCORRELATIONS OF TIME SCORES ON FIVE SPACE PROBLEMS

Problem	Problem				
	1	2	3	4	5
	(67 cases)	(67 cases)	(64 cases)	(59 cases)	(55 cases)
1	—				
2	0.47±0.05	—			
3	0.17±0.07	0.39±0:07	—		
4	0.46±0.07	0.43±0.07	0.34±0.07	—	
5	0.02±0.09	0.10±0.09	0.30±0.08	0.11±0.09	—

Term grade in Psychology and Problem 2, r = −0.25±0.06.
Term grade in Psychology and Thorndike score, r = 0.50 ± 0.06.

Problem 2. Here the lower maze is turned half-way round. This position might be expected to test intelligence more definitely than that of Problem 1. A reasoned method tended to produce a successful procedure in this case. The lack of a rational plan left the *S*s no other method than trial and error. Apparently the time at which a logical or rational method began to control the procedure depended upon the grade of intelligence of the *S*. This problem seems to measure the same qualities enumerated for Problem 1; in addition it requires quick analysis of a more unusual situation, but one that is not infrequently met in every-day life. The similarity of 1 and 2 appears in the correlation of their time scores (+0.47 ±0.05).

Problem 3. A quarter turn. At this point the influence of learning probably begins to make itself felt, especially in the case of some of the *S*s who were slowest on Problem 2. For the individual who reasons, a quarter-turn is more difficult than a half-turn (Problem 2), or than a half-turn reversed (Problem 4). We are not accustomed to thinking in terms of quarter-turn adjustments. Visual imagery when available, probably aids reasoning at this point. The *S* who gets along as best he may by 'trying' or 'feeling it out' may 'know' the maze better from his long experience with Problem 2. He may therefore perform this really more difficult task in even less time than he required for 2. The relation between 3 and 1 or 2 has then two contradictory elements: (a) for the reasoner, 3 is quite a different problem from, and a much harder one than, 2 or 1; (b) for the kinaesthetic learner the problem is similar to 2 and 1 and easier than these because of learning through practice on them. Inasmuch as most individuals are partly reasoners and partly kinaesthetic learners, these two contrary principles may be assumed to be at work in greater or less degree in all cases. As a logical problem, 3 is less like 1 than 2. The correlation of 1 and 3 shows this dissimilarity increased perhaps by the learning element in 3 (+0.17±0.07), while 2 and 3 give clearer evidence of their relationship, (+0.39±0.07).

Problem 4. After a half-turn the maze is turned on its face. This position is more like 1 and 2 than it is like 3 or 5. It demands the same analytical response as that which proved effective in 2, but here even more than in 3 learning enters into the results, simply because of the position of this problem so late in the series. We may, therefore, expect some of the *S*s who spent more time on 2 and 3 to solve 4 more quickly in proportion than other individuals do who are proceeding by a more rational method. Because of its essential characteristics as a motor space 'maze problem' 4 correlates positively with 1 ($r = 0.46 \pm 0.07$), 2 ($r = 0.43 \pm 0.07$), and 3 ($r = 0.34 \pm 0.07$); because of its greater similarity to 1 and 2 the time scores in these cases agree more closely.

Problem 5 After a quarter-turn, the maze is turned on its face. This problem is the hardest of the five for rational solution. It is like 3 in the awkwardness and unfamiliarity of the quarter-turn situation. But in its position in the series the element of motor space learning is pronounced, especially for the slowest individuals, and this obscures the effect of the rational difficulty of the problem. A number of the slowest workers have dropped out leaving contrasts in time not so great as they would otherwise probably be.

Problem 2, preceded by Problem 1, is the most satisfactory as a measure of general ability as scored on the Thorndike test ($r = -0.59 \pm 0.05$). Thorndike score and term grade in elementary psychology correlate $+0.50 \pm 0.06$. Time scores on Problem 2 appear to measure in large part something different from the ability necessary to succeed in elementary psychology as measured by term grade ($r = -0.25 \pm 0.06$).

There are two technical factors which, beside unrelatedness of function, make for low correlations between intelligence and speed in this experiment series. First, the fastest workers can not go much faster by reason of learning. A certain amount of time is required to go through the motions; even knowing the maze completely will not solve it in 'no time.' Hence as learning progresses the time scores tend to bunch at the lower end of the scale. Secondly, the continuous dropping out of the slowest cases in Problems 3, 4, and 5, and the fact that all who required more than 15 min. were scored equally at 900 sec. reduces the averages. The correlation of intelligence score and average time for all five problems (using only the 36 cases in which the series was completed and omitting all 900-sec. scores), gave -0.39 ± 0.09. It is evident that the group of 36 is highly selected and made up of better records only. If all of the 67 *S*s had been allowed to complete all of the problems the correlation would probably have been higher.

Intelligence Scores. In order to bring out contrasts among different intelligence levels in time scores, cues and imagery, rational or logical procedure, the *S*s have been divided into three intelligence groups as follows:

Group I. *S*s with Thorndike scores more than 1 S.D. above the mean (14 cases, av. score 96.64 ± 6.10). See Fig. 1 for individual time scores.

Group II. *S*s with intelligence scores between 1 S.D. and −1 S.D. (41 cases, av. score 70.50 ± 7.62). See Fig. 1 for the time scores of 12 cases from this group; selected at random.

Group III. *S*s with intelligence scores more than 1 S.D. below the mean. (12 cases, av. score 48.45 ± 4.8). See Fig. 1 for individual time scores.

Time scores. The figures in Table III show that the three ability groups average in their time scores in each of the five maze problems in the same order as they register in the Thorndike test (see also Fig. 2). It may be observed, however, that the order of difficulty of the problems for the three intelligence groups is not the same. Group I follows what is probably the actual order of difficulty if the learning factor were eliminated (1,2,4,3,5). Group II shows two modifications of this order. Problem 2 is on an average not as intelligently attacked by the *S*s of average intelligence as by Group I, and the time required is therefore longer. The same is true of the procedure of Group II in Problem 3. By the time the *S*s reach Problem 4 they have spent almost as much time (921 sec.) as Group I spends on the entire series (1013 sec.); the maze pattern by this time is to some extent *learned* and so Problem 4, which is really harder than Problem 2, is solved in less time. Similarly, Problem 5 is solved in less time than Problem 3, which is undoubtedly harder. Group III shows the order of difficulty even more disturbed by the learning factor introduced by slowness (1,4,5,3,2). Work on the next to the easiest problem—Problem 2—gives these *S*s so much experience with the maze pattern that the far harder Problem 3 is more quickly solved than Problem 2; the hardest problem of all—Problem 5—is done in

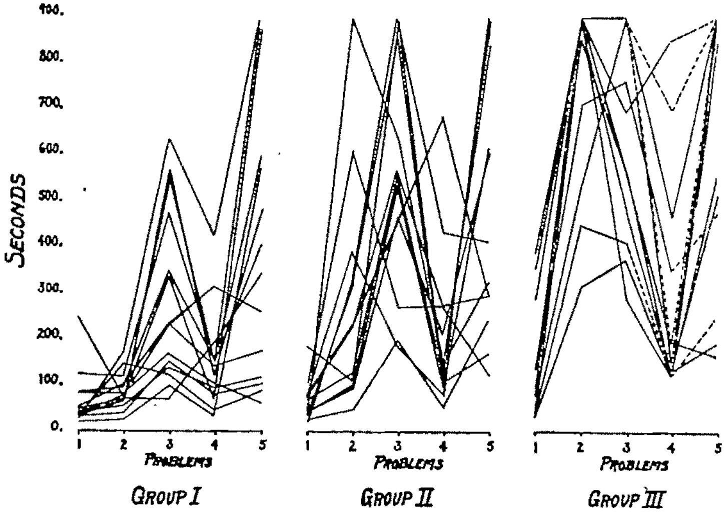

FIG. 1. INDIVIDUAL TIME RECORDS ON MAZE POSITIONS FROM THREE COLLEGE INTELLIGENCE GROUPS. GROUP I, SUPERIOR, GROUP II AVERAGE, GROUP III INFERIOR INTELLIGENCE

less time than the two easier problems (Problems 2 and 3); and Problem 4, which is harder than Problem 2 requires on an average less than one third as long for solution.

In spite of the agreement between intelligence group placement and time score the variability of individual scores is large within each of the three groups (see Fig. 3). The probable correctness of the difference between the averages of the groups has been computed by Boring's method and formula to indicate the 'correctness' of the means.[10] The difference between each two means so measured is greater than six times the probable error of the means. This gives an index of 1.00 for the probable correctness of the position of the means of the three groups, *i.e.* they are 'certain.'

TABLE III

AVERAGE TIME SCORES (IN SEC.) ON 5 SPACE PROBLEMS FOR THREE INTELLIGENCE GROUPS

Intelligence Group	Problem				
	1	2	3	4	5
I	55	84 (±7)	318	137	419
II	94	338 (±29)	489	237	431
III	138	763 (±39)	595	255	462
Av. total no. cases	96	361	463	217	432

The degree to which each group overlaps the median of another has also been determined for one of the problems (Problem 2) by Kelley's method[11] of measuring overlapping in which the reliability of the measure is taken into account. We find an index of 1.00 in five of the six comparisons; Group III does not overlap the median of Group II or Group I, nor is the converse true; Group I does not overlap the median of Group II. Eight percent, however, of the cases in Group II overlap the median of Group I. The percentage of each group whose time score exceeds a given number of seconds is given graphically in Fig. 3.

TABLE IV

SPEED OF MOVEMENT AT THREE LEVELS OF INTELLIGENCE

Intelligence Groups	Rapid movers		Normal or slow movers	
	%	No.	%	No.
I	50	7	50	7
II	42	18	58	23
III	24	3	76	9
Total	42	28	58	39

The time scores indicate speed or slowness in reaching the exit. Speed of movement has been distinguished from speed as registered in time score. Those who move rapidly have been differentiated from the group of normal and (a few cases of) subnormal speed. The results in Table IV appear for the three intelligence groups.

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[10] E. G. Boring, On the computation of probable correctness of differences, this JOURNAL, 28, 1917, 454-459.

[11] T. L. Kelley, Measurement of overlapping, *J. Educ. Psychol.* 10, 1919, 229-232.

Thirty-nine *S*s did not move faster than a fairly moderate rate. About 5 of these might have been classified as slow movers; sluggish in every motion, making long pauses between moves. The most intelligent group was divided equally between the normal and the rapid rate. Group II showed the same proportion of fast workers as the group as a whole, *i.e.* just less than half were fast. Of the dull group one quarter were fast. One of these was probably the quickest of all the 67 *S*s as far as motion alone was concerned, but the motions were trial and error with very little evidence of learning by experience.

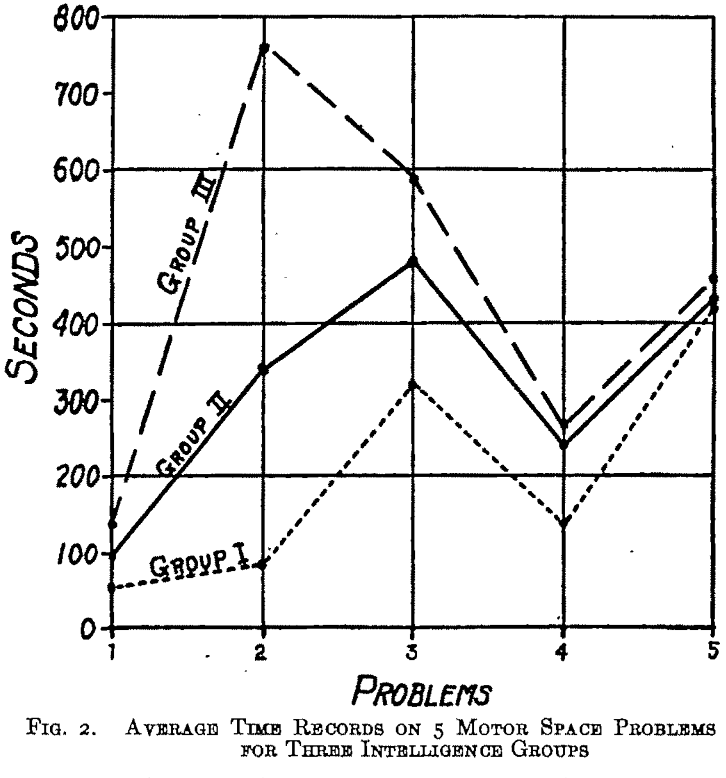

FIG. 2. AVERAGE TIME RECORDS ON 5 MOTOR SPACE PROBLEMS FOR THREE INTELLIGENCE GROUPS

Error Scores. The amount of error is significant in connection with the time score. In comparing the three intelligence groups with respect to errors the following classification into error groups has been used: 0 is the index of performance freest from error; 1 means some error largely confined to one part of the maze; 2 means some error in various parts of the maze; 3 means much error throughout. Errors could and did

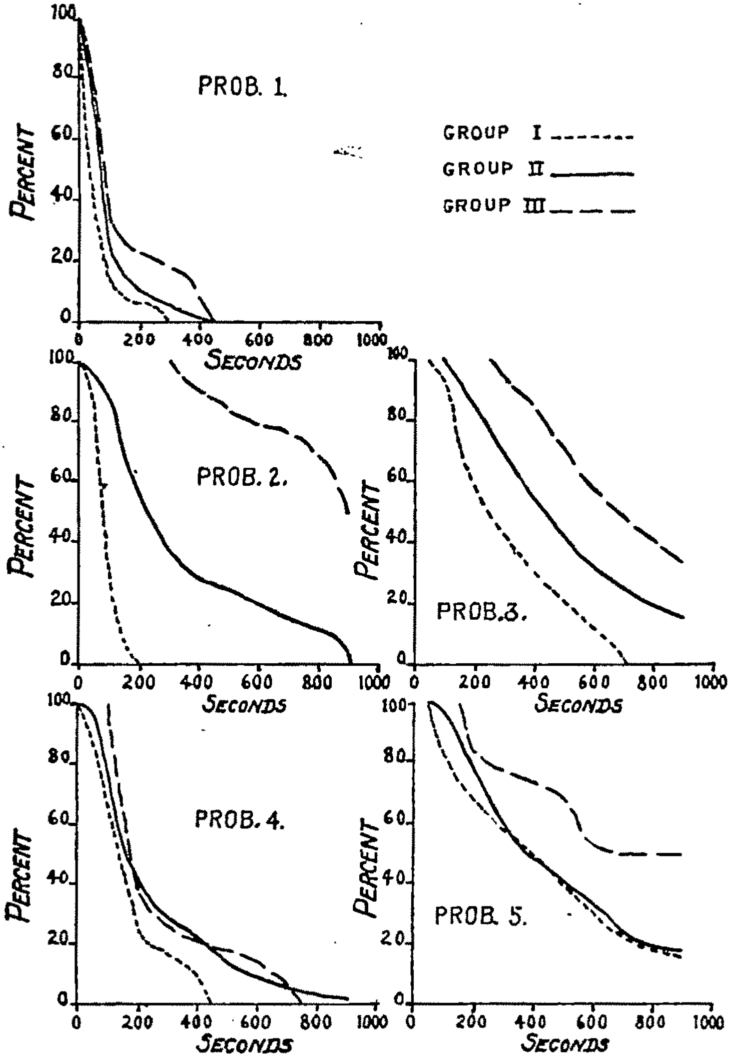

FIG. 3. GROUP TIME RECORDS ON 5 MAZE POSITIONS. THE PERCENTAGE OF EACH INTELLIGENCE GROUP WHOSE SPEED EXCEEDS ANY GIVEN NUMBER OF SECONDS

result even when the *S*s were using intelligent plans of procedure. These ratings are given in Table V, and are, of course, subject to the constant error of a single judgment and the unreliability of results based on small groups. Still they are as obviously indicative of a general trend.

TABLE V

ERRORS IN MAZE PROBLEMS AT THREE LEVELS OF INTELLIGENCE

Intelligence Groups	Error Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	14	2	29	4	36	5	21	3
II	55	23	33	13	10	4	2	1
III	83	10	17	2	0	0	0	0

Group I has 21% of its members in the class practically free from error, 36% in the class of few errors, 29% in the class of many errors, and is represented by 2 cases—14%—in the worst class of all. Group II has only 2% in the class free from error, 10% in the slight error class, 33% in the many error class, and 55%—more than half—of all its cases in the worst class of all. Group III has no cases in the first two classes, almost 17% in the third class, and the remaining, 83%, are in the worst class. Some individuals scoring highest in intelligence score among the poorest in number of maze errors, but of those who score low in intelligence there were in the present experiment none whose performance was as free from error as 57% of the superior and 15% of the average *S*s.

V. INTELLIGENCE AND METHODS OF PROCEDURE, IMAGERY, AND OTHER CUES

Intelligence and trial-and-error procedure. The extent to which trial and error governs procedure has been rated for each individual performance in the series of experiments (see Table VI).

Degree of incoördination (the extent to which the two hands operated independently in time, but more especially in direction), repetition of the same errors, failure to push toward the outside when other schemes fail, and the extent of occurence of random, apparently unreasoned movement, have been considered in making the rating. Index 3 means that behavior was best described simply as "trial and error;" Index 2, lapse to trial and error at least half of the time; Index 1, occasional lapses to trial and error; Index 0, absence of trial and error, evidence that behavior was rationally motivated in all of the problems.

Two cases of Group I fell into trial and error occasionally, and 2 cases at least half of the time, the 10 remaining cases (72%) appeared to show that behavior was reasoned throughout. Group II had one case free from trial and error (2%), 14 cases (35%) who lapsed occasionally, 17 cases (41%) whose performance was at least half of the time of the trial-and-error type, and 9 cases (22%) trial and error throughout. Group III had no cases free from trial and error, one case (8%) almost free, 5 cases (42%) at least half trial and error, and 6 cases (50%) whose performance was trial and error throughout.

TABLE VI

TRIAL AND ERROR AT THREE INTELLIGENCE LEVELS

Intelligence Groups	Trial-and-Error Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	0	0	14	2	14	2	72	10
II	22	9	41	17	35	14	2	1
III	50	6	42	5	08	1	0	0

Intelligence and imagery. All of the *S*s in all three Thorndike groups (with one possible exception, a member of intelligence Group III) made use of visual guidance in solving the maze problems. Similarly, kinaesthetic guidance was apparently in every case an element in solution. The typical situation was that in which the *S* 'looked at' the upper maze and 'felt' the lower maze. In Table VII each individual has been arbitrarily assigned to one imagery classification or another, on the basis of *his statement* of the type of imagery that prevailed in his case. Perhaps a method of rating the *extent* to which each type of imagery was used would have given a more adequate characterization of procedure. The more intelligent *S*s probably did depend on imagery to a greater *extent* and *more effectively* than the less intelligent.

TABLE VII

PREDOMINATING TYPE OF IMAGERY REPORTED AT THE THREE LEVELS

Intelligence Groups	Visual		Kinaesthetic		Mixed	
	%	No.	%	No.	%	No.
I	14	2	50	7	36	5
II	32	13 (1 doubtful)	46	19 (2 doubtful)	22	9
III	25	3	33	4 (1 doubtful)	42	5
Total	27	18	45	30	28	19

Of the entire group of 67 almost half (45% were more aware of 'feeling' their way than of 'seeing' the path, 18 (27%) were more conscious of the visual guidance, and 19 (28%) thought 'feeling' and 'seeing' were equally important to them. The more intelligent *S*s (Group I) were more apt to consider the kinaesthetic imagery as more important (50% kinaesthetic);

the average *S*s (Group II) 19 cases (2 doubtful) (46%) believed they had depended on kinaesthetic imagery; and 4 cases (1 doubtful) (33%) of the inferior group (Group III) were conscious of kinaesthetic aids.

The difference between the superior and the inferior group in kinaesthetic guidance seems significant, and agrees with the observations of the extent of trial-and-error performance in these groups. The fact that only 14% (2 cases) of the high testing individuals, 32% (13 cases, one doubtful) of the average (Group II), and 25% (3 cases) of the inferior group (Group III) thought of the predominance of visual imagery seemed at first surprising. It may perhaps be explained by the greater attention of the more intelligent individuals to a less usual aspect of experience (the kinaesthetic) because it proved to add to the visual cues and imagery, which were mentioned by all, a fairly dependable guide in the double maze situation.

TABLE VIII

USE OF THE STARTING KEY AT THE THREE LEVELS

Intelligence Groups	Percent of group who used key		disregarded key	
	%	No.	%	No.
I	56	8	44	6
II	47	19	53	22
III	17	2	83	10
Total	44	29	56	38

In connection with visual guidance mention should be made of the use of the 'key.'[12] Effective application of this visual guide probably depended to a considerable extent on the ability of the individual to make a rational or systematic plan for procedure to which this was the one and only cue. Twenty-nine *S*s—just less than half the total group—were observed to use the 'key' as a guide or stated that they had used it. Their distribution in the three Thorndike groups is given in Table VIII. These made up more than half of Group I, about half of Group II, and a sixth of Group III. Here the two higher testing groups do not differ greatly, but the low testing individuals seem far less apt to avail themselves of this device.

(*a*) *Introspective reports.* In *E*'s notes were recorded, throughout the series, the types of procedure followed. The *S*'s introspective report given at the close of the experiment usually included mention of fewer cues and methods than had probably actually been followed. Sometimes it included those that had not demonstrably affected the procedure at all. Where the guidance of a certain principle was definitely stated, and if the effort to follow it had been obviously present even if only in part, it was credited to the individual and to the group.

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[12] The 'key' is a small replica of the central part of the maze. It is fastened to a small round disk attached to the top of the maze box (see Plate I). Rotated to the position of the center of the lower maze in each problem position it could serve as a cue to the starting direction. The key could not, however, be turned over on its face.

(*b*) *Reasoned methods of maze problem solution.* In addition to the general visual-motor activity and imagery, characteristics of the response-behavior included rational plans containing specific cues, and abstract theories of procedure. Considerable disparity was shown in the *extent* to which plans were used. The behavior of the *S* and the corroboration or contradiction of his introspective account are considered in the ratings on the degree to which a theory or logical scheme directed movement. In Table IX, Index 3 means a definite guiding plan evolved and, in the main, adhered to; Index 2 means evidence of a plan functioning more than half of the time; Index 1 means evidence of a plan whose operation could at times be discerned in the *S*'s movements in the maze; Index 0, no logical plan in evidence at any time.

TABLE IX

FORESIGHT AND PLAN

Intelligence Groups	Plan Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	86	12	7	1	7	1	0	0
II	10	4	41	17	41	17	8	3
III	0	0	17	2	75	9	8	1

Of Group I, 86% (12 cases) gave evidence of a definite plan, in the main operative. No case in this group was entirely planless. Of Group II 10% (4 cases) appeared to have a definite plan and 8% (3 cases) were apparently planless. Half of the remaining cases in the group had a plan that functioned half of the time; half appeared to attempt to follow a plan, but with less success. Group III had no case with a definite plan consistently operative. Two cases (17%) had plans that functioned half of the time. Nine cases (75%) had plans that were at times discernible in action and one case that showed no evidence of plan. 'Planfulness' and Thorndike score appeared to show some agreement in the group comparisons; there were no cases in Group III as planful as those in Group I, and none in Group I that were planless.

Intelligence and other cues. (*A*) *Concrete plan elements.* Some of the specific concrete elements involved in the schemes could be separately recognized and may be so discussed. These include (1) the general notion of movement toward the outside of the maze; (2) the use of excessive pressure on the outer edges of the alleys; (3) methods for recognizing and escaping from the complication at opening 5.[11]

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[11] Opening 5 is the only passage through which the true path leads inward instead of toward the outside of the maze.

(*1*) *The use of the cue of direction of movement toward the outside.* In the present series of experiments the object as stated is to "get out," that is, to reach the exit. Some *S*s, even among those who plan their procedure with foresight, seem never to happen on the scheme of pushing along outside edges rather than inside in seeking alley outlets. Other individuals would succeed in getting out by use of this principle of direction alone were it not for the complication at opening 5. Even so, the chances are in favor of correction of the error at 5, for the *S*s who direct their movements outward are quickest to discover their error, if the equal probability carries them, after they have passed 5 on the wrong side, back through 4. They discover their error because the path lengths are, of course, shorter as the center is approached.

TABLE X

INTELLIGENCE AND MOVEMENT TOWARD THE OUTSIDE OF THE MAZE

Intelligence Groups	Index 3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	7	1	79	11	14	2	0	0
II	10	4	45	19	25	10	20	8
III	0	0	25	3	35	4	40	5

The *S*s have been classified (Table X) into four groups according to the extent to which they use the principle of movement toward the outside. Those who use the principle consistently as a basis for solving the problem are given the Index 3. Those who use it at least half of the time are rated 2; those who use it occasionally when other methods fail are rated 1; those who use it not at all are rated 0.

Group I predominated in the percentage use of this method, Group II was second, and Group III third. In each index class the order of the groups was preserved except in the first where Group I with 1 case (7%) is exceeded by Group II with 4 cases (10%).

TABLE XI

Intelligence Group	Excessive pressure		No excessive pressure	
	%	No.	%	No.
I	14	2	86	12
II	12	5	88	36
III	33	4	67	8

(*2*) *The use of excessive pressure toward the outside.* A number of *S*s who had the notion of moving the stylus along the outer edge of the paths seeking outlets were not content simply with feeling their way by keeping close along the wall. So intent were they on missing no gate that they pushed with tremendous force, at times actually pressing the maze walls out of position. The percentage distribution of those from the three intelligence groups who used excessive pressure is shown in Table XI. The two upper groups (I and II) appeared less apt (14% and 12% respectively) to use excessive pressure, than the inferior intelligence group (33%). The method appeared to be in the nature of an overdoing of the previous method.

(*3*) *The degree to which the contradictory character of opening 5 was recognized, and the recognition acted upon.* *S*s who used the indirect visual guidance of the upper maze with comprehension, noticed the inward passage at opening 5, and tried to direct their movements accordingly. Only one individual succeeded in consistent avoidance of error from start to finish. Index 3, in Table XII, means recognition of opening 5 in the first experiment and consistent avoidance of error at that point. Index 2, recognition of the fact that an inward passage existed and effort made to overcome the difficulty when met. Index 1, recognition of the nature of a recurring difficulty and escape from it with difficulty, but apparently as the result of a scheme. Index 0, no apparent awareness of opening 5; escape finally due to trial and error. The intelligence groups, in the main, fall in their order of brightness in distribution on the basis of awareness of the recurring difficulty. Group I shows the largest percentage of conscious recognition of this factor, Group III the smallest.

TABLE XII

RECOGNITION OF THE INWARD PASSAGE AT OPENING 5

Intelligence Groups	Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	7	1	49	7	22	3	22	3
II	0	0	22	9	29	12	49	20
III	0	0	8	1	0	0	92	11

(*B*) *Abstract plan elements.* In most of the performances a type of procedure was observed, at least part of the time, that indicated logical and reasoned efforts to work the space problems systematically on the basis of a thought-out plan. This procedure appeared in the form of coördination of movement between the two hands. From the *S*s' own reports it appeared that the observed coördination was the result of any one of three theories of procedure. (1) A theory that the moving of the two hands simultaneously would offer a basis for checking on position; (2) a theory that moving the hands in response to certain verbal formulas should achieve the desired success (such formulas were "upper hand backward, lower hand forward," or "hands at right angles and outward"); (3) a direction of attention to the relation of the lines but without definite formulation of 'direction.' In many cases, as later appeared, the coördinated movement was a combined result of two or all of these tendencies.

(*1*) *Coördination of movement of the two hands.* Eleven *S*s moved the two hands simultaneously and in the corresponding paths with only slight occasional slips. Nineteen *S*s showed coördination most of the time. Twenty *S*s showed some effort to coördination at the beginning of each problem, but later lapsed into independent movements of the two hands. One *S* completely dissociated the performance in the two mazes by tracing completely the upper maze before attempting the lower one at all. Fourteen *S*s moved the two hands at the same time but without correlating direction or amount of path covered. These 14 and the one unique case are rated as showing no coördination. In Table XIII, Index 3 means definite and precise coördination, practically throughout; Index 2, tendency to coördinate with occasional lapses followed by recovery of coördination or successful exit from the maze; Index 1, partial coördination, lapses not followed by recovery, tendency to coördinate movement in a certain area, but not in every movement; Index 0, no coördination—hands moved quite independently of one another. In each index class the three groups follow in percentage of coördination in the order of their intelligence. Group I uses coördination to the greatest extent; Group III to the least.

(*g*) *Other cues and plan elements.* In the introspective reports a miscellaneous group of cues were mentioned. Some of them were elements included in the plans discussed above, while others were more inclusive than our classifications and have been split up into the parts analyzed in the previous paragraphs. The results of two comparisons seem significant (Table XIV A and B): (1) the *total number* and average number of cues reported by the *S*s in each of the three intelligence groups; (2) extent to which a plan was made up of predominantly concrete or abstract elements. The possible discrepancy between cues reported and cues actually followed should be kept in mind particularly in the case of the report of 'relations of direction' as a method of solution. A number of *S*s, notably from the two lower Thorndike groups, who stated that such relations served as a cue, if judged by their performance, must have had a very vague notion, if any, of the actual relation of directions in the two mazes.

TABLE XIII

COÖRDINATION BETWEEN THE TWO HANDS

Intelligence Groups	Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	35	5	50	7	15	2	0	0
II	15	6	25	11	25	11	35	11
III	0	0	8	1	55	7	36	4

The figures in Table XIV A and B, showing the *S*s' remembrance of the cues that had seemed most important, or of which they had been most conscious, do not necessarly agree with the figures in Tables III–XIII, which were based upon *E*'s notes recorded during the progress of the problem solving. The *S*s mentioned three concrete methods of solution: (1) direction of movement toward the supposed position of the exit; (2) direction of movement away from the position, or the supposed position, of the center; and (3) pressure toward the outside (*i.e.* on the outer edges of the path). Totaling the number of cases from each group who reported using these definite schemes we have (Table XIV A) 29% from Group I, and 73 and 67% from Groups II and III, respectively. The difference between the last two groups is small, perhaps insignificant, but both report a significantly larger number of method elements of this kind than the members of Group I. In Table XIV B are given the same plan elements expressed as percentages of total number of cues reported. The concrete cues made up only 14% of all the cues reported by the superior group; 39% for Group II, and 44% for Group III. Group I has not only reported fewer concrete cues, but these are also a far smaller percentage of the total cues reported than is the case in the other two groups.

Three abstract plan elements were also reported: (1) The method of coördinating the movement of the two hands; (2) the method of giving oneself verbal directions as to hand movements; and (3) the method of observing the relation of the two hands in the maze. Each of these methods involves a more complicated order of procedure than the simple practical measures discussed above. The latter are directed simply toward the solving of the out-of-view problem, the former are based upon the double-maze situation and are directed to a solution of it *in relation* to the visible maze. In the case of the three abstract elements of methods the

TABLE XIV

INTELLIGENCE AND METHODS OF MAZE SOLUTION

Part	Intelligence Group	Concrete (Specific) Plan Elements								Abstract (General) Plan Elements								Total concrete and abstract	
		Direction of movement toward the supposed position of exit		Direction of movement from the supposed position of center		Pressure toward outside		Total methods involving concrete (specific) elements		Coördination		Verbal directions		Relation of line in two mazes		Total methods involving abstract (general) elements			
		%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.
A*	I	0	0	14	2	14	2	29	4	71	10	21	3	86	12	178	25	207	29
	II	27	11	2	1	44	18	73	30	20	8	17	7	76	31	112	46	185	76
	III	42	5	8	1	17	2	67	8	33	4	0	0	50	6	83	10	150	18
B†	I	0	0	7	2	7	2	14	4	34	10	10	3	41	12	86	25	100	29
	II	14	11	1	1	24	18	39	30	11	8	9	7	41	31	61	46	100	76
	III	29	5	6	1	11	2	44	8	22	4	0	0	33	6	56	10	100	18

*Gives the percent of individuals in the group who reported a certain cue. Since one individual may report more than one cue, the average of this row may be more than 1.00.

†Gives the frequencies of cues expressed as percentages of the total cues for the group.

general order of percentages shows Group I with 1.78%, Group II, 1.12%, and Group III, 0.83%. (The group percentages may be more than 1.00, because one individual may report more than one plan element.) These respective plan elements are 86, 61 and 56% of the total procedure types reported. Here the order is reversed from that in the case of the concrete simple plan elements.

The totals of both sorts of plans show Group I with 2.07% of plan elements, Group II with 1.85%, and Group III with 1.50%.

The group of superior intelligence (Group I) has one third more plan elements in conscious remembrance than Group III, and more than twice the number of abstract plans. Group II falls about half way between the superior and the inferior groups.

VI. Conclusions

(*1*) In solving motor space problems on the two-story maze, success (measured by time scores) and intelligence (measured by scores on the Thorndike College Entrance Test) showed a significant correlation. In three out of five problems the correlation coefficient was more than four times the probable error. In the two problems where the correlation was not significant, learning had entered into the situation, so that the problem was for many of the subjects no longer a new experience. On the series as a whole (5 problems) the correlation of speed with Thorndike score was also significant.

(*2*) Differences in reaction pattern appeared between three intelligence groups.

(A) Group I (14 cases) (Thorndike Intelligence score 88.5–110). These *S*s (1) made a shorter average time record, (2) fewer errors, (3) used trial-and-error less frequently, (4) used the 'key' as a guide more often, (5) recognized a contradictory element in the maze and overcame the difficulty more frequently, (6) definitely coördinated their hand movements, (7) moved in the general direction, but (8) did not make extensive use of excessive pressure, (9) were quicker in movement, (10) showed far greater evidence of foresight and planfulness, (11) relied slightly more on kinaesthetic cues, and (12) used more cues and tended toward the abstract and general in schemes of solution.

(B) Group II (41 cases) (Thorndike score 55.50–88.50) is intermediate between Groups I and III in the points in which the superiority of Group I was noted, except possibly with respect to excessive pressure. Groups I and II are clearly differentiated from Group III though not from each other in regard to pressure. *S*s of Group II reported more conscious dependence on visual imagery than did either of the other groups. In number of cues used, Group II is nearer Group I than Group III.

(C) Group III (12 cases) (Thorndike score 35.0–55.50) is inferior to both other groups on every count except excessive pressure where its members exceed both other groups.

(D) The groups are decidedly variable and there is much overlapping of performance.

(*3*) Practically all of the *S*s reported visual and kinaesthetic guidance or cues. Members of Group I were more apt (50%) to be conscious of the value of kinaesthetic imagery, than of mixed (36%), or especially of visual (14%) in this performance. Group II also believed themselves more dependent on kinaesthetic imagery (46%) but a considerable number of them reported visual imagery (32%) and mixed imagery (22%). Group III reported mixed imagery most frequently (42%), then kinaesthetic (33%), and visual (25%). The typical procedure was in all the groups to 'look at' the upper maze and 'feel' the lower one, and imagery in most cases aided these efforts to make as coördinated a performance as was achieved.

(*4*) For the solving of maze problems presenting new space relations intelligence as measured by Thorndike score seems to be an asset. The test index accords with speed of movement and with freedom from error and it seems also to register the ability of individuals to make use of imagery effectively, to organize logical and efficient plans of procedure, and to 'see through' (or 'feel through') the relations between a visible space situation and a similar one out-of-view.

(*5*) In the series of 5 problems one was found that appeared, when preceded by a practice problem, to offer a favorable performance test of intelligence. The correlation between time scores on Problem 2 in this series and Thorndike score was high (+0.59 ±0.06). With a wider intelligence range than a college group offers it might be standardized into a useful discriminative test.

FALSE SUGGESTION AND THE PIDERIT MODEL

By SAMUEL W. FERNBERGER, University of Pennsylvania

The following is the fourth report of a series of experiments performed at the University of Pennsylvania with the Piderit model for the demonstration of facial expression.[1]

Some years ago Buzby[2] showed six faces (Anger, Dismayed, Horrified, Disdainful, Disgusted and Bewildered) to 716 *Ss*. These *Ss* were merely asked to choose the best descriptive term from a list of eighteen among which were six correct terms. The results show a wide scatter of judgments and the percentages of correct judgments are relatively small.

In spite of these findings it is evident that the Piderit model is a very useful instrument for class demonstration. Jarden and Fernberger[3] showed the same six faces to 995 *Ss* with two different degrees of suggestion. The faces were shown and the emotion was named and the *Ss* were asked to judge (1) if the face was not a representation of the emotion, (2) if it was a poor, (3) a fair, or (4) a good representation of the emotion named. Except in the case of Anger, the percentage of correct judgments was very great—80% or over and, in one case, as high as 99%. Several months later, the same faces were shown to the same *Ss* with a very high degree of suggestion—the expression was analyzed and the face built up before the group. In the case of Anger there was a still further increase of correct judgments. For those faces which already had a very high percentage of correct judgments with the lower degree of suggestion, these percentages were not further increased.

Fernberger[4] worked all three procedures on six more faces—Quizzical, Amazed, Contemptuous, Attentive, Stubborn, and Reverential—with 750 *Ss* and obtained results which were exactly comparable to those of Buzby, and of Jarden and Fernberger. He found a wider scatter when no suggestion was given, a high percentage of correct judgments when the lower degree of suggestion was given, and a somewhat but not considerably higher percentage with the complete suggestion of analyzing and building up the faces.

Procedures. In the present experiment, we employed the same six faces used by Buzby, and by Jarden and Fernberger, viz., Anger, Dismayed, Horrified, Disdain, Disgusted, and Bewildered. Exactly the same procedures were again used.[5]

Subjects. For the first procedure (picking the best descriptive term from a list of 18) 666 *Ss* were used; for the second procedure (face 'named') 658 *Ss*; and for the third procedure (face 'analyzed') 630 *Ss*. None of these had been *Ss* with Buzby or with Jarden and Fernberger.

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*Accepted for publication December 26, 1927.

[1] E. G. Boring and E. B. Titchener, A model for the demonstration of facial expression, this JOURNAL, 34, 1923, 471-485.

[2] D. E. Buzby, The interpretation of facial expression, *ibid.*, 35, 1924, 602-604.

[3] E. Jarden and S. W. Fernberger, The effect of suggestion on judgment of facial expression of emotion, *ibid.*, 37, 1926, 565-570.

[4] Fernberger, Six more Piderit faces, *ibid.*, 39, 1927, 162-166.

[5] For a complete description of instruction, cf. Jarden and Fernberger, *op. cit.*, 565-566.

Results of first procedure. The results for the first procedure will be found in Table I. The number of judgments for each of the 18 descriptive terms appear in successive columns. The number of judgments for the correct terms are in boldface. The results show the usual scatter—for Anger and Dismayed every one of the 18 terms was checked at least three times; for Horrified and Disdain all but 2 of the terms were checked; for Bewildered all but 4 and for Disgusted all but 7 terms were checked. Although the percentages are not identical, the scatters are a very close confirmation of the Buzby results with these same faces. Indeed the rank order of correct judgments is exactly the same in both cases: Horrified and Disgusted are highest; Disdain and Bewildered intermediate; and Dismayed and Anger very low.

TABLE I

FREQUENCIES OF JUDGMENT WITHOUT SUGGESTION

666 cases	Anger	Dismayed	Horrified	Disdai n	Disgusted	Bewildered
Pleased	155	4	6	2	—	2
Displeased	6	12	4	32	13	1
Stubborn	4	15	3	69	7	4
Attentive	7	294	—	4	—	103
Quizzical	69	114	2	11	—	115
Inattentive	5	11	3	35	—	24
Dismayed	21	**28**	22	15	1	32
Reverential	6	38	—	21.	1	40
Affable	60	10	4	2	—	18
Bewildered	67	51	24	7	1	**184**
Amazed	26	17	55	—	—	136
Horrified	23	3	**355**	—	2	1
Raging	25	3	65	2	1	—
Disapproving	15	28	13	92	54	5
Disdainful	57	19	10	**210**	73	1
Anger	**15**	3	9	3	—	—
Contemptuous	79	8	26	115	226	—
Disgusted	26	8	65	46	**287**	—

From the former experiments it is evident that there was a great effect of suggestion and we decided to see how well one could 'get across' false suggestion by the two procedures of 'naming' and 'analyzing' the faces.

It was decided to use in the second and third procedures, three terms in the same emotional series (differing only in degree), and three terms in different emotional series. For the same emotional series we called the Dismayed face, 'Quizzical;' the Disgusted face, 'Contemptuous;' and the Disdain face, 'Displeased.' For the different emotional series we called the Anger face, 'Pleased;' the Horrified face, 'Attentive;' and the Bewildered face, 'Reverential.' The true faces and the false suggestions for the two emotional series are tabulated below:

SAME EMOTIONAL SERIES		DIFFERENT EMOTIONAL SERIES	
True Face	*False Suggestion*	*True Face*	*False Suggestion*
Dismayed	Quizzical	Anger	Pleased
Disgusted	Contemptuous	Horrified	Attentive
Disdain	Displeased	Bewildered	Reverential

The terms we chose had varying degrees of validity of judgment in the original scatter. The Anger-Pleased, Dismayed-Quizzical, and Disgusted-Contemptuous combinations had high validity, and the Horrified-Attentive, Disdain-Displeased, and Bewildered-Reverential combinations had low validity. Indeed, Attentive was one of the two terms not checked at all for the Horrified face.

Results of second and third procedures. The results of the judgments under the two degrees of false suggestion are given in Table II. Under the caption 'named' are given the percentages of 'good,' 'fair,' 'poor,' and 'no' judgments for each of the six faces for the lower degree of false suggestion. Under the caption 'analyzed' are given the percentages for the higher degree of false suggestion. The accompanying chart (Fig. 1) gives the percentages of 'correct' judgments under all three instructions. The shaded portions indicate the percentages of correct judgments with no suggestion, *i.e.* the percentages of the actually correct name of the face. The white portions indicate the percentages of 'correct' judgments under the 'named' degree of suggestion, *i.e.* the percentages of the 'good,' 'fair' and 'poor' judgments combined. The black portions indicate similarly the

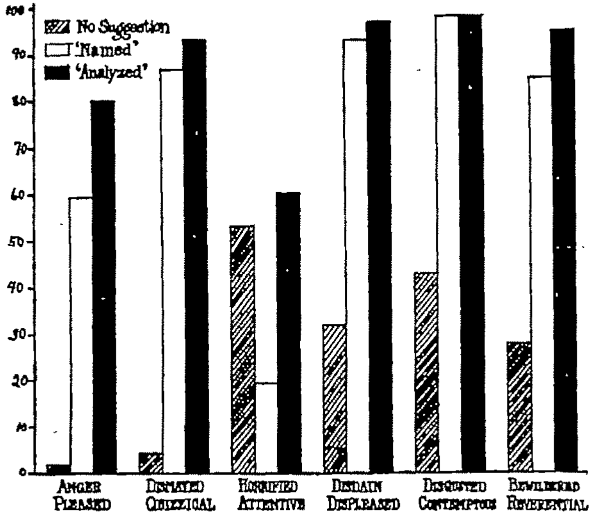

FIG. 1. COMPARISON OF RESULTS

TABLE II

RELATIVE FREQUENCIES OF JUDGMENT WITH TWO DEGREES OF FALSE SUGGESTION

True Face	False suggestion	'Named'—658 cases				'Analyzed'—630 cases			
		Good	Fair	Poor	No	Good	Fair	Poor	No
Pleased	Anger	6	24	29	41	16	33	31	20
Quizzical	Dismayed	31	43	13	13	47	36	10	7
Attentive	Horrified	5	5	9	81	11	20	30	40
Displeased	Disdain	39	38	16	7	61	29	7	3
Contemptuous	Disgusted	83	12	3	2	80	16	3	2
Reverential	Bewildered	29	41	15	15	45	37	13	5

combined percentages of 'correct' judgments for the 'analyzed' degree of false suggestion.

The results for four of the faces (Quizzical-Dismayed, Displeased-Disdain, Contemptuous-Disgusted, and Reverential-Bewildered) show a similar distribution differing only in degree. In each of these four cases the percentage of correct judgments is relatively low when no suggestion is given. These percentages increase enormously with the lower degree of false suggestion and, except in the case of Contemptuous-Disgusted, increase slightly again for the higher degree of false suggestion. The results for Contemptuous-Disgusted indicate that both degrees of false suggestion carried in 98% of the judgments. The higher degree of suggestion was adequate to produce a false judgment, in these four cases, in at least 93% of the cases. The lower degree of suggestion was adequate to produce a false judgment in these four cases in at least 85% of the *Ss*.

It will also be observed in Table II that, in the cases of Dismayed-Quizzical, Displeased-Disdain, and Reverential-Bewildered, the higher degree of suggestion gives a higher percentage of judgments in the 'good' and 'fair' categories.

The results for Attentive-Horrified are quite different from all the others. In this case we chose a face violently contorted in expression and tried to suggest it as an emotion for which the expression is relatively calm. Also we were here dealing with a face that 'gets across' in its own right very much better than any of the others (56% correct judgments with no suggestion).

When the face was 'named' falsely, the percentage of correct judgments dropped to 19. In every other situation it was considerably increased.

We therefore found it necessary, under the 'analyzed' false instruction, to create a general situation which might prove adequate. We told the *Ss* that this was a special case of attention, such as would be apparent if an individual had been giving a very high degree of attention in one direction at the time when a compelling stimulus from another direction forced attention to it. We emphasized the importance of the adjustment of muscular mechanisms for all attention and of the general muscular tenseness for all keen attention, and finally used again an invented proverb that one attends better while breathing through the mouth than through the nose.[6] We then acted out the situation and finally built up the face.

That this suggestion was successful, in part at least, is evidenced by the fact that there were 60% of correct judgments. This percentage for Attentive-Horrified, under the highest degree of suggestion, is considerably lower than that for any of the other five faces. Still it is reasonably adequate when one considers that the false suggestion is 9% better than when there is no suggestion at all.

Three of the terms employed in the present experiment are repeated from Fernberger's former study where they were used as true suggestions, while in this study they are used as false suggestions. It is of interest to note that the false suggestion is actually better than the true. The results for Contemptuous are practically the same for both degrees of suggestion and very high in both cases. Those for Quizzical and Reverential are better for false than true suggestion for both degrees. This means that Bewildered was judged 'correctly' more times as Reverential than Reverential was itself. Similarly Dismayed was judged 'correctly' more times as Quizzical than was the correct Quizzical face. This result was probably due to the greater enthusiasm of the experimenter in trying to 'sell' or 'put across' the faces in false suggestion than in the true.

The results for both degrees of false suggestion were analyzed to determine a possible significant sex-difference in suggestibility. In order to save space, this analysis is not included inasmuch as the results were entirely negative. No significant sex-differences in suggestibility of either the lesser or greater degrees of false suggestion were evident.

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[6] We were not able to find out for how many *Ss* this invented proverb had a familiar ring. After telling some of the later groups how the results were coming out and what we had been trying to do, many of the *Ss* volunteered the information that they had thought the proverb an old and familiar one.

It would seem that the results outlined above have a certain theoretical significance. They would indicate that little can be determined of an emotional state in another individual from the perception of an abstracted facial expression completely divorced from a general, total stimulus-situation. If a stimulus-situation is indicated, the emotional state is judged in accordance with that situation rather than in accordance with the facial expression. This judgment is made correctly with a high probability if the stimulus-situation is correctly indicated (Jarden and Fernberger) and it is made incorrectly with quite as high a degree of probability if the stimulus-situation is falsely indicated. Indeed, in the cases of Pleased-Anger and Quizzical-Dismayed, the false suggestion gave a higher percentage of 'correct' judgments than the true suggestion had formerly. This would indicate that our perception of the emotional states in others are in the nature of social meanings dependent more upon the stimulus-situation than anything else.

This point of view is also indicated in the results of Sherman.[7] Working with infants under 12 days of age, Sherman experimentally produced hunger, fear, anger, and pain. Motion pictures were taken of the stimulus-situation and of the reaction. These were shown for (1) reaction only without the stimulus-situation, (2) reaction with the correct stimulus-situation, and (3) reaction with a transposed or false stimulus-situation. Graduate students in psychology, medical students, student nurses, and Normal School freshmen acted as *S*s. The results show a wide scatter of judgment when the reaction is shown alone, much greater validity of judgment when the stimulus-situation and the reaction were both shown, and also a greater but false validity with the transposed stimulus-situations. Sherman concluded in part: "The results indicate that the stimulus preceding the reaction shown was usually the deciding factor in the name given to the reaction."[8]

It is of interest to mention in passing that in analyzing the faces, one could logically find as adequate a biological reason why each of the features should be shown in the false suggestion as had been true in the true suggestion several years ago. This aspect of the problem, which involves much more extended treatment than we can give here, may be of some importance in criticizing the theories of the followers of the Darwinian school of the emotions.

SUMMARY

(1) The results for the Piderit model, when no suggestion was given, showed a wide scatter with little validity of correct judgment in the interpretation of expression of emotion. This is a complete confirmation of the findings of Buzby working with the same six faces representing Anger, Dismayed, Horrified, Disdainful, Disgusted, and Bewildered.

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[7] M. Sherman, The differentiation of emotional responses in infants: I. Judgments of emotional responses from motion picture views and from actual observation, *J. Comp. Psychol.*, 7, 1927, 284.

[8] *Op. cit.*, 284.

(2) With the lower degree of suggestion involved in 'naming' the face, false suggestion led to a great increase in the percentage of 'correct' judgments when the false suggestion represented Anger as Pleased, Dismayed as Quizzical, Disdain as Displeased, Disgusted as Contemptuous, and Bewildered as Reverential.

(3) With the higher degree of false suggestion involved in 'analyzing' the face, a relatively great increase in 'correct' percentages was found for the Anger-Pleased situation and only a slight increase for the other four situations. All of the other four faces gave 'correct' judgments of more than 93% under this higher degree of false suggestion.

(4) Horrified gave a high percentage of correct judgments when no suggestion was given and a considerably lower percentage when the face was merely falsely named. It was found possible to create, under 'analyzed,' a situation which gave the highest percentage of correct judgments for this face (60%).

(5) These results seem to indicate that the perception of emotional states in others by their expression is of the nature of social meanings and that they are much more dependent on the stimulus-attention than anything characteristically intrinsic in the facial expression or reaction.

SENSITIVITY TO ODORS AND OTHER PHENOMENA DURING A FAST

By JOHN ARTHUR GLAZE, University of Michigan

In a previous experiment it was found that some *S*s showed a marked increase in sensitivity to odors during a fast.[1] This was particularly true with the *S*s that endured long fasts of 17 and 33 days. The observations of Marsh, as we mentioned in the earlier study, confirm these findings.[2] As neither Marsh's nor our study was designed to test this phenomenon, the following study was undertaken.

TESTS WITH ODORS

Apparatus. The Zwaardemaker olfactometer was employed in the experiments. It consists of "one tube with a curved end to be inserted in the nostrils, and of a second tube of larger size containing an inner lining of the substance to be investigated which slips over the former. The amount of stimulation is measured by the area of the outer tube exposed beyond the inner."[3] As a matter of convenience we shall call the tube containing the substance the *x* tube, and the inner conducting tube that extends through the shield to the nose the *y* tube. The end of the *y* tube was covered by a smooth nickel cap so that it closed the side of the nose in which it was placed. The *S* always closed the other nostril with a finger during the test.

Seven odoriferous tubes were used. The liquid odors were diffused through warm paraffine, poured into tube *x*, and a hole the size of tube *y* was bored through the paraffine after it had hardened. The ends of the *x* tubes were sealed with tin foil so as to hold the substances and also to prevent wear at the ends. A hole, the size of tube *y*, was of course made in the center of the tin foil caps.

Stimuli. The substances used were asafoetida, benzoin, cedar wood, rubber, balsam, beeswax, and Russian leather. The first was very weak, the last very strong, the others ranged in strength between these. When not in use the tubes were kept in screw-top glass containers.

Procedure. Several *y* tubes were used, and they were thoroughly washed and dried at the end of every test. When tube *x* was placed on tube *y* it was shoved beyond the end so that tube *y* projected out of tube *x*. *S* then blew the air out of *y* and, after a short wait, placed the nickel cap over the end of the *y* tube and the experiment began. Tube *x* was exposed slowly until *S* indicated that he scented the odor. Then the *x* tube was shifted in the other direction until *S* reported that the odor was lost. The mean of these two points was taken as the *S*'s limen. Only in a few cases were these deviations large, and these few cases were more apt to occur with the strong and weak odors than with those of intermediate strength. It took from 20–30 min. to test an *S* with all 7 odors, consequently a rest

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*Accepted for publication February 29, 1928.

[1] J. A. Glaze, Psychological effects of fasting, this JOURNAL, 40, 1928, 236–253. Cf. especially 250 f.

[2] H. D. Marsh, Individual and sex differences brought out by fasting, *Psychol. Rev.*, 23, 1916, 438.

[3] W. B. Pillsbury, *The Fundamentals of Psychology*, 2nd ed. 1922, 180.

period of about 5 min. was usually interpolated between the fourth and fifth tubes. Sensitivity was measured by the number of mm. of the odoriferous material exposed.

Subjects. Two *S*s were used: B, the writer, who served in the previous experiment; and D, a 10-yr. old boy, a patient in the University of Michigan Hospital. Eight weeks before the beginning of the experiment, D was admitted to the hospital and shortly afterward was operated on for tonsils and adenoids. He was able to walk from the ward to the laboratory where the tests were held.

D was up to grade in school subjects, had a mental age on the Binet test of 9½ yrs., and I.Q. of 87. He was decidedly overweight for his age and was willing to fast to reduce. Indeed, he reported that he had fasted before, and that he had gone as many as 5 days without food so as to get rid of his excess weight—a subject about which he was sensitive.

B fasted for 10 days, and D for 5 days. As in the earlier study, a practice period preceded the fasts. In general the pre-fast and post-fast test periods were of about the same length as the fast periods. Neither *S* showed signs of a cold throughout the experiment, and D never left the hospital.[4]

TABLE I

SENSITIVITY OF B AND D TO ODORS DURING THE PRE-FAST, FAST, AND POST-FAST PERIODS

Sensitivity expressed in mm. of odoriferous material exposed

Pre-Fast				Fast				Post-Fast			
B		D		B		D		B		B	
day	mm.	day	mm.	day	mm.	day	mm.	day	mm.	day	mm.
1	*	1	*	11	60.1	7	19.0	21	29.3	12	25.3
2	69.0	2	53.0	12	34.0	8	6.9	22	22.0	13	18.1
3	56.4	3	22.6	13	25.4	9	− 0.6	23	41.9	14	21.9
4	44.4	4	14.6	14	13.4	10	− 1.4	24	26.7	15	28.9
5	37.7	5	20.9	15	− 2.1	11	− 1.7	25	26.6	16	48.9
6	48.1	6	36.7	16	− 9.7			26	37.0		
7	63.3			17	− 3.7			27	31.6		
8	42.3			18	0.6			28	38.4		
9	70.0			19	0.7						
10	66.4			20	− 1.7						

*Practice, no data taken.

Results. The results are shown in Table I. Minus values are shown better after the second day of D's fast and after the fourth day of B's fast. This means that sensitivity was so acute that the *S*s were able to scent some of the odors before the *x* tubes were drawn out and exposed in the *y* tubes. In the case of Russian leather and part of the time in the cases of beeswax and balsam this was quite pronounced. At first *E* was suspicious that the *S*s had anticipated the odor, but when the report was verified on successive days there remined no doubt but that the first measures were accurate.

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[4] I have Dr. David M. Cowie and his assistants of the University Hospital staff to thank for their kindness in allowing me to use this *S* and in giving instructions to the diet kitchen to withhold the plate from him during the 5 days.

It must be said that tube y did not completely fill tube x. Six of the y tubes were so nearly the same diameter that they were used indiscriminately with any of the 6 odors. The seventh y tube was slightly smaller than the others and was always used with the rubber x tube. Minus values mean then that S was able to scent the odors when none of the x tube projected beyond the end of the y tube. Russian leather, beeswax, and balsam had large minus values whereas asafoetida was exposed as much as 50 mm. on some days during the fast to reach the sensory threshold. Benzoin was also weak. The averages of the 7 odors, given in Table I, do not show how difficult it is to sense some of the stimuli.

Asafoetida, rubber, and benzoin were not scented at times during the pre- and post-fast periods even when they were exposed at their full strength—100 mm. We counted these cases, however, as if they were sensed at 100 mm. Ostensibly this diminishes the ratio of increased sensitivity during the fast, since we do not know how much of an increase was necessary to bring these odors to the sensory threshold. Before the fast, B was unable to scent two and sometimes three of these odors when fully exposed, while after the fast he never scented asafoetida at all. In the case of D a smaller number of failures was found. This observation raises the old question whether children are more sensitive to odors than adults.

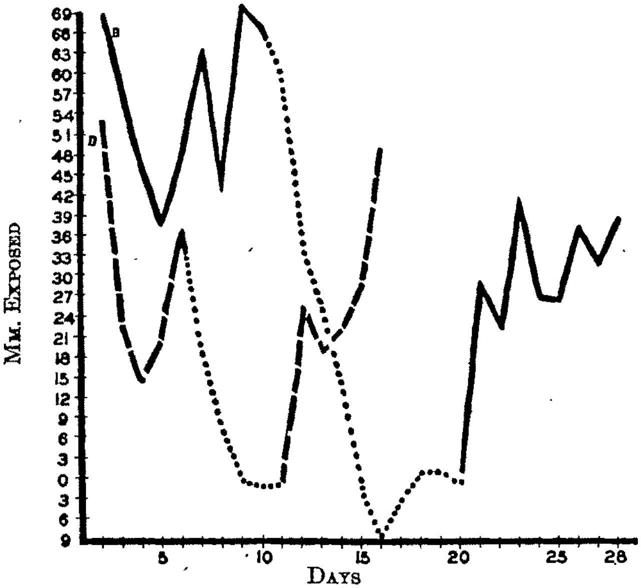

FIG. 1. SENSITIVITY TO ODORS
Broken lines show results of the fasts

Fig. 1 illustrated the results of Table I. The dotted lines show the results during the fast. The sixteenth day for B is not quite so low as it should be in the figure.

The literature on experiments of this kind is meager. Most of it is the by-product of experiments similar to Marsh's and our own earlier study. Hoelzel and Kleitman, for example, discuss the increased sensitivity of the digestive tract in connection with increased gastric acidity during a fast.[8] They show that the digestive tract is more sensitive to pain during and immediately following a fast than at other times.

That this sensitivity should be reflected in the upper part of the digestive tract seems reasonable. The close relation of taste and smell is well known. In the case of leather and beeswax a burning sensation was noticed in the nasal tract. This did not apparently extend beyond the throat, in the region of the thorax. The pleasant odors of balsam apparently did not extend so deeply, but filled the nasal cavity with a fullness unlike that felt at other times.

In the absence of more experimental work of this kind it is difficult to formulate a theory as to why one should be more sensitive to odors during a fast. As between central and peripheral mechanisms we are inclined to attribute more importance to the latter. As between the rest of the alimentary tract and the olfactory bulbs the evidence, it seems to us, is also on the side of the latter, although the former may affect sensitivity in an indirect way. It has been suggested that the bipolar sensory cells located in the nasal mucous membrane are not necessarily more sensitive during a fast but the secretion covering the surface of the epithelium is more condensed. The gaseous particles entering into solution would make a more condensed solution and the corresponding increased stimulation may result.

It is well known that the greatest loss in weight during a period of fasting occurs in the first 3 or 4 days. The undigested food residue accounts for part of this, but not all. The liquid intake is greatly reduced during a fast, and the *S* has to force himself to drink a sufficient amount to meet the needs of the body. It is reasonable to suppose that the fluid about the mucous and serous cells that ordinarily covers the mucous membrane would be less with the decreased food and water supply. Hence the above assumption that the gaseous particles form a more condensed solution as they are absorbed by the liquid of the mucous membrane seems plausible.

OTHER TESTS

In order to check the result of our earlier study we employed four other tests with B. The first two were tests of equilibrium and steadiness—the same as were used in the earlier work and administered in the same way; the third was a memory test; and the fourth a relations test.

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[8] F. Hoelzel and N. Kleitman, Some conditions affecting subjective and objective manifestations of hunger, *Arch. Inter. Med.*, 39, 1927, 710-740.

Equilibrium. The test of equilibrium, as before, gave no definite nor significant results (see Table II).

Steadiness. The steadiness test gave positive results after the first 5 days, *i.e.* a constant increase in steadiness as the fast progressed. These results are shown in Fig. 2. The averages in Table II show no gain during the fast because the marked unsteadiness the first few days of fasting tend to obliterate the actual increase. The holes in the metal plate, in this study, were somewhat smaller than in the earlier work, and that is why more errors are recorded in the present study. This change improved the test since the smaller the holes the more definite the results.

Memory. In order to measure more adequately the 'central processes' we prepared a test from our classification of nonsense syllables.[6] We took an equal number of syllables from the 8 divisions that are represented from 0–40% inclusive. These syllables are difficult to memorize and although meaning can be attached to some of them, most of them meant nothing for the *S*s. Sixteen of these were used daily. They were presented one every 2 sec. for 10 readings at the beginnings of the group of tests. After an interval of 25 min., B was tested for the number retained. The interval was occupied with other tests arranged in such a manner that the olfactory test came before the test employing nonsense syllables. It is evident that we could stop the olfactory test at any place, to return to it later, without invalidating the results. In case of approaching adaptation an interval of rest was interpolated, but this never occurred in this test (olfactory) before the memory test. After a measure of the syllables retained we proceeded with successive presentations until all of the syllables were memorized. This gave us two sets of data, as can be seen in Table II.

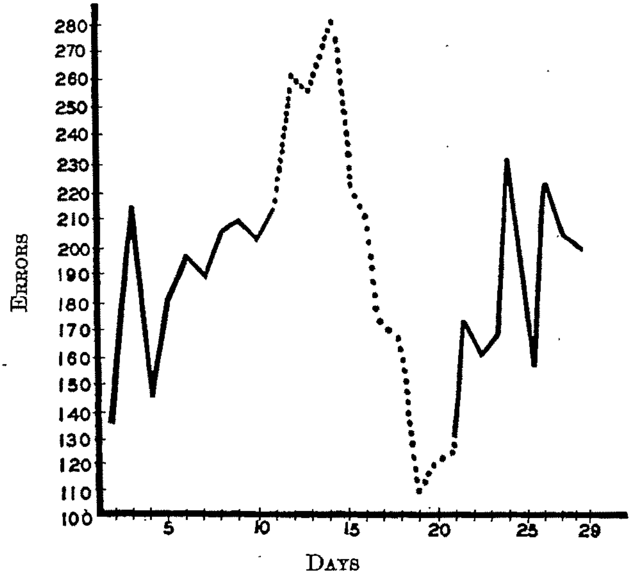

FIG. 2. STEADINESS TEST
Results of B. Broken lines show results of the fast

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[6] J. A. Glaze, The association value of nonsense syllables, *Ped. Sem.*, 35, 1928, 255–267.

The anticipation method was used. In view of the post-fast performance it can be said that nothing definitely in the way of increased mental acuity is shown during the fast.

Relation. A different process is represented by what we have chosen to call the 'relations test.' This test was made up of 5 consonants arranged in the form of a nonsense word, the following being examples: MWGKX, HGKZD, FNZKB, ZBJVN, VBGSX, ZSFMK, DGFBH. This type of test has been used before in the Michigan laboratory.[7] The problem consisted in (a) indicating every 'word' that contained three consecutive letters not found in the preceding 'word,' and (b) indicating every two adjacent 'words' that had two consecutive letters in common. Every nonsense word was placed on a card, and 120 cards were presented every day, requiring 238 judgments. A 5-sec. exposure time was used. The results of this test are shown in Table II.

TABLE II

PERFORMANCES OF B IN FOUR TESTS DURING THE PRE-FAST, FAST, AND POST-FAST PERIODS

mm = millimeters movement; c = contacts

Test		Pre-Fast		Fast		Post-Fast	
		Av.	m.v.	Av.	m.v.	Av.	m.v.
Equilibrium	Blindfold	68.8 mm	8.8	66.6 mm	13.2	63.6 mm	13.1
	Seeing	56.2 mm	9.3	58.9 mm	10.4	44.5 mm	12.3
	Av.	62.7 mm	8.9	62.8 mm	6.0	59.7 mm	10.2
Steadiness (hands)	Right	84.4 c	15.0	93.7 c	32.9	91.6 c	16.4
	Left	98.4 c	12.2	94.5 c	24.1	95.4 c	10.5
	Av.	94.4 c	10.2	95.6 c	26.7	93.5 c	11.8
Memory	Savings	3.25%	1.9	5.1%	1.5	5.9%	2.0
	100% learning	12.6	5.1	10.4	2.4	8.5	2.5
Relations correct		92.79%	2.79	94.79%	2.25	98.28%	0.9

SUPPLEMENTARY EXPERIMENTS

(1) *Sensitivity before and after meals.* Evidence was gained in a supplementary experiment that showed *Ss* are more sensitive to odors before a meal than after it. Seven students in elementary psychology (6 sophomores and 1 senior) were tested with the 7 odors mentioned above. The *Ss* agreed to eat normally, to forego eating or chewing anything, or drinking anything that contained odorous or food substances between 8:00 A.M. and noon, the testing hour. A prohibition was also placed on smoking during these hours. After the first tests the *Ss* (only 2 a day could be tested) went to lunch and returned immediately afterward for the second tests. Each *S* was tested 6 days. The results of the first day, so as to eliminate the effects of practice, were discarded. The effects of practice, however, did not appear to be very marked. The results are shown in Table III.

All the *Ss*, with the exceptions to be noted, ate a light breakfast and a fairly heavy lunch. G.R. went without breakfast, and I.B. and D.H. ate light lunches, usually nothing more than a sandwich and a cup of chocolate.

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[7] C. P. Wang, *The General Value of Visual Sense Training in Children*, 1926, 68.

(2) *Retest with D.* Six days after the experiment with D ended we decided to test him by imposing another 5-day fast. He was available and apparently willing, but was very anxious for the fast to end after it was under way. Four days of preliminary testing was done and 5 days follow-up. No significant results were secured.[8]

TABLE III

SENSITIVITY TO ODORS BEFORE AND AFTER THE NOON MEAL
Sensitivity expressed in mm. of odoriferous material exposed

Time of test	*Ss*						
	R.E.	R.S.	J.H.	H.A.	I.B.	D.H.	G.R.
Before meal	46.1	61.8	63.7	27.4	36.5	28.2	28.9
After meal	93.0	68.3	66.9	34.9	39.0	26.3	28.3

CONCLUSION

We conclude from the results of this study:

(1) That there is a marked increase in the sensitivity to odors during a fast.

(2) That steadiness, measured by the pointing test, tends to increase as the fast progresses.

(3) That the higher mental processes are not improved while the fast is in progress.

(4) That the most pronounced success, both in mental and muscular activity, is attained during the post-fast period.

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[8] It was a mistake to use this *S* again so soon after the first fast. The intimacy of contact in the previous fast had its disadvantages. On the fourth day of the fast, for example, he came to the laboratory (on the same floor as his ward) chewing gum. He had secured it from some boys in his ward. He became somewhat secretive about the matter when questioned after the test. The results for the retest are: pre-fast, 32.4 ± 5.4; fast, 35.3 ± 3.5; post-fast, 34.1 ± 3.8.

THE LIGHT REACTIONS OF THE MEAL WORM (*TENEBRIO MOLITOR* LINN)

By Charles Edward Balfour, Princeton University, and
Leonard Carmichael, Brown University

The object of the experiments reported in this paper was to determine the reactions of the meal worm, *Tenebrio molitor* Linn, to certain light conditions.[1]

The meal worm is a yellow, waxy, slender larva of an average length of 2 cm. In adult form it is a 'darkling' beetle approximately 1½ cm. in length with wing covers which are rigid lengthwise. One generation a year is said to be normal. The beetle is nocturnal. The white, bean-shaped eggs are approximately 1 mm. in length. They are deposited in meal. The larvae mature in about three months and the pupa stage has a duration of approximately three weeks.[2]

No extensive work has been done on the light reactions of *Tenebrio molitor*. In 1893 Loeb characterized this type as negatively heliotropic,[3] and in 1905 he reported a rather casual study of the meal worm.[4] He notes that if the animals are placed upon a plate near a window they will creep to the dark side of the receptacle. If the plate be covered with dark blue glass the result of the experiment is the same, but under red glass the larvae behave much as they do in the dark. If the plate be covered one half with red glass and one half with blue glass the animals will move from that part of the plate under the blue glass to that under the red glass, but not from the portion of the receptacle under the red glass to that under the blue. If one half of the plate is covered with red glass and the other half with opaque cardboard the animals scatter in all directions, but at length the greater number collect under the cardboard. All of these experiments were done in direct sunlight. Loeb noticed that the meal worms show a marked stereotropism and tend to collect in the corners of boxes. He also noted that the animals avoid or "flee from moisture and seek dry spots."[5]

It is easy to substantiate the general fact of what Loeb has called the heliotropism of this organism. The present writers have frequently noticed at night, on entering the laboratory room in which the meal containing the worms was kept in a glass jar, that, immediately after turning on the light, a large number of worms may be observed upon the exposed surface of the meal. In the light the organisms at once began to burrow back into the meal and in a few seconds none were visible. The experiments reported below were undertaken in an effort to give a more exact description of certain responses of the meal worm.

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[1] The authors are indebted to Professor G. H. Parker of Harvard University for suggesting this problem. The major portion of the experimental work was carried out in the psychological laboratory of Princeton University.

[2] The facts of the natural history of *Tenebrio molitor* Linn given above are based upon one of the few accounts of this organism available. Cf. W. Lockhead, *Class Book of Economic Entomology*, 1919. Most of the facts presented, however, have been verified by the authors who have studied the material through a period of several years in the laboratory.

[3] J. Loeb, Ueber künstliche Umwandlung positiv heliotropischer Thiere in negativ heliotropische und umgekehrt, *Arch. f. d. ges. Physiol.*, 54, 1893, 102.

[4] Loeb, *Studies in General Physiology*, 1905, 70 f.

[5] *Loc. cit.*

EXPERIMENTAL

Apparatus. In order to study the light reactions of the meal worm under controlled conditions a simple maze was constructed consisting of three alley-ways in the form of the letter 'T.' This maze was so arranged that the worm, on being placed in the central alley, could move forward for a distance of approximately 10 cm. before it came to the end of the alley. At the end of this entrance alley were openings into two alleys which lead to the right and to the left respectively and together formed the top of the 'T.' Each of these two upper alleys was 10 cm. in length. The walls of all alleys were approximately 5 cm. in height. The alleys themselves were ½ cm. in width. The maze was so arranged that electric lights could be flashed on at all three openings of the 'T.' Thus by appropriate switches the end of the right or left alley could be illuminated. A light was also placed behind the entrance alley for it was found that this device assisted in directing the animal to move into the maze. These lights consisted of small 6.2-volt Mazda bulbs. The entire maze was constructed of wood and was evenly painted a dull black.

Procedure. Before any differential experiments were attempted, each worm was taken from darkness and run through the maze 10 times. During these preliminary trials all lights were extinguished save the one at the entrance alley. This precaution was taken because it was discovered that certain of the worms tended under neutral conditions to turn consistently either to the right or to the left. Only worms were selected for experimental study which, in a series of 10 trials, turned at least 5 times toward the alley that was to be lighted in that particular experimental series. By observing this precaution it was rendered more probable that the differential action observed in the worm was due to the light, rather than to a former habit or some intrinsic structural or physiological peculiarity of the particular worm which determined it to turn predominately either to the right or to the left. It is a curious fact that something analogous to the human condition of right- and left-handedness was discovered to exist in these larvae.

Each larva thus selected was run through the 'T' apparatus 20 times with light on the right or on the left side, as the case might be. The larvae were inserted into the entrance alley and gently stimulated by the light as already noted. In some cases a small camel's hair brush was also used to stimulate the animal. As indicated above by Loeb, the larvae were decidedly stereotropic and this response in the larger worms tended to retard their progress through the alleys of the maze. In general the smaller worms moved more rapidly than did the large ones.

When a worm reached the point at which the entrance alley joined the two alleys forming the cross bar of the 'T,' one side of its anterior segments was illuminated by the light placed at the right or left exit as the case might be. The behavior of the animals in this situation was by no means uniform. The same worm did not always give the same reaction at this point nor did any two worms necessarily behave in the same manner. It was possible, however, roughly to classify the behavior of the worms at this point into six general types. These types are indicated in the table as *A*, *B*, *C*, *D*, *E*, and *F*. Behavior that eventually resulted in turning away from the light is characterized as either *A*, *B*, or *C*. That which resulted in turning the animal toward the light is termed *D*, *E*, or *F*. It must be understood that these six types of behavior are in no sense considered anything more than an essentially arbitrary effort on the part of the authors to give some quantitative estimate of the nature of the behavior of the larvae as seen in the specific situation described.

In explanation of the behavior of the classes designated by the letters appearing in Tables I and III, the following definitions may be listed:

A. This activity includes those responses in which the worms turned and went down the dark alley, directly, without moving toward the light at all.

B. This activity includes those responses in which the worms first moved a few segments into the lighted alley and made slight movements with the anterior segments, before definitely turning into the dark alley.

C. This activity includes those responses in which the worms moved for a few segments into the darkened or lighted passage a number of times before eventually moving into the darkened passage.

D. This activity includes those responses which are the reverse of the responses included in class *C*. In this activity after the worms had moved for a few body-segments into the darkened or lighted passage a number of times, they eventually moved into the lighted passage.

E. This activity includes those responses which are the reverse of the responses included in class *B*. In this activity after the worms had moved for a few body segments into the dark alley, they turned and went definitely into the lighted passageway.

F. This activity includes those responses which are the reverse of the responses included in class *A*. In this case the worms moved directly from the entrance alley into the lighted passage.

Results. The results of these maze experiments are summarized in Tables I–IV.

Table I uses the letters as indicated above. In this table the reactions of the 21 worms of the first experimental series are shown in detail. In the first vertical column (reading from left to right) is given the number assigned to each worm. In the second column, headed "No Light," is given the reactions of each worm to the maze when both the right and the left alleys were unilluminated. The next columns show the reactions made by each worm to 20 trials in each one of which the light was on the right side.

Table II summarizes the results given in full in Table I. The numbers in the vertical columns indicate the frequency in the twenty trials of each of the types of behavior listed above as *A*, *B*, *C*, *D*, *E*, and *F*. The group of columns labeled "first half" and "second half" respectively show the difference between the first 10 trials and the second 10 trials. Below the individual columns is given the sum of the frequency of the 6 classes of behavior. These sums are also shown as percentages of the total number of trials. Finally in the bottom line is given the percentage of trials which led away from the light and those which led into it. It will be seen that in approximately 4 out of every 5 trials the worms turned away from the light. Little significant difference is to be noted between the first and the second series of 10 trials. In other words, the present experiments give no real indication of a practice effect, although they do not demonstrate that such an effect is absent.

Table III shows the result of a series of check experiments in which the light was on the left and not on the right side. In other respects this table may be read like Table I, above.

TABLE I

DETAILED RESULTS OF EXPERIMENTAL SERIES, WITH LIGHT ON RIGHT SIDE OF MAZE

Worm	No Light		Trials: Light on Right																			
	R	L	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1	5	5	F	A	C	C	E	A	A	B	A	A	B	F	A	C	B	A	A	B	E	A
2	7	3	B	B	B	B	F	B	B	B	B	B	B	C	B	B	B	B	B	B	C	C
3	5	5	A	A	A	D	B	B	A	A	B	A	E	B	B	A	B	B	A	B	B	A
4	8	2	B	B	D	A	B	B	A	B	B	B	B	B	B	B	D	D	A	B	B	A
5	8	2	F	F	D	C	B	A	B	B	B	B	B	B	F	B	B	B	B	B	F	C
6	10	0	F	B	B	F	B	F	B	B	F	F	B	B	B	D	B	F	B	F	F	D
7	5	5	B	D	A	A	A	A	A	A	A	A	A	B	E	A	A	A	A	A	A	B
8	10	0	F	F	F	B	F	F	F	B	F	F	F	F	F	B	B	A	B	B	F	B
9	6	4	A	F	A	F	B	F	B	A	B	B	F	A	B	B	B	B	B	B	B	B
10	5	5	B	A	A	A	A	A	A	A	A	C	B	A	C	A	B	A	A	C	A	B
11	5	5	B	E	B	A	F	A	A	A	A	F	F	F	A	A	B	A	A	A	A	A
12	9	1	B	F	B	B	F	B	B	B	B	B	B	F	B	F	B	B	F	F	B	B
13	9	1	B	B	A	A	F	A	B	B	F	B	A	B	B	F	A	A	B	B	F	B
14	8	2	B	C	C	A	C	C	C	C	D	A	C	A	A	A	A	A	A	A	C	A
15	5	5	B	B	A	F	B	C	A	B	B	A	A	A	A	B	A	A	B	C	B	B
16	8	2	B	F	B	F	D	F	A	A	C	B	B	F	B	C	B	B	A	C	B	B
17	7	3	A	A	A	A	A	A	A	A	A	F	F	A	A	B	A	A	B	C	F	A
18	7	3	A	B	B	A	B	B	F	B	A	B	B	F	B	B	B	B	F	A	A	B
19	7	3	A	A	F	B	B	A	A	B	A	A	A	A	A	A	A	F	B	F	A	A
20	8	2	B	A	A	A	A	A	B	F	B	F	B	B	A	A	A	A	B	A	A	A
21	10	0	A	A	A	F	A	A	A	B	A	A	A	A	A	A	A	A	F	A	A	A

TABLE II

SUMMARY AND ANALYSIS OF RESULTS OF EXPERIMENTAL SERIES SHOWN IN TABLE I

Worm	End Reations (20)						First Half (10)						Second Half (10)					
	A	*B*	*C*	*D*	*E*	*F*	*A*	*B*	*C*	*D*	*E*	*F*	*A*	*B*	*C*	*D*	*E*	*F*
1	9	4	3	0	2	2	5	1	2	0	1	1	4	3	1	0	1	1
2	0	16	3	0	0	1	0	10	0	0	0	0	0	6	3	0	0	1
3	9	9	0	1	1	0	6	3	0	1	0	0	3	6	0	0	1	0
4	4	13	0	3	0	0	2	7	0	1	0	0	2	6	0	2	0	0
5	1	12	2	1	0	4	1	5	1	1	0	2	0	7	1	0	0	2
6	0	10	0	2	0	8	0	5	0	0	0	5	0	5	0	2	0	3
7	15	3	0	1	1	0	8	1	0	1	0	0	7	2	0	0	1	0
8	1	7	0	0	0	12	1	2	0	0	0	7	1	5	0	0	0	4
9	4	11	0	0	0	5	3	4	0	0	0	3	1	7	0	0	0	2
10	13	3	3	1	0	0	8	1	1	0	0	0	5	2	2	1	0	0
11	12	3	0	0	1	4	5	2	0	0	1	2	7	1	0	0	0	2
12	0	14	0	0	0	6	0	8	0	0	0	2	0	6	0	0	0	4
13	6	10	0	0	0	4	3	5	0	0	0	2	3	5	0	0	0	2
14	10	1	8	1	0	0	2	1	6	1	0	0	8	0	2	0	0	0
15	8	9	2	0	0	1	3	5	1	0	0	1	5	4	1	0	0	0
16	3	9	3	1	0	4	2	3	1	1	0	3	1	6	2	0	0	1
17	14	2	1	0	0	3	9	0	0	0	0	1	5	2	1	0	0	2
18	5	12	0	0	0	3	3	6	0	0	0	1	2	6	0	0	0	2
19	13	4	0	0	0	3	6	3	0	0	0	1	7	1	0	0	0	2
20	12	6	0	0	0	2	5	3	0	0	0	2	7	3	0	0	0	0
21	17	1	0	0	0	2	8	1	0	0	0	1	9	0	0	0	0	1
Individual Sums	156	159	25	11	5	64	80	76	12	6	2	34	77	83	13	5	3	29
Trials Total						420						210						210
% of Each Reaction	37	37	06	02	01	15												
Totals		L 340			80 R			L 168			42 R			L 173			37 R	
% of Gross Bodily Reactions		80.95			19.05			80			20			82.38			17.62	

TABLE III

DETAILED RESULTS OF CHECK SERIES WITH LIGHT ON LEFT SIDE OF MAZE

Worm	No Light		Trials: Light on Left																			
	L	R	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
C–1	9	1	*B*	*B*	*B*	*A*	*B*	*B*	*A*	*A*	*A*	*A*	*F*	*B*	*A*	*B*	*B*	*B*	*B*	*B*	*A*	*B*
C–2	6	4	*A*	*A*	*A*	*B*	*B*	*B*	*F*	*B*	*B*	*B*	*B*	*A*	*B*	*A*	*A*	*A*	*A*	*A*	*A*	*B*
C–3	8	2	*A*	*F*	*A*	*A*	*B*	*A*	*F*	*F*	*A*	*B*	*B*	*B*	*B*	*B*	*F*	*F*	*A*	*F*	*A*	*B*
C–4	9	1	*F*	*A*	*A*	*A*	*F*	*B*	*X*	*B*	*A*	*C*	*B*	*A*	*B*	*F*	*B*	*C*	*A*	*B*	*F*	*B*
C–5	6	1	*F*	*C*	*C*	*C*	*C*	*F*	*E*	*A*	*B*	*C*	*E*	*B*	*B*	*A*	*A*	*A*	*A*	*F*	*F*	*C*

TABLE IV

SUMMARY AND ANALYSIS OF RESULTS OF CHECK SERIES SHOWN IN TABLE III

Worm	End Reactions (20)						First Half (10)						Second Half (10)					
	A	*B*	*C*	*D*	*E*	*F*	*A*	*B*	*C*	*D*	*E*	*F*	*A*	*B*	*C*	*D*	*E*	*F*
C–1	7	12	0	0	0	1	5	5	0	0	0	0	2	7	0	0	0	1
C–2	10	9	0	0	0	1	3	6	0	0	0	1	7	3	0	0	0	0
C–3	7	7	0	0	0	6	5	2	0	0	0	3	2	5	0	0	0	3
C–4	6	7	2	0	0	4	4	2	1	0	0	2	2	5	1	0	0	2
C–5	5	3	6	0	2	4	1	1	5	0	1	2	4	2	1	0	1	2
Individuals Sums	35	38	8	0	2	16	18	16	6	0	1	8	17	22	2	0	1	8
Trials Total						99						49						50
% of Each Reaction	35	38	08	0	02	16												
Totals	R 81			18 L			R 40			9 L			R 41			9 L		
% of Gross Bodily Reactions	81.82			18.18			81.64			18.37			82			18		

Table IV shows the summary of the results given completely in Table III. It should be read like Table II. The fact should be noted that under the conditions of this check series the worms turned away from the light in approximately 4 out of every 5 trials.

Beside these experiments in the 'T' maze other observations were made upon the light reactions of the meal worm.

The tube and objective of a small compound microscope was fastened on the end of a pocket flash-light. When the light was "turned on," this device produced a fine pencil of light between 1–2 mm. in diam. When a meal worm was placed upon a table in a dark room it was found possible to direct its movements in a most automatic fashion by turning this pencil of light upon the right or left side of the anterior terminal segment of the worm. The reactions of the worm under these conditions were so direct and stereotyped that they indeed suggest Hammond's "Electric Dog," an artificial heliotropic machine described by Loeb.[6] The anterior segment of the worm was the only segment which seemed subject to this stimulation. It seems indeed that the light receptors of this organism are concentrated in this region. Small pigment spots are probably the locus of the receptive function. Our observations show that there is little reason to suppose that the entire integument of the worm is in any marked degree sensitive to light as is the case in certain other organisms.[7]

The localization of the light receptor in the anterior segment was indicated in still another manner. Ten worms were selected which showed no marked tendency to turn normally to the right or to the left when allowed to move away from the strong daylight. One side of the head segment of each of these worms was then painted with a black opaque varnish. In 5 worms the right side was painted and in 5 the left side was so treated. In approximately 4 out of 5 cases in over 100 trials painted worms turned so as to bring the black varnish pigment spot toward the source of light. That is, if the left spot was varnished the worm would turn to the left, and if the right side was varnished the worm would turn to the right. The varnish used was made of collodion and lampblack. A number of worms were painted with a transparent coating of collodion on one side of the head, but this had no effect that could be observed on their behavior. It is reasonable to suppose, therefore, that the absence of light produced by the black varnish was the cause of the behavior noted above. Worms with both sides of the head painted black when stimulated went with apparent indifference to the right or to the left.

The paths inscribed on smoked paper by a number of larvae were also studied. This method gave an accurate tracing of the path of the worm if the paper was not too densely smoked, and seems to be a better method for the type of behavior under consideration than would have been Mast's procedure of drawing with a fine pen in white ink upon the black paper the course of blowfly larvae, a result which he accomplished by keeping the pen about a centimeter in front of the organism.[8]

When a worm was moving in a direct path and a light was suddenly flashed on to the right or left of the animal, it was found that in practically all cases a curve would be described in which the animal oriented itself away from the new source of light. In these cases the light of the bulb at the rear was extinguished when the new source of illumination was turned on.

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[6] Loeb, *Forced Movements, Tropisms, and Animal Conduct*, 1918, 68 f.
[7] Cf. A. Forel. *Das Sinnesleben der Insekten*, 1910, 45-79.
[8] S. O. Mast. *Light and the Behavior of Organisms*, 1911, 176 ff.

At present it is not possible to say with absolute assurance that all of the responses noted were to light and not to heat. It seems to the present investigators, however, improbable that heat reactions were involved to any great extent in the experiments recorded. This tentative conclusion is based upon the following observations: (1) There seems to be little reason why there should be a noticeable difference in the heat reactions of the larvae which had one side of the anterior segment painted with black varnish and those painted with translucent varnish. (2) The source of light used in these experiments was not such as to produce much heat. (3) We were able to show, moreover, that the worms will move away from light and into warm areas. A copper pipe full of live steam was arranged at right angles in front of a piece of glass tubing. The worms were placed in the opposite end of the tube and due to properly arranged light stimulation they moved forward in the tube. They continued this advance within a few mm. of the hot pipe before stopping. In some cases they even touched the hot pipe itself. This experiment seems to suggest, as have the other observations noted above, that in the present settings the worms are much more sensitive to light than to heat.

The worms that moved into the lighted alley in every case stopped before touching the lighted bulb and in all cases after a short delay began to back out of the lighted alley.

THEORETICAL

May *Tenebro molitor* be characterized on the basis of the observations noted above as a negatively heliotropic organism? The answer to this question obviously depends upon the definition of the word tropism that is adopted. Mast has listed fifteen tropism "theories," or, as they might better be called, definitions of the word 'tropism.'[9] In the table printed above the fluctuations of the response of the organisms are indicated and yet it is shown that the responses of the meal worm are in the main such that they will cause the animals to move away from the light. As Parker has said: "Anyone who watched carefully the orientation of an earthworm or a fly larva in a field of light will have noticed the many fluctuating movements of the head made by these animals during this process. As to the presence of these so-called trial and error movements there can be not the least question, but as to their significance there is, in the mind of the reviewer at least, great doubt; for in any animal that orients more or less directly these movements are apparently always subordinate to a principle essentially tropic."[10]

According to this view, then, there can be no doubt of the fact that the meal worm is negatively phototropic. The striking fact remains, however, that in approximately one-fifth of the cases, when all of the trials of many worms are averaged together, the response is not away from the light. Two of the

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[9] Mast, *op. cit.* 53-57. This author also notes in a later publication that two other theories have been added to the list; *idem*, Discussion of certain questions raised by Parker in a review of "Light and the behavior of Organisms," *J. Animal Behavior*, 2, 1912, 209f.

[10] G. H. Parker, Mast's "Light and the behavior of organisms," *J. Animal Behavior*, 1, 1911, 462.

many possible explanations of this fact may be suggested here. First, in the main experimental series in the 'T' maze certain worms were used which had in the preliminary series shown a marked tendency to turn to the right. In such cases, if the right alley was illuminated some of these strikingly 'right-handed' organisms tended to follow their peculiar proclivity even though such turning lead them into the light. Secondly, the light of the maze may not have been strong enough to stimulate certain worms when some peculiar morphological change had taken place in their constitution, as, for example, abnormal thickening and hardening of the chitin of the head segment. In no case, however, as noted above, was a worm observed to touch the lighted bulb. Moreover, the activity of the larvae in the experiments noted above in which a pencil of light was turned upon one 'eye spot' was quite uniform and they might almost be characterized as negatively heliotropic in the strict sense of that term as used by Loeb, for they seemed in almost all cases compelled to turn and move away from the source of light.[11]

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[11] Loeb, *Forced Movements, Tropisms, and Animal Conduct*, 1918, 48.

EFFECTS OF GENERAL DISTRACTION ON THE HIGHER THOUGHT PROCESSES[1]

By H. Birnet Hovey, University of Oregon

"I have long held the opinion," wrote Schopenhauer, "that the amount of noise which anyone can bear undisturbed stands in inverse proportion to his mental capacity, and may therefore be regarded as a pretty fair measure of it. . . . Noise is a torture to all intellectual people."[2] Although there has been considerable experimentation with the effects of distraction upon simple processes, nothing, to my knowledge, has been done to determine the effects of distraction upon the higher thought processes except by Tinker, who gave an intelligence test to 56 students under standard conditions and also during the intermittent ringing of two bells.[3] He found that the distractor neither aided nor hindered average performance, but that the better students seemed to be more hindered than the poorer ones. He also found indications of less dispersion under the distraction. As intelligence tests are generally thought to be measures of the capacity to solve problems, to handle abstractions, they may, then, be safely considered as measures of higher thought processes. To determine distraction effects it would therefore only be necessary to administer a reliable intelligence test under distraction conditions. Using this procedure the following problems have been investigated:[4] (1) effects of distraction on certain higher mental processes; (2) the relationship between susceptibility to distraction and intelligence as measured by the Army Alpha; (3) individual differences in ability to overcome distraction; and (4) how much, if at all, distraction affects the reliability of a mental test.

Cassel and Dallenbach found that distraction inhibited, lengthened, or facilitated the simple sensory reaction; the effect being dependent upon the conscious attitude of the observer during the distraction.[5] The passive attitude was conducive to constant sensory reaction of normal length; the active attitude to a slow and variable one. Morgan found in a study of the overcoming of distraction while pressing keys, that the initial effect of distraction was a retardation of speed which was later followed by accelera-

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[1] I wish to express my thanks to Professors Edmund S. Conklin and Howard R. Taylor for their valuable suggestions and guidance in the course of this investigation together with their reading of this MS.

[2] Quoted from Will Durant, *The Story of Philosophy*, 1926, 330.

[3] M. A. Tinker, Intelligence in an intelligence test with an auditory distractor, this JOURNAL, 36, 1925, 467-468.

[4] In connection with a study of the relation between distraction effects and extro-introvert tendencies, we found our data to be applicable to such an investigation as this.

[5] E. E. Cassel and K. M. Dallenbach, The effect of auditory distraction upon the sensory reaction, this JOURNAL, 29, 1918, 129-143.

tion.[6] Upon removal of the distractions the speed fell below normal. Greater nervous tension was evident. Morgan believed that the overcoming of distraction is due to lip movements which incidentally change the rate of breathing; the *S*s who made the best scores used more vocal movements and showed a higher breathing ratio. Noise was found to have a dynamogenic effect, which may be expressed as a general tendency to meet resistance with an extra output of energy. Morgan thought that this might possibly affect the higher mental processes. Other workers have found distraction to be a spur to attention. Tinker found that distraction, which consisted of a bell ringing each time a stylus touched the sides of the apparatus used for testing motor control, did not interfere with performance; but rather that it served as a spur to attention.[7] Tanner and Anderson found that the sounds of a tuning fork intensified the sensations of *S*s while trying to determine changes in colors which were presented.[8] Individual differences in ability to compensate for distraction seem not to have been experimentally determined. Dockeray, in an experiment in which *S*s discriminated sounds under auditory and electric shock distraction, found indications that differences in this ability could be determined, but does not presume to have measured them.[9] Tinker's finding of less ability to compensate by the more intelligent *S*s is based on too few cases to be conclusive.[10]

METHOD AND PROCEDURE

As a means of attacking the problems presented above, two groups of *S*s were used; one as an experimental group, and the other as a control. Each group was a section of the sophomore class in General Psychology. The experimental group numbered 171 *S*s and the control 123. Six tests of Army Examination Alpha, Form 8, were given under normal conditions to both groups. The same 6 tests of Form 7 were given under distraction conditions to the experimental group, and under normal conditions to the control group. The two forms were given one and one-half months apart in each instance.

The 6 tests from Army Alpha were presented to the *S*s in the following order: test 4—opposites; test 2—arithmetic; test 5—disarranged sentences; test 6—number completion; test 7—analogies; and test 8—general information. These 6 tests were selected and used in this particular order for reasons connected with another phase of the study to be reported later. The procedure in giving the Alpha tests was similar to that used in the Army,[11] except for such alterations in the preliminary statements as were necessary to fit the particular groups tested and the experimental conditions. In the instructions for Form 7 given under distraction the *S*s were told that there would be distractions and that they were to do their best regardless. None of the *S*s knew what the nature of the experiment was to be until the instructions were read. The timing and scoring were the same as used in the Army.

The experimental group met for the distraction experiment at its regular class hour, but in a special room containing 200 individual desks arranged in nine rows. One professor and 8 advanced students in psychology assisted. The first took charge of the testing. Four of the students assisted in the operation of the mechanical distractors. The other 4 students served as distractors themselves by performing specified stunts. The distractors used were: 7 bells and 5 buzzers;[12] 5500-watt spotlight;[13] 90,000-volt rotary spark gap;[14] phonograph; two adjustable organ pipes and three metal whistles, 14, 24, and 26 in. long;[15] 55-pound circular saw (36 in. in diam.) mounted on a wooden frame; mounted camera operated by a well-known photographer; and the four students performing stunts.

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[6] J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 5, 1916, no. 35, 1-84.

[7] M. A. Tinker, A study of the relation of distracted motor performance to performance in an intelligence test, this JOURNAL, 33, 1922, 578-583.

[8] A. Tanner and K. Anderson, Simultaneous sense stimulations, *Psychol. Rev.*, 3, 1896, 378-383.

[9] F. C. Dockeray, Attention, distraction and fatigue, *J. Comp. Psychol.*, 2, 1922, 331-370.

[10] Tinker, *op. cit.*, this JOURNAL, 36, 1925, 467 f.

[11] C. S. Yoakum and R. M. Yerkes, *Army Mental Tests*, 1920, 63-69.

Several distractors were often used simultaneously. Sometimes but one or two were used, and at other times there were intervals of complete silence. More than one form of distraction was used during each of the six tests, and each form was used more than once throughout the experiment with the exception of the photographing. The production of a climactic effect was sought for; more than half of the various forms of distraction were used sometime during the last test. The intervals of quietude distributed throughout the experiment were usually followed by novel forms of stimuli to heighten the effect of the silence periods that were to follow. One assistant operated a keyboard (in a laboratory room hidden from the *Ss*) connected with the bells and buzzers. A small bell would ring in one part of the room followed by perhaps a buzzer in another part, then a very sonorous bell in still another place, and so on. The spotlight was flashed about during most of the tests. One assistant produced discordant noises with the rotary spark gap by pressing on a key. *Mary Lou* was played on the phonograph during part of the test and *Petrushka* during part of another. The organ pipes and whistles were operated by two assistants. Sometimes major chords were played and at other times discords. The circular saw was first beaten with a wooden club and later battered with iron bars—rhythmically for intervals and then unrhythmically. The camera was carried into the room and adjusted while the *Ss* worked on the last tests. Some instructions were then given the cameraman, to make sure that the *Ss* would be aware of the fact that a photograph was to be taken. With the exception of the bells and buzzers, all the distracting apparatuses were carried into the large room while the *Ss* were working. The phonograph, the pipes and whistles, and the saw, were first used in the adjoining room out of sight of the *Ss*. Each of these were also sounded while being carried into the large room.

During the second test of Alpha the 4 stunt men marched about, keeping step, on the floor above. Some of the *Ss* said afterwards that they interpreted the noises as being produced by a number greater than 4. This illusion was due to the passing of the sounds through a partially soundproof floor. Later the stunt men walked into the room unusually attired, and marched up and down the aisles. During the last test they walked into the room carrying formidable looking instruments which were unfamiliar to the *Ss*. The assistants pretended to make use of these. After every aisle was walked once the men returned to the small room and came back, each carrying a nail keg. They rolled the kegs down to the ends of the aisles, picked them up, and carried them to the front. This was continued until all the aisles were used several times.

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[12] On the moulding of the special room were placed 5 buzzers and 7 bells approximately equally spaced. The bells were of the following dimensions: one was 4 inches in diameter, one was 12, one was 6, two were 8, and two were 2½. The buzzers were all the same size but varied in pitch. These bells and buzzers were so distributed that there was not a concentration of similar noises in any part of the room; 12 volts d.c. were used to operate them. A common ground wire led to each. From the other terminal of every instrument a separate wire led to a keyboard in an adjoining laboratory room.

[13] The spotlight was used in the rear of the room so that the beams could be thrown on the walls, on the backs of the *Ss* and on the furniture, thus preventing them from shining in the *Ss'* eyes. The focus of the light could be reduced to a small diameter or spread over a large area. One man attended to this machine throughout the test but operated it only for intervals, according to instructions given him.

[14] The rotary spark gap is an apparatus which makes an unusual crackling noise which was easily heard throughout the room.

[15] The organ pipes and whistles were attached to a source of compressed air that was constantly regenerated. The pitch of these could be varied by moving their pistons.

In order to make the experimental and control groups comparable, individuals of one group were paired off with individuals of the other. The pairing was done on the basis of raw score totals individually obtained on Alpha, Form 8. For each score obtained in the control group, a score of similar size obtained by an individual in the experimental group, was sought for. Whenever a score in this second group could be found that differed two points or less from the score under consideration in the first group, these two scores were isolated as a pair. Each of the 123 individual scores in the control group were thus considered. Ninety out of this number could be successfully matched with 90 of the 171 scores in the experimental group. Thus 90 pairs were isolated, there being in no case a difference of more than two points between members of a pair. The correlation between the two paired sets indicates the closeness of the pairing: $r = 0.999 \pm 0.0$.

RESULTS AND DISCUSSION

(1) The data obtained indicate some results with reference to efficiency in thinking. It was believed that if individuals are disturbed by distraction they would make scores lower than they would otherwise. The mean score of the experimental group on Alpha, Form 8, given under quiet conditions was 127.5 ±1.3; under distraction this was raised to 133.9 ±1.2 on Form 7. The control group made a mean score of 127.5 ±1.3 on Form 8, and one of 137.6 ±1.2 on Form 7. The improvement under distraction was 6.4 as compared to 10.1 under normal conditions. Thus the control group did better than the experimental by 3.7 ±1.7. The difference is probably significant but not large enough to warrant the inference that distraction greatly impedes thinking. It shows, conversely, that distraction is not a serious handicap to the group as a whole during 19 min. of concentrated thinking (19 min. were spent in actual work on the test problems). A longer and more exacting examination might, however, show much greater effects. Undoubtedly the onset of fatigue from the added output of energy, which Morgan found to be the situation in the overcoming of this distraction, would appear later if the testing were prolonged. But then one might be measuring the effects of fatigue on ideational processes, rather than the direct effects of distraction; and also perhaps indirectly measuring fatigue produced by distraction. Although the *Ss* in our experiment were under considerable stress during the 19 min., there may have been compensating factors. The results on the arithmetic test revealed that it is possible for even problem solving to be performed under a bombardment of irrelevant stimuli without much loss in accuracy and speed. The mean scores for the experimental and control groups on this test were, respectively, 11.4 and 11.3 on Form 8; and 12.2 and 12.7 on Form 7.

Comparison of total score averages of the two forms of Alpha indicate that distraction does not raise the level of total output for the group taken as a whole. Compensation evidently does not reach a stage of overcompensation. If an individual works to his full capacity on a mental test perhaps nothing can make him work better, though if he is not maintaining his maximum the distraction may serve as an incentive. This is the only way the writer can account for one finding. The correlation between the two forms of Alpha is higher within the experimental group than it is in the control. The two are, respectively, $r=0.87$ and $r=0.84$. It is not altogether unreasonable to suppose that the reliability of one measure as based on another is higher if incentive is maintained at an optimum in either or both of the tests than it would be otherwise. If two similar forms of a test can be considered perfect measures of the same traits, the correlation should be perfect, and the nearer the correlation between such measures approaches 1, the more nearly may the tests be considered true measures of the traits. To the extent that the difference between 0.87 and 0.84 is significant, we are justified in concluding that distraction spurs students to a performance which is nearer to their true ability. Now with two groups so exactly paired that the r between the paired individuals on the same form of Alpha is 0.999, we found that the difference between the rs in the two groups has some significance. The significance of this difference may be determined by the standard deviation of such differences; which was found to be 0.02.[16] The obtained difference, which is 0.03, divided by the standard deviation is 1.5. On the basis of 0.02 as sigma on a distribution curve of these differences, 1.5 would include about 86% of all possible differences, leaving 14% of possibilities beyond this point. Hence there are 86 chances to 14, or a little better than six chances to one, that the true difference is greater than zero, or that students more nearly approximate their maximum ability under distraction.

(2) We found that the more intelligent are as capable of thinking under distraction as the less intelligent, contrary to Schopenhauer's thesis. In fact, there are slight indications of the converse. This was shown when Alpha initial scores (intelligence scores) were compared with gain, or increase in score in the second test over the first, under distraction and under normal conditions. The relationship between intelligence scores and gain in Form 7 under distraction, over Form 8 was, $r= -0.31 \pm 0.06$ (experimental) and only $r= -0.38 \pm 0.06$

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[16] For the method of computation see T. L. Kelley, *Statistical Methods*, 1923, formulae 128 and 139.

(control) under normal conditions. It should be said at this point that the negative coefficients are not infallible indications of a negative relationship between gain and initial ability. Thorndike explains that in general, imperfection in the original measures will distort the relationship between true initial ability and true gain making it more negative.[17] Past estimates of this relationship, he says, are all underestimates. We believe that the negative coefficients we obtained were due to the greater difficulty of originally high scores being exceeded. We are concerned here, however, only with relative gain in the two groups.

(3) Since the correlation between Forms 8 and 7 in the experimental group is not lower than it is in the control, the situation must be that shifts in rank under distraction are no greater than they are under normal conditions. This indicates that individual differences in ability to resist distraction are not very great. The spread in the group tested under distraction is no greater than it is in the other group. The standard deviation of the control group on Form 7 is $\sigma = 17.23 \pm 0.87$, and $\sigma = 16.71 \pm 0.84$ for the experimental. The comparative range of improvement in Form 7 for the experimental group is also smaller than it is for the control. The standard deviations of the differences between Forms 8 and 7 (gains and losses) for the experimental is $\sigma = 8.93 \pm 0.45$, which can be compared with that of the control, $\sigma = 10.48 \pm 0.53$. So the variability in the amounts of improvement was actually less under distraction than it was otherwise. Thus 19 min. of testing the higher thought processes under distraction yields no evidence at all of individual differences in susceptibility to distraction. Even with sufficient nervous strain to cause crying spells afterwards, in the cases of two girls, there was average improvement in one test score, and better than average in the other.

(4) Our results bring out another fact which is at least interesting. Quiet and freedom from disturbance are supposed to be necessary features of good test procedure because voluntary attention is an important factor in all so-called intelligence tests. If there are no disturbing effects of distraction there is little need of guarding against it in mental testing procedure. If there are effects, they must, of course, be considered in the testing technique. Attention has been drawn to the fact that distraction actually raised the reliability of Alpha. Some significance, as has been mentioned, can also be attached to the difference between the sigmas of gains and losses in the two paired groups. The difference between these sigmas is 1.55.

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[17] E. L. Thorndike, The influence of the chance imperfections of measures upon the relation of initial score to gain or loss, *J. Exper. Psychol.*, 7, 1924, 225-232.

The sigma of such differences is 1.03, making it about 86 possibilities in 100, or better than six chances to one, of the true difference being greater than zero. When true ability within a group is measured, the spread is smaller than it would be otherwise. Therefore it can be said that perhaps testing under distraction in our experiment approached the measurement of true ability to a greater extent than testing under standard conditions. It is more consistent to explain this as being due to the induction of an increased incentive by the experimental situation rather than as a direct effect of the distraction.

SUMMARY

Some effects of general distraction on higher mental processes have been experimentally investigated. This was done by giving a mental test to a group of college students under standard conditions and a re-test under severe general distraction. A control group was given the same tests without the use of distraction. The following inferences seem justified from the data: (1) Higher mental processes are comparatively unimpeded by distraction. (2) Intelligence is not related to susceptibility to distraction. (3) There are no individual differences in susceptibilty to distraction. (4) True mental ability is more nearly approximated under distraction than under standard conditions.

THE RELATION BETWEEN DIFFERENT MEASURES OF ABILITY TO REPORT

By John A. McGeoch, Washington University

It is difficult to evaluate work in the psychology of testimony without knowing the extent of the correlation between different methods of reporting upon the same material, and between similar methods of reporting upon different materials. In order to work out these relationships, Pearson product-moment coefficients of correlation have been computed, in this paper,[1] between the narrative and interrogatory forms of report on each of three different report materials, and between narrative and narrative, interrogatory and interrogatory, on the three materials. These materials were the Binet object-card, a picture, and an event.[2]

The most significant scores for this purpose are total items right and wrong. The correlations between these scores on narrative and interrogatory are given in Table I. Each age group has been treated separately.

Table I

Correlations Between Narrative and Interrogatory

Age	Total Items Right			Total Items Wrong		
	Object-card	Picture	Event	Object-card	Picture	Event
9	0.397	0.000	0.397	0.304	0.073	0.126
10	0.157	0.196	0.419	0.341	0.127	0.118
11	0.346	0.194	0.278	−0.077	−0.074	−0.041
12	0.404	0.385	0.185	0.205	0.361	−0.021
13	0.352	0.258	0.633	9.221	0.273	0.177
14	0.454	0.352	0.658	0.224	0.042	0.211
Mean	0.351	0.230	0.428	0.203	0.133	0.095

The coefficients of correlation between items right on narrative and interrogatory are all positive, with the exception of one which is zero; they range from 0–0.658. Their probable errors have been computed, and all of the coefficients save a few of the smallest are four times their probable errors. The mean for each type of report material shows in a single figure the general tendency of the results. In order of increasing size, the

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[1] From the Psychological Laboratory of the University of Chicago.

[2] A description of these report materials and of the experimental procedures used will be found in J. A. McGeoch, The fidelity of report of normal and subnormal children, this Journal, 36, 1925, 434-445. The numbers of *Ss* and their intellectual status have been given in a preceding paper on age and sex differences, this Journal, 40, 1928, 458-466.

means run: picture, object-card, event. The means represent the general tendency with some accuracy and indicate that there is a low but definite tendency for the scores to be related positively.

There are evident variations with age in the size of the coefficients. These may be due to too many chance factors to warrant attention here, since the main problem is not age differences in correlation but the general tendency of relation between different report methods. Suffice it to note a slight tendency for the size of the coefficients to increase with age.

The coefficients of correlation between items wrong are much lower than those between items right. They are, with a few exceptions, positive. The means run in order of increasing size, event, picture, object-card, but they are so small as to indicate a negligible relationship.

TABLE II

CORRELATIONS BETWEEN REPORT SCORES: ITEMS RIGHT

	Narrative			Interrogatory		
Age	Object-card	Object-card	Picture	Object-card	Object-card	Picture
	Picture	Event	Event	Picture	Event	Event
9	0.470	0.000	0.211	0.232	0.303	0.511
10	0.442	0.293	0.072	0.296	0.204	0.369
11	0.372	0.276	0.237	0.217	0.051	0.217
12	0.395	0.268	0.664	0.280	0.246	0.122
13	0.284	−0.485	0.853	0.116	0.092	0.161
14	0.467	0.104	0.554	0.489	0.371	0.564
Mean	0.405	0.076	0.431	0.271	0.211	0.324

Partial correlations have been computed, with intelligence held constant, with the result that the size of the coefficients is reduced, but only slightly.

Coefficients of correlation between similar methods of reporting upon different report materials are given in Table II.

The coefficients of correlation between the object-card and picture narratives, at every age, indicate a positive and in most cases a moderately high relationship. The object-card correlates much lower with the event, however. All of the coefficients are low and positive, with the exception of one deviation toward definite negative relationship at age 13. The relation between picture and event narratives is somewhat higher than any of the others, particularly in the later years of the age range.

The coefficients of correlation between the interrogatories indicate, at practically every age, a positive but low relationship. The means run, as do those for the narratives, in increasing order of size: object-card with event, object-card with picture, picture with event.

When intelligence is 'partialled out,' the narrative coefficients are not significantly reduced, while the interrogatory coefficients suffer an average reduction of 0.11.

It remains only to present in Table III the relations between error scores in similar methods of reporting.

The coefficients of correlation between object-card and picture narratives are all positive and indicate mainly low relationship. Much the same is true for the correlation between object-card and event. The relations between picture and event narratives are, with one exception, low; three of them are negative.

TABLE III

CORRELATIONS BETWEEN REPORT SCORES: ERRORS

	Narrative			Interrogatory		
Age	Object-card	Object-card	Picture	Object-card	Object-card	Picture
	Picture	Event	Event	Picture	Event	Event
9	0.227	0.412	0.169	0.408	0.344	0.390
10	0.264	0.203	−0.035	0.487	0.378	0.392
11	0.394	0.168	−0.081	0.413	0.084	0.412
12	0.352	0.595	−0.159	0.369	0.403	0.316
13	0.306	0.054	0.579	0.297	0.292	0.205
14	0.074	0.375	0.157	0.439	0.333	0.435
Mean	0.269	0.301	0.105	0.402	0.305	0.358

The correlation between interrogatory errors is uniformly higher than that between narrative errors, and indicates a definite relationship. There are few large deviations in the size of the coefficients at different ages, and the means may be taken as fair representatives of the relationships. In an increasing order of size these are: object-card with event, picture with event, object-card with picture.

The 'partialling out' of intelligence has a slight lowering effect on practically all of the coefficients, both for narrative and for interrogatory errors.

CONCLUSIONS

(1) The correlation between the narrative and the interrogatory forms of report upon the same material, measured in terms of items right, is positive but mainly low, with the amount of the relationship appearing as a function of the material upon which report is made. The correlation between the two forms of reporting, measured in terms of items wrong, is positive but negligible.

(2) When similar methods of reporting upon different report materials are intercorrelated, the following results appear: (a) Considering the narrative form of report, the relations between

object-card and picture, and between picture and event are positive and moderately high. The relation between object-card and event is negligible. (b) Considering the interrogatory form of report, the relationships between reports on the three materials are all positive but low, the means of the coefficients running in increasing order of size: object-card with event, object-card with picture, and picture with event.

(3) Narrative errors are related to a very small degree, but the relations are mainly positive. Interrogatory errors are related positively and to a moderate degree. They are more highly related than are narrative errors.

(4) The ability to report, in the light of the facts stated, seems to be largely a function of the material upon which the report is made and of the method of making the report. There is a small degree of correlation remaining between performances under these various conditions, even when intelligence differences are removed. The fact that relationships are so low can only indicate that the ability which has been measured in this experiment is largely a specific function, or otherwise stated, that the reliability of diverse report experiments is very low. Evaluation, therefore, of results upon sex and age differences, educability of report, and the like must carefully take into account this specificity of report ability both to methods and to materials.

INTELLIGENCE AND THE ABILITY TO REPORT

By JOHN A. McGEOCH, Washington University

Two investigations have indicated that the reports of children of normal intelligence are distinctly superior to those of children of subnormal intellectual status.[1] In one of these studies[2] it was found that the correlation between intelligence scores and correctness of report tended to be either zero or slightly inverse with a selected group of normal children and moderately positive with a selected group of *Ss* of equal chronological age but three years lower in mental age. The present study[3] presents the correlations between intelligence scores and various measures of ability to report on observed material, using as subjects the total population of each age from 9 to 14, found in the 4–8 grades in schools for supposedly 'normal' children, from which no selection save of the extremely subnormal children has been made. The report materials used were the Binet object-card, a picture, and an event.[4] The report scores which are in correlation with intelligence scores are the total number of right and wrong items and the percent of attestation. The intelligence measures are the National Intelligence Tests, Scale A, Form 1, and Army Alpha, Form 5, combined by adding algebraically their sigma standings and dividing by two.

In Table I are given the coefficients of correlation between items right and intelligence scores.

There are large gross differences between the coefficients at different ages and between those for different report materials at the same level. While these variations from age to age are too large to make a mean highly significant, the mean may serve as a convenient single expression of the tendency of the coefficients. Probably the most significant fact is that the means agree so closely in magnitude, thus indicating the presence of a small positive relationship between intelligence and correctness of report.

The coefficients of correlation between items right on the interrogatory and intelligence are given in Table II.

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[1] T. H. Pear and S. Wyatt, The testimony of normal and mentally defective children, *Brit. J. Psychol.*, 6, 1914, 387-419; J. A. McGeoch, The fidelity of report of normal and subnormal children, this JOURNAL, 36, 1925, 434-445.

[2] McGeoch, *op. cit.*

[3] From the Psychological Laboratory of the University of Chicago.

[4] These materials and the experimental methods used have been described by the writer, *loc. cit.* The number and intellectual status of the *Ss* are given in a preceding paper on age and sex differences, together with some slight variations in the experimental methods. *See* this JOURNAL, 40, 1928, 458-466.

TABLE I

CORRELATIONS: NARRATIVE, ITEMS RIGHT, WITH INTELLIGENCE

Age	Object-card		Picture		Event	
	r	P.E.	r	P.E.	r	P.E.
9	0.224	.066	0.254	.060	0.360	.069
10	0.258	.048	0.153	.054	0.268	.059
11	0.495	.042	0.177	.058	0.048	.057
12	0.397	.052	0.366	.056	0.271	.060
13	0.386	.068	0.342	.067	0.438	.066
14	0.035	.097	0.410	.080	0.380	.080
Mean	0.297		0.283		0.294	

TABLE II

CORRELATIONS: INTERROGATORY, ITEMS RIGHT, WITH INTELLIGENCE

Age	Object-card		Picture		Event	
	r	P.E.	r	P.E.	r	P.E.
9	0.705	.035	0.309	.057	0.296	.073
10	0.419	.043	0.436	.045	0.232	.061
11	0.334	.049	0.159	.059	0.228	.055
12	0.456	.049	0.339	.057	0.328	.057
13	0.353	.060	0.433	.061	0.459	.065
14	0.368	.082	0.241	.091	0.056	.095
Mean	0.439		0.319		0.266	

All of the coefficients are positive, and most of them are high enough to indicate a definite relationship. If we take the mean of all ages to represent the central tendency, the different materials yield correlations with intelligence in increasing order, thus: event, picture, object-card. It is clear that the degree of the relationship is a function of the material placed in correlation with intelligence and that, with the exception of the event, the interrogatory yields a higher correlation than does the narrative.

The coefficients of correlation between items wrong and intelligence for both narrative and interrogatory are shown in Table III, which omits the probable errors.

TABLE III

CORRELATIONS: ITEMS WRONG, NARRATIVE AND INTERROGATORY, WITH INTELLIGENCE

Age	Narrative			Interrogatory		
	Object-card	Picture	Event	Object-card	Picture	Event
9	−0.141	0.239	0.323	−0.415	−0.119	−0.260
10	−0.086	−0.005	0.258	−0.315	−0.180	−0.061
11	0.105	0.139	0.074	−0.471	−0.318	−0.230
12	0.121	0.115	0.149	−0.434	−0.379	−0.386
13	−0.116	0.084	−0.149	−0.263	−0.195	−0.305
14	−0.508	0.308	−0.277	−0.486	−0.055	−0.224
Mean	−0.104	0.146	0.063	−0.397	−0.207	−0.224

Both individual age coefficients and the means of all ages indicate a negligible relation between narrative errors and intelligence: the tendency with the object-card is slightly negative, that with the picture is slightly positive, and that with the event is positive in the earlier years of the age range and negative in the later years. Errors in interrogatory report bear a consistently negative relationship to intelligence; the amount of the relationship varies with the material upon which the questions are asked, in increasing order of magnitude, thus: picture, event, object-card.

The percentage of items attested represents the assurance of the *S* that his report is correct. This is, presumably, a factor different from those involved in the type of immediate memory involved in the making of a simple report upon observed data. It is particularly interesting, therefore, to know whether or not it is related to test intelligence. Table IV contains the correlations between test scores and percentages of attestation.

TABLE IV

CORRELATIONS BETWEEN ATTESTATION AND INTELLIGENCE

Age	Narrative			Interrogatory		
	Object-card	Picture	Event	Object-card	Picture	Event
9	−0.195	0.020	−0.043	0.087	−0.014	−0.104
10	−0.036	0.083	0.090	−0.078	−0.085	0.150
11	0.008	0.089	0.147	0.072	−0.111	−0.037
12	0.101	−0.056	−0.218	0.045	−0.162	−0.219
13	0.272	0.181	0.145	0.149	−0.153	0.177
14	−0.207	−0.148	0.191	−0.121	0.053	−0.092
Mean	−0.009	0.028	0.052	0.025	−0.078	−0.020

It is evident that there is no consistent positive or negative relationship throughout the different age levels, with the slight exceptions of the picture and event interrogatories. Means are hardly fair representatives here, but, whether we consider them or the individual coefficients at each age, it is clear that there is virtually zero relationship between percentage of attestation and intelligence, with both narrative and interrogatory forms of report. It is probable that we have in attestation a 'personality' factor. In any case, it is a trait not related to test-intelligence.

CONCLUSIONS

(1) Total items right on the narrative uniformly correlate positively, but slightly, with intelligence. Total items right on the interrogatory correlate with intelligence positively and somewhat more highly than do those on the narrative.

(2) Total errors in narrative report show no significant relation with intelligence. Interrogatory errors are correlated with intelligence negatively and to a low or moderate degree; the amount of the relationship is a function of the report material involved.

(3) Percentages attested are wholly unrelated to intelligence, no matter which of the report materials or of the reporting methods is considered. Attestation probably involves a 'personality' factor.

(4) It is clear that, in general, the relationship between intelligence and the ability to report is specific to three factors: the material upon which report is made, the method of making the report, and the method of scoring or measuring the report.

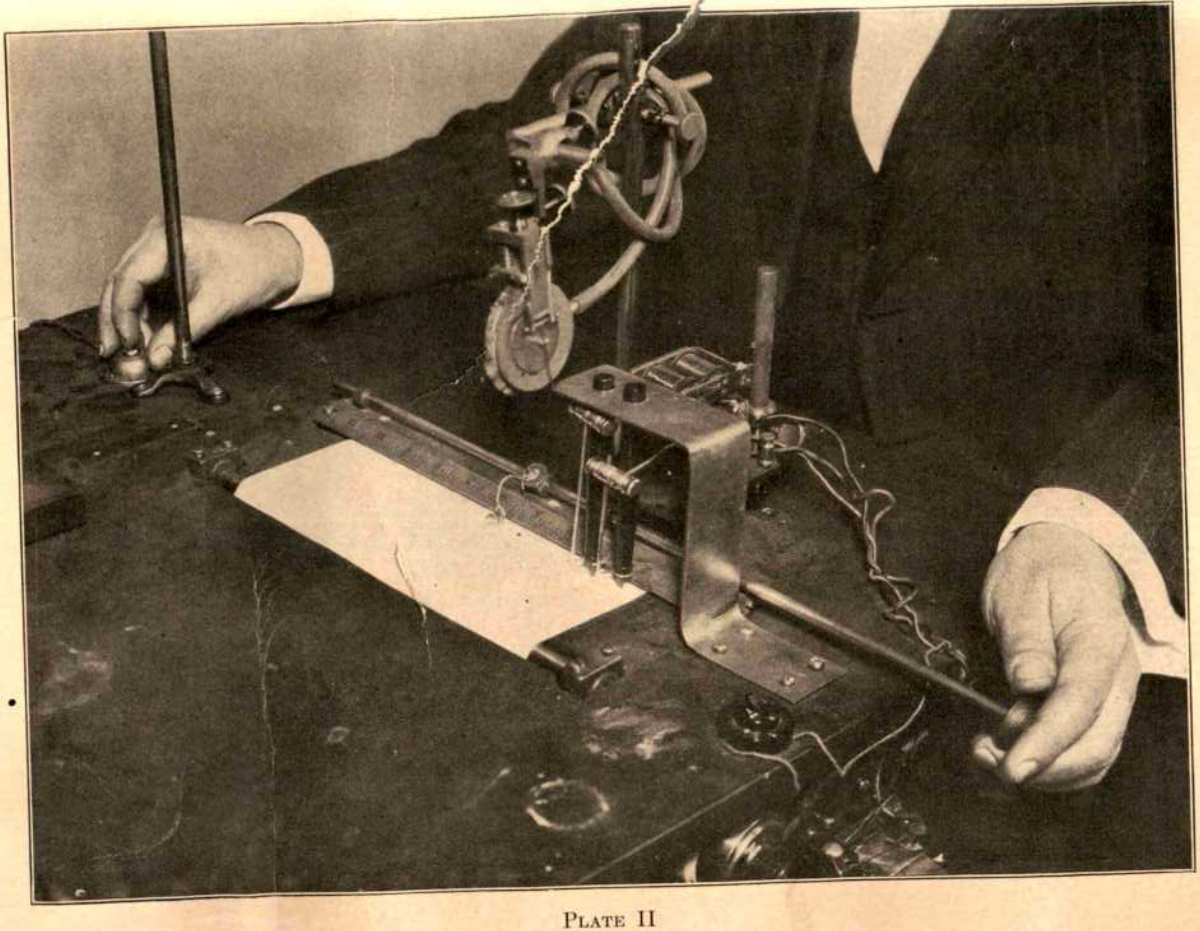
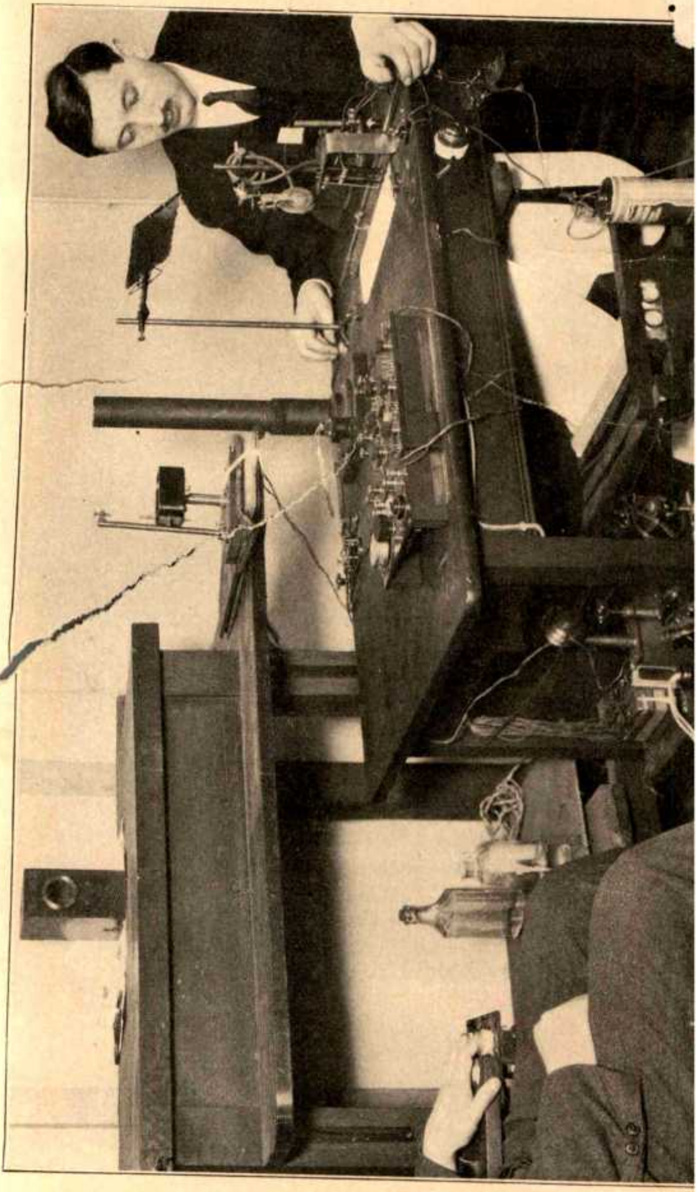


PLATE II



The object of our present investigation was to test out the position taken above and to obtain further data on the specificity problem. As in the cited experiments, our method of approach was through a study of self- and inter-correlations of the psychogalvanic response to emotionally stimulating situations.

METHOD AND PROCEDURE

Records were taken with 35 *S*s (a random selection of college undergraduates, with a few graduate students) employing a standard series of 40 stimuli. The galvanic reactions were obtained by means of a D'Arsonval moving-coil type of galvanometer and exsomatic current, with the subject inserted as unknown resistance in the fourth arm of a Wheatstone bridge. A standard current of approximately 0.0001 M.A. was employed with every subject, variations of body resistance being compensated for through the use of a potentiometer. The electrodes were applied to the fingers of the hand, and consisted of small clay cups filled with normal saline, resting in somewhat larger cups of saturated $ZnSO_4$. Into the saline solutions the *S* was required to place the index and second finger of his right hand. The fingers were immersed the full length of the third phalanx, the surface exposed to the liquid being kept constant by the use of fitted rubber finger cots.

The galvanometric excursions were recorded by a mechanical device designed to give continuous graphic records of each subject's reactions. The essential features of the apparatus are apparent in the accompanying photograph, Plate I. The beam of light coming from the galvanometer is intercepted by a plane mirror which is so inclined as to reflect the beam perpendicularly down upon a moving surface of white paper. The paper is being displaced at a very slow speed over a narrow plate from a large roll of paper below. Above the paper a small ink-syphon is mounted on a horizontal bar (see Plate II) which moves laterally across and at right angles to the paper. The *E* observes the beam of light and, as it moves, follows it with the ink-syphon in the direction of the beam's displacement. In this way excellent graphs of the galvanometric excursions may be obtained, which compare favorably with direct photographic records. An advantage of this technique consists in the immediate visibility of the records, and in the opportunity to make accompanying notes on the same sheet. Further, the deflections can be magnified to a greater extent than in ordinary work with sensitive paper or film, since it is possible to use kymograph paper 9 in. in width. Errors, due to lag or slight deviations of the *E* in following the course of the light, are negligible save where very precise measurements of latent period are desired. Various electric markers can be readily attached, and two such are shown in the photograph, one connected with a metronome to mark seconds, and another to indicate moments of excitation.

The experimental situation was simplified as far as possible. The *S* was asked to seat himself in a chair equipped with an arm rest and electrodes. While he was making himself comfortable, he was shown how to keep his fingers in the saline-filled cups and given brief instructions as to what was to follow. The *S* had a general idea as to the nature of the experiment but knew nothing of its actual content. There were two *E*s, one who controlled the apparatus and another who gave instructions and presented the stimuli. The apparatus and the first *E* were at all times screened from the *S*.

TABLE I

CHARACTER AND ORDER OF STIMULI USED

Stimulus No.	Nature of Stimulus
1	Unexpected noise (turning on of motor).
2	Verbal Instructions: "I am going to read you some numbers. When I am through I want you to repeat them backwards."
3–6	Four series of digits to be repeated backwards.
7	Verbal Instructions: "I am going to show you some pictures on a screen. As soon as you see each picture say out loud the first word that comes to your mind."
8–11	Four colored pictures thrown on a Trans-Lux screen by means of an opaque projector. The pictures were advertisements from the *Saturday Evening Post.* Although chosen for a supposed provocativeness, their content proved to have little emotional value.
12	Verbal instructions: "Now I am going to say some words. You are to respond to each word by telling me the first word that comes into your mind."
13–29	Seventeen association words (one buffer and sixteen taken from Whateley Smith's series).[3] Eight of the words were chosen for minimal emotional value, and eight because of their position at the head of Smith's list. The former we termed "non-critical," the latter "critical." The two classes of words were given in alternate groups of 4 each.
30–32	Physical aggression: *E* approached *S* unexpectedly and grasped his nose between thumb and forefinger. Repeated once, and the third time merely simulated.
33–35	Loud auditory stimulus (klaxon sounded).
36	Adjustment of inductorium electrodes to *S*'s leg.
37	Verbal instructions: "Now I am going to give you an electric shock. At first it will be mild, but later it will be very severe. When you begin to feel the current say 'feel' and when you can't stand it any longer say 'stop.'"
38	Auditory stimulus (buzz of make-break on inductorium).
39	Inductorium shock. *S* reports 'feel.'
40	Inductorium shock. *S* reports 'stop.'

The above stimuli were given without undue interruption, and so far as possible in the same manner, to all *Ss.* Where the standard conditions could not be adhered to, the records were thrown out; this occurred in the case of 6 *Ss* (due to failure to follow instructions, interruptions from outside, or apparatus defects). The duration of an experiment varied from 20–30 min.; this variation was largely due to the extra time that had to be allowed to certain *Ss* between successive stimulations, owing to the character of their galvanic reactions. Thus, although we planned to allow only 20 sec. between the first and second digits tests, it was sometimes necessary to wait 20 or 30 sec. additional, owing to the occurrence of spontaneous or multiple reactions to the initial stimulus. In a sense, however, it might be said that the interval between stimuli was physiologically standardized, since it was determined by the time required for the achievement of stability in each individual's resistance.

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[3] W. Whateley Smith, *The Measurement of Emotion*, 1922, 76.

The reactions of the *S* were thus recorded as a continuous graph 6 or 7 feet in length, presenting a large number of undulations, the successive 'waves' of which represented the responses to the various stimuli in order of presentation (together with secondary responses.) The deflections were measured to the nearest millimeter and recorded in their proper place on a 40-fold table covering the total number of stimuli employed. Owing, however, to the fact that stimuli 36–40 (electric shock) were hard to control and in sensitive *Ss* often produced deflections beyond the edge of the paper, the results for these last 4 stimuli were not used in our calculations. The final data available from our experiments thus consisted of the psychogalvanic reactions of 29 adult male *Ss*, to a series of 36 distinct stimuli standardized for the given conditions of the experiment, and quantitatively comparable.

RESULTS

Our first step in the analysis of the data was to calculate individual correlations between galvanic reactions to any given stimulus with any other series in the series. We calculated 112 such correlations using the rank-order method (ρ converted into r).[4] The correlations were then grouped into 3 classes: (1) self-correlations, those between repeated or identical stimuli, *e.g.* between the first and second startle evoked by a klaxon; (2) intercorrelations between responses to similar stimuli, *e.g.* association words by pictures, or the sound of starting motor by sound of klaxon; (3) intercorrelations between responses to dissimilar stimuli, *e.g.* word association by aggression stimuli. The values for each group of coefficients were then averaged and compared, both as to magnitude and reliability.

TABLE II

SELF-CORRELATIONS

Stimuli	r	P.E.
Mental effort (Digit repetition stimuli 3,5 x 4,6)	0.81	0.05
Aggression (Stimuli 30 x 31)	0.80	0.05
Startle (First by second klaxon)	0.67	0.07
Average	0.76	

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[4] The writers are indebted to Mr. Herbert S. Conrad for this statistical work.

The reliability of the single items, or pairs of items, in tests of motor skill, memory or intellectual ability is seldom as high as the average found above. Yet such traits or abilities are commonly considered more stable and more subject to precise measurement than the emotional reactions with which we are concerned. Applying the Spearman-Brown formula, $r_{nn} = n \cdot r_{xy}/[1+(n-1)r_{xy}]$, the reliability of our psychogalvanic reflex measures for the total of 4 mental effort stimuli becomes 0.90; the reliability of the 3 responses to aggression is 0.92, and of the 3 startle responses, 0.83. This is almost certainly an exaggeration, however, for (1) the rapid 'exhaustion' of the psychogalvanic reflex when the same stimuli are repeated, cuts down the variability of the later measures, and hence reduces self-correlation; and (2) the first and second responses, given at any one time, may be supported by a temporary mood condition, yielding a higher correlation than if they were separated by a day or more. With due regard to these considerations, however, the consistency of the results remains higher than had been expected.

TABLE III

SELF-CORRELATION IN THE ASSOCIATION TESTS[5]

Stimuli	r	P.E.
Critical words (1st four x 2nd four)	0.55	0.08
Non-critical words (1st four x 2nd four)	0.53	0.09
Pictures (1st and 3rd x 2nd and 4th)	0.44	0.10
Average	0.53	

The absolute deflections and variability of the deflections to these stimuli are considerably less than are the responses to the mental effort, aggression and startle stimuli just discussed. Not a few *Ss* gave zero deflections to the uncritical words and pictures coming at or near the end of the series. As might be expected, the degree of self-correlation appears to vary with the emotional intensity of the stimulus.

The series of digits, the pictures, and the association words are similar in that they present to the *S* a situation, more or less simple, requiring the formulation of a verbal response. The emotional involvement is most marked when conflicts interfere with a prompt formulation. The sound of the electric motor and the klaxon warning, are also similar to each other in that they are loud and unexpected.

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[5] The association test data will be reported in more detail in a separate paper.

TABLE IV

INTERCORRELATIONS OF DEFLECTIONS TO SIMILAR STIMULI

Stimuli	r	P.E.
Word by picture association (Stimuli 8-11 x 13-29)	0.56	0.08
Word association by mental effort (3-6 x 13-29)	0.46	0.09
Picture association by mental effort (3-6 x 8-11)	0.55	0.08
Verbal directions (2 x 7)	0.25	0.11
(2 x 12)	0.43	0.09
(7 x 12)	0.68	0.08
Startle (motor by klaxon) (1 x 33-35)	0.46	0.09
Average	0.48	

TABLE V

INTERCORRELATIONS FOR DISSIMILAR STIMULI

Stimuli	r	P.E.
Startle (motor) by mental effort (1 x 3-6)	0.21	0.12
by picture association (1 x 8-11)	0.24	0.12
by word association (1 x 13-29)	0.03	0.12
by aggression (1 x 30-32)	0.15	0.12
Mental effort by aggression (3-6 x 30-32)	0.04	0.12
by startle (klaxon) (3-6 x 33-35)	0.13	0.12
Picture association by aggression (8-11 x 30-32)	0.21	0.12
by startle (klaxon) (8-11 x 33-35)	0.35	0.11
Word association by aggression (13-29 x 30-32)	0.11	0.12
by startle (klaxon) (13-29 x 33-35)	0.47	0.10
Aggression by startle (klaxon) (30-32 x 33-35)	0.44	0.10
Average	0.22	

The coefficients run considerably lower than in the previous groups, and the P.E.s are of course higher. Only two coefficients are more than four times their P.E.s. The higher correlation between aggression and startle, if due to other than chance features, might be considered as the outcome of a certain similarity of the stimuli; both are of a primitive and provocative character. The correlation between klaxon and word association cannot be attributed to this, but perhaps rather to the fact that in these two classes of stimuli the decrement due to exhaustion of the psychogalvanic reflex is more conspicuous than elsewhere: with the klaxon because of its intensity, and with the word association because of the extended number of stimuli used. Hence the correlation would be expected to be reënforced by individual differences in the rate of negative adaptation.

The predictive value of each class of stimuli for other classes is indicated by the average of the intercorrelations with each of the other classes. For the 5 principal groups under discussion these are as follows:

TABLE VI

THE PREDICTIVE VALUE OF STIMULI

Stimuli	r	k
Startle (klaxon) (33–35)	0.37	0.93
Word association (13–29)	0.33	0.94
Picture association (8–11)	0.32	0.95
Mental effort (3–6)	0.29	0.96
Startle (motor) (1)	0.22	0.97

The predictive value of dissimilar stimuli for each other is thus seen to be extremely low. Of the classes considered, the three sounds of the klaxon have the highest predictive value, but the differences possess slight reliability and in any case the coefficient of alienation is so great that the interpredictive functions can have no value.

SUMMARY

Correlations between psychogalvanic reflex deflections can be classified into three groups, the coefficients constituting a hierarchy with but little overlapping.

Self-correlations between repeated stimuli (*e.g.* two identical sounds) give an average of 0.76 (Table II).

Inter-correlations between stimuli imposing similar tasks (*e.g.* associations to two words, or to a word and a picture) average 0.50 (Tables III and IV).

Dissimilar stimuli (*e.g.* a loud sound *vs.* an association word) yield very low coefficients, averaging 0.22 (Table V). Correction for attenuation, if proper data were available, would raise cross correlations from 0.05–0.15 points. But under the conditions of the experiment, even the corrected intercorrelations would leave the coefficients of alienation from 0.80 upwards, indicating a high degree of specificity in the galvanic response.

Our data agree very closely with previous results obtained by Wechsler. The question that still remains is whether a similar specificity would be shown by other physiological indicators of emotion.

GALVANOMETRIC TECHNIQUE IN STUDIES OF ASSOCIATION

By HAROLD E. JONES and DAVID WECHSLER, University of California

During the past two decades recurrent use has been made of the psychogalvanic response in attempts to measure the affective values of words, in the detection of 'guilt,' and in clinical studies of emotional complexes.[1] The major portion of this work has been of a tentative and exploratory nature, with estimates of the method ranging from the very confident claims of Whately Smith[2] to opinions less wholly enthusiastic.[3] It would appear that progress has been slow, and that technical difficulties have discouraged many psychologists from continuing in this field.

It is of course true that the psychogalvanic response involves a delicate and complex procedure, with many variable factors having to do with the electrodes, the E.M.F., the area and location of the skin contacts, the coil suspension, and the processes of recording the deflections. Irregularities or lack of standardization at any one of these points may result in grave disturbance of the data. In the present report, the writers are concerned not with matters of apparatus technique which have been held as constant as possible,[4] but with the effect of alterations in the schedule of stimuli, and the influence of sequence and temporal position in series.

The stimuli employed were the following association words, given in the order shown with one half of our *Ss*,[5] and in the reverse order with the remaining half: table, work, carrot, white, money, afraid, dance, woman, glass, flower, swim, pencil, insult, marry, love, kiss.

The stimuli were derived from Whately Smith's list of 100 words, arranged by him in a rank order according to a weighted mean psychogalvanic response obtained in an association experiment with 50 *Ss*. Eight of the words (kiss, love, marry, insult, woman, dance, afraid, money) were taken from the 12 standing at the head of his list, and showing average deflections ranging—in terms of his recording system—from 72.8 to 35.6.

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*Accepted for publication November 18, 1927.

[1] This paper includes results reported in part at the 1926 meeting of the American Psychological Association, and abstracted in the *Psychol. Bull.*, 24, 1927, 173-174.

[2] "I believe it will be be clear to any reader . . . that this phenomenon [the psychogalvanic response] is by far the most delicate, reliable, and quantitatively accurate method at present known for detecting and measuring emotional changes." Whately Smith, *The Measurement of Emotion*, 1922, 25.

[3] *Cf.*, for example, J. A. Larson, The cardio-pneumo-psychogram and its use in the study of the emotions, with practical application, *J. Exper. Psychol.*, 5, 1922, 324.

[4] The writers' apparatus has been described in the preceding article *supra*, 601.

[5] The *Ss* were a random selection of male undergraduate and graduate students of Columbia University. Thirty-five *Ss* were tested, the reversed order of stimuli being used with every other *S*. Due to interruptions, or errors of technique, the total number of cases was reduced to 29 for the later statistical treatment.

The remaining 8 (pencil, swim, flower, glass, white, carrot, work, table) were chosen from the 12 at the bottom of the list, with deflections from 18.5–14.2. In subsequent discussion, words from the top of the list, having conspicuous emotional values, will be called 'critical,' while the others will be called 'indifferent' or 'non-critical.' The order of stimuli indicated in the above list, beginning with table, will be called A; the reverse order, given to alternate *Ss*, will be called B. In all cases the association words were preceded by a buffer word, 'salt;' the total series occurred in the middle of a stimulus schedule, the first part of which consisted of a picture association test and a test of repeating digits backward. When the association words were reached in this schedule, instructions were given: "Now I am going to say some words. You are to respond to each word by telling me the first word that comes into your mind." The stimulus words were given orally, separated by an interval (from 10–30 sec.) sufficient to permit the resistance of the *S* to return to its previous level after the psychogalvanic response. As each word was sounded, the *E* pressed a key which gave a stimulus signal on the continuous kymograph record. A second *E* (concealed from the *S*) tended the apparatus and made observations on qualitative aspects of the reactions. A reproduction of two representative responses by the same *S*, is given in Fig. 1.[6]

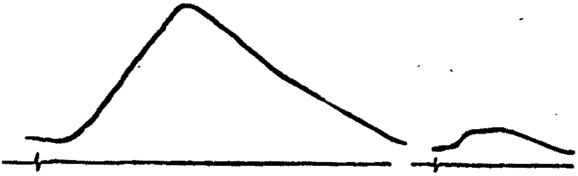

FIG. 1. REPRODUCTION OF TWO REPRESENTATIVE RESPONSES FROM THE SAME *S*

S: "afraid"
R: "not"

S: "flower"
R: "beautiful"

The deflections to each stimulus word were measured to the nearest mm. and measures of central tendency were computed. In Table I the stimulus words are arranged in the order of magnitude of the Whately Smith means for the same words, and the comparative data of the two investigations are listed.

The gross magnitudes obtained are of course relative to the arbitrary standard conditions of the experiment.[7] The two lists agree in the demarcation of critical and non-critical words, and in placing the sex words conspicuously at the top of the list. A correlation of 0.90 was obtained between the two rankings on the basis of the means, and of 0.95 when the second list is ranked on the basis of the medians. Another important comparison is in what Smith terms the resolvability or range of variability of the psychogalvanic response. The mean value of the word eliciting the largest deflection in Smith's list is 72.8, while the mean value at the other end is only one fifth as great. In the writers' data this ratio becomes about 3.5 to 1, but remains greater than the resolvability of reaction time (2.5 to 1) for the same words. This may be considered to be one of the advantages which the psychogalvanic response shows over reaction time, or for that matter over most of the laboratory indicators of emotion. Its wider relative variability allows for greater precision of measurement, and tends to increase the reliability of the results.

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[6] Owing to its length, an entire record cannot conveniently be reproduced here.

[7] The scale on which the deflections occur depends upon the reflecting system used, the E.M.F., the original sensitivity of the galvanometer, and the shunt.

Table I gives the average deflection for each word, combining the two orders A and B. If, however, we compute averages separately for the two orders of presentation, a striking difference appears.

TABLE I

SHOWING THE MEAN DEFLECTION FOR EVERY STIMULUS WORD OBTAINED BY SMITH, AND THE MEAN AND MEDIAN DEFLECTION OBTAINED IN THIS EXPERIMENT

Stimulus-word	Smith: Mean deflection	This Experiment: Mean deflection	This Experiment: Median deflection
kiss	72.8	11.46±1.04	9.50±1.30
love	59.5	10.69±0.96	8.33±1.19
marry	58.5	10.39±0.97	8.67±1.22
woman	40.3	7.96±0.92	7.67±1.15
dance	37.4	7.31±0.60	6.67±0.77
afraid	36.8	9.12±1.23	7.00±1.55
money	35.6	7.58±0.69	5.00±0.86
insult	32.5	8.65±0.75	7.50±0.94
table	18.5	4.23±0.54	4.00±0.68
work	18.3	6.85±1.10	4.50±1.37
carrot	18.2	6.70±1.20	3.33±1.50
white	17.8	4.69±0.60	3.00±0.77
glass	17.6	5.31±0.70	4.60±0.88
flower	16.5	6.12±0.72	5.00±0.90
pencil	15.4	4.65±0.60	3.25±0.77
swim	14.2	3.38±0.33	2.60±0.41

TABLE II

SHOWING THE MEDIAN DEFLECTION FOR THE TWO ORDERS IN WHICH THE STIMULUS-WORDS WERE PRESENTED

Stimulus-word	Order of presentation	A Median	Order of presentation	B Median
table	1	5.25±1.06	16	0.72±0.81
work	2	7.75±1.25	15	0.93±2.24
carrot	3	6.50±2.12	14	3.17±2.11
white	4	5.25±1.06	13	0.81±1.17
money	5	4.75±1.03	12	5.50±1.31
afraid	6	7.75±2.18	11	6.50±1.15
dance	7	7.25±1.11	10	6.25±1.05
woman	8	7.75±0.42	9	7.50±1.73
glass	9	4.88±1.41	8	2.50±0.99
flower	10	4.50±0.84	7	5.25±1.55
swim	11	1.50±0.39	6	2.88±0.87
pencil	12	1.50±0.93	5	3.83±1.25
insult	13	6.50±1.01	4	8.50±1.63
marry	14	8.25±1.32	3	10.50±2.00
love	15	8.75±1.90	2	7.50±1.37
kiss	16	8.75±1.32	1	10.50±2.14

Inspection of Table II immediately suggests the importance of position in series. The effect of adaptation is obvious, in spite of the fact that an attempt was made to minimize this by employing a buffer word, and by giving the association test in the middle of a much larger schedule of stimuli. The median[8] deflection for 'table' is 5.25 when it appears first in the list, and 0.72 when it appears last. The median deflection for 'work' is 7.75 when it appears second in the list, and only 0.93 when it is given next to the last. It will be observed, however, that in the case of the most 'critical' words the emotional value is not greatly negatived by a late position in the series. The words 'kiss,' 'love' and 'marry' give the highest median deflections whether they appear first or last in the list. In the A order the 'non-critical' words are given positions 1-4 and 9-12 in the list; the mean of the 8 medians is 4.64. The less favorably situated 'critical' words (positions 5-9 and 13-16) yield a mean of 7.47. If now we change to order B, the mean for the 'non-critical' medians drops nearly one half to 2.51, while the mean of the 'critical' words increases slightly (and unreliably) to 7.82. It is, then, established that the factor of position in series exerts a greater influence upon the words of slight emotional tone than upon those which already have a significant affective value. This relation is further emphasized in Table III.

Several of the above differences, particularly for the 'critical' words, have a very low reliability; nevertheless, of 16 differences, 13 are in favor of the earlier position in series, and the exceptions, in two of the cases at any rate, appear to be reversals due to the influence of emotional carry-over from contiguous words. Thus, the word 'glass' when it occurs 8th in series has a smaller deflection than when it occurs 9th, but in the latter case it occupies a post-critical position, following the word 'woman.'

The influence of position in series may be brought out in another manner. In the group of *S*s taking the A order of words, each individual's average deflection was computed for the 8 'critical' and for the 8 'non-critical' words; the correlation between these two variables was 0.61 (rho corrected to r). For the *S*s given the B order of words a practically identical correlation (0.64) was obtained. If now we consider the total group and correlate the critical by the non-critical averages, a similar r should be found, unless some disturbing influence has affected the comparability of the two groups. The obtained r, 0.23, may be considered as evidence for such a disturbing influence. But the sole difference in technique with the groups was in the order of presentation of the words; the effect of which was to give large deflections of the 'non-critical' words when they occurred first, and small or insignificant deflections when they were given in the middle or at the end of the series.

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[8] In these and in further comparisons, we have made use of the median deflection for each word rather than the mean; the median more genuinely represents the distribution, and is freer from the influence of aberrant individual responses.

TABLE III

SHOWING THE IMPORTANCE OF THE POSITION OF THE STIMULUS-WORDS IN THE STIMULUS-SERIES

Stimulus-word		Change in position	Deflection difference in favor of earlier position	% of increase, per unit of change in position
Non-critical	table	16 to 1	4.53	41.9
	work	15 to 2	6.82	56.4
	carrot	14 to 3	3.33	9.5
	white	13 to 4	4.44	61.7
	pencil	12 to 5	2.33	22.2
	swim	11 to 6	1.38	18.4
	flower	10 to 7	.75	5.5
	glass	9 to 8	−2.38	−48.7
		Mean	2.65	20.8
Critical	kiss	16 to 1	1.75	1.3
	love	15 to 2	−1.25	− 1.1
	marry	14 to 3	2.25	2.4
	insult	13 to 4	2.00	3.4
	money	12 to 5	− .75	− 1.9
	afraid	11 to 6	1.25	3.9
	dance	10 to 7	1.00	5.3
	woman	9 to 8	.25	.33
		Mean	.81	2.1

Table III should be read: The word 'table' shows a median deflection 4.53 mm. greater when it is presented first in series than when it is presented sixteenth. Taking as a base the median value when it is presented last, this value is increased 41.9% for each unit of change in forward position, or a total of 628% for a change from sixteenth to first.

A question may arise as to the equivalence of groups A and B in the functions measured. The mean of the 16 medians is 6.09 for group A, and 5.18 for group B. From Table II it is obvious that this difference is due to the fact that in group A the 'non-critical' words have priority. A fairer basis of comparison would be with the middle 8 words rather than with the total series; here the means are respectively 4.99 and 5.03, and we may accept this as an indication that the two groups were comparable, for the purposes of this experiment.

CONCLUSIONS

(1) In a serial association experiment, the magnitude of the psychogalvanic response varies with the temporal position of the stimulus. Hence, in clinical studies or in the detection

of guilty knowledge, the emotional values of stimulus words cannot be compared without taking into account the factor of position in series. This of course might be inferred from the general experience with the rapid 'exhaustion' of the psychogalvanic response in repetitions of the same stimulus, but the application of the principle to association experiments has frequently been neglected. Such an error may account, in part, for the inconsistent results occasionally reported in the use of the psychogalvanic response.

(2) 'Critical' and 'non-critical' words cannot readily be distinguished unless a preceding series of at least five buffer words has been used.

(3) The importance of position is greater with 'non-critical' than with 'critical' words; stimuli connected with complexes yield reactions of considerable uniformity, regardless of where they occur.

(4) If different *S*s are to be compared as to emotivity, the schedule of presentation can be varied only within very narrow limits.

(5) When reduced to a comparable scale, the median deflection values for each of the 16 words are closely similar to the results obtained by a previous experimenter. In terms of averages, this indicates a high degree of dependability of the psychogalvanic response.

AN INVESTIGATION OF BLUFFING

By ERNST THELIN and PAUL C. SCOTT, University of Cincinnati

In a report of an experiment conducted in one of his classes at the University of Pennsylvania. Professor S. W. Fernberger has raised the question of the prevalence of bluffing by students.[1] We are told that "a class of 29 showed evidence of more than the usual bluffing in a quiz. A trap was set for them in the next examination in an attempt to determine just how much and how frequently the college student would bluff. To this end, the class was asked to discuss ten terms, devoting not less than one page to each. The seventh term in the list was made up out of whole cloth—'*psychoterminality.*' Of the group of 29 men and women, only 2 made the positive statement that they did not know what the term meant. Six other left the question blank without a definite negative statement. The other 21 students made some sort of an answer ranging from one-half to three pages in length."

At the time of the appearance of the above report, an investigation not greatly dissimilar in character was being conducted at the University of Cincinnati. The general problem of this investigation was to study the existence and extent of bluffing among university students. The investigation did not concern itself at all with any moral question involved and no effort was made either to defend bluffing as honest or to condemn it as dishonest.

For clarity and specificity of discussion, some definition of the term 'bluffing' was felt to be necessary. Hence a working definition might be stated as follows: Pretending to have greater knowledge than is actually possessed.

As a vehicle for the investigation, there was constructed a battery of tests in which there were provided opportunities for bluffing, as defined above. Since English is a universal high school and university requirement and a subject of which all students are expected to have a general knowledge, it was decided to give the test the appearance of being a general English test. As a basis it was decided to use a number of items selected from the Cross[2] and the Wakefield English Tests.[3] Their objective form was observed in the construction of all our tests, which in general took the forms of the New Type Tests. Several new test forms, however, were used, *e.g.* the Vocabulary Tests.

Situations conducive to bluffing were secured by inserting irrelevant and fictitious items into tests containing legitimate items. These irrelevant and fictitious items were arranged in such a way that they would not easily be detected in a survey of the material of the test. An attempt was made to form the fictitious items without any basis of fact. The legitimate items ranged from those of common to those of less general familiarity; the fictitious items, from those apparently easy to those of extreme difficulty. The extent to which the person taking the test expressed familiarity with items of this fictitious character was scored as bluffing.

The following excerpts will give an idea of the test in its final form, after revisions based upon preliminary administrations to about 100 university students and 47 high school students.

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*Accepted for publication September 17, 1927.

[1] S. W. Fernberger, On bluffing in examinations, this JOURNAL, 38, 1927, 155 f. Cf. also H. B. English, *ibid.*, 40, 1928, 350.

[2] E. A. Cross, *Cross English Test for High Schools and Colleges*, 1923.

[3] E. W. Scott, *Wakefield Diagnostic English Test for Secondary School Pupils*, 1926.

ENGLISH TEST

General instructions. This is a general English test. Some of its items are so easy that no one should miss them. Some are so difficult that probably no one will get them all right. Accordingly, every individual's score will have a relative value. Your score will indicate whether you approximate the average of your class, or whether you exceed that average, or fall below it.

If you wish, you may glance through the whole test before you begin work. The test has been timed, so you should experience no difficulty in completing it during the class period. The various sections of the test should be taken in the order in which they appear.

Idiomatic expressions. Direction: Draw a heavy line under the correct form in each sentence.

(1) They were not unmindful (of, to) the French customs.

[There were 10 such sentences, all legitimate.]

Recognizing a sentence. Directions: Some of the following sentences are faulty, and some are correct. Print an (F) before each sentence that is faulty; before each that is correct print a (C).

(1) The reason I was absent was because I was ill.

[Ten such sentences, all legitimate, were presented.]

Shakespeare—A. Directions: Indicate with an (H) or an (M) whether each of the following statements relate to Hamlet or Macbeth.

(1) Malcolm suspected Macduff of treachery.
(2) Mercutio was loyal to the Thane of Cawdor.

[Twenty statements were presented, 12 legitimate, 8 fictitious.]

Shakespeare—B. Directions: Underline the character to whom you ascribe each of the following quotations:

Group I. Quotations from Hamlet.

(1) To be or not to be; that is the question (Claudius, Hamlet, Horatio).

(2) Call me what instrument you will, though you fret me, you cannot play upon me (Ophelia, Laertes, Rosencrantz).

[Five quotations were presented, 2 genuine, 3 fictitious.]

Group II. Quotations from Macbeth.

(1) The night is long that never finds the day (Lady Malcolm, Mercutio, Oliver).

[Five quotations, all fictitious.]

Novels—A. Directions: Underline the proper title or author as found in each parenthesis.

(1) A Tale of Two Cities by (Sir Walter Scott, Ralph Connor, Charles Dickens).

(2) Paths of Victory by (Edith Wharton, Jules Verne, John Erskine).

[Fifteen titles or authors, 12 genuine, 3 fictitious.]

Novels—B. Directions: Within the parentheses before each title, write the number of the author of that work, as given in the second column.

Group I.

[Five titles and five authors, all genuine.]

Group II.

[Five titles, two genuine; and five authors, two genuine.]

Group III.

[Five titles and five authors, none genuine.]

Literature. Directions: Indicate, by underscoring, the appropriate item or items in each of the following.

(1) To whom is literature most indebted for the presentation of the immortal figure of Faust? (Marlowe, Schiller, Goethe).

(2) From which of the following ancient writers is Milton supposed to have taken many of his classic figures in his L'Allegro and Il Penseroso? (Archides, Plymander, Harmonides).

[Ten questions, 5 genuinely related to the answers suggested, 5 with fictitious answers suggested.]

Vocabulary test—A. Directions: A student employs more than 4000 words of the English language adequately to express his thoughts. What is the extent of your vocabulary? Four thousand words, 6000, or even more? In the following tests, words have been selected from groups of graduated word lists on the basis of which we shall be able with some accuracy to determine the breadth of your vocabulary.

[Fifteen synonyms, antonyms, or words standing in some other relation to a given word, were to be underlined among those printed in parentheses. In six cases the words were coined.]

Vocabulary test—B.

[In 10 sentences, the person tested had to underline among words in parentheses a certain type of word, *e.g.* archaic, concrete, hackneyed. In 5 of the sentences one or more of the suggested words were coined.]

Vocabulary test—C.

[Paragraphs were presented with certain words underlined, some genuine, some coined, and the person tested was asked to underline for each of these words its synonym among three words in parentheses. Some of the words in parentheses were also coined.]

The items of the test were by their nature such as to lend themselves to the following system of scoring: (1) all legitimate items were subject either to correct or incorrect checking; (2) every fictitious item marked or accepted for definition was counted as a bluff; (3) in any particular situation where there was more than one possible correct answer, or opportunity for multiple bluffing, each correct answer and each bluff counted as one. The total number of legitimate items was 120, the irrelevant and fictitious items totaled 113.

In order to secure a comparison of the results of the test with the intelligence rating of the students, they were given the Otis Self-Administering Tests of Mental Ability (Higher Examination: Form A, For High Schools and Colleges). A further comparison was made of the test score of each student with his Average Grade, representing the average of all grades received while in the university. Since grades were found expressed in qualitative forms, in order to facilitate statistical treatment they were given quantitative values as follows: A = 1; B = 2; C = 3; D = 4; E = 5.

The test was given, in various classes of the College of Liberal Arts, to 291 students classified as follows: 19 freshmen, 138 sophomores, 75 juniors, 47 seniors, 12 specials. To get a basis of comparison between the amount of bluffing done by university students and by those engaged in various vocations outside of student life, the test was given also to groups outside of the University. These groups totaled 58, and included bookkeepers, toolmakers, chauffeurs, pressmen, salesmen, superintendents, and managers of a laundry company.

RESULTS

Universality of bluffing. The investigation seems definitely to have established the existence of bluffing among university students, at least among those groups to which the test was given. Some bluffing was done by every student; the amount of bluffing varying from just below 5% to more than 81%. That the amount of bluffing in general was quite large is indicated by Table I. Half of the students bluffed 46.58% or more; a fourth of them bluffed 60.97% or more, and only a fourth of them bluffed 30.55% or less. The average bluffing score of all students was 44.6%.

The same condition, however, was not found to exist in the groups to whom the test was given outside of the University. The amount of bluffing by these groups ranged from 0 to 69.1%, the mean of the entire group being 25.7%; the median 19.16%; and the Upper and Lower Quartiles 38.44% and 6.78% respectively. Of the 58 people in this group, 4—a bookkeeper, a salesman, and managers of two local laundries—or 7%, did no bluffing at all, *i.e.* they did not express familiarity with any of the 113 fictitious items. The extreme cautiousness of these 4 is further attested by the narrow range of the legitimate items which they attempted. This narrower range of items attempted by all the members of the groups outside the University, which may have been due to unfamiliarity with the nature of the material which constituted the body of the test, resulted in lower *C* (Correct) scores. They attempted less, hence their *C* scores and *B* scores are both smaller. But even a comparison of the average *C* and *B* scores of the two groups (Table II) shows that the University group did proportionately slightly more bluffing than did the group outside of the University, the proportions being 0.873 and 0.829.

TABLE I

DISTRIBUTION SHOWING PERCENT OF BLUFFING

Percent of Bluffing	Students		Outside Group	
	No.	%	No.	%
80–84	1	.34	0	—
75–79	0	—	0	—
70–74	2	.68	0	—
65–69	23	7.96	1	1.72
60–64	58	19.93	1	1.72
55–59	19	6.53	4	6.89
50–54	22	7.56	2	3.44
45–49	30	10.30	3	5.17
40–44	15	5.15	1	1.72
35–39	26	8.93	8	13.79
30–34	25	8.59	2	3.44
25–29	19	6.52	1	1.72
20–24	30	10.30	5	8.62
15–19	11	3.78	6	10.37
10–14	3	1.03	5	8.66
5–9	6	2.06	7	12.06
0–4	1	.34	12	20.68
Totals	291	100.00	58	100.00
Mean	44.6		25.7	
Median	46.58		19.16	
Upper Quartile	60.97		38.44	
Lower Quartile	30.55		6.78	

Age and bluffing. To determine whether or not there existed any relationship between age and bluffing, the data were given the treatment shown in Table II, which gives the average age by classes of the various groups with number and percent of legitimate items answered correctly; number and percent of fictitious items attempted; intelligence test scores; and average grades. The freshmen are shown to have bluffed most (50.9%), the seniors least (39.4%), with both sophomore (40.3%) and junior (45.4%) groups bluffing less than the freshmen and more than the seniors. That this relationship between class and amount of bluffing was not consistent, however, is shown by the fact that the sophomores bluffed slightly less than the juniors. Moreover, the average age of the sophomores was slightly less than that of the freshmen. The average of the special students (46%) was larger than that of the sophomores, juniors, or seniors but less than that of the freshmen. The coefficient of correlation between age and amount of bluffing was found to be −0.1697 ± 0.38, which would suggest a slight tendency toward decreased bluffing with increasing maturity.

Sex and bluffing. Our data further lent themselves to a treatment designed to show the relative amount of bluffing by men and women. This treatment of the data is given in Table III.

TABLE II

AVERAGES BY CLASSES AND GROUPS

Group	Age	No. C	No. B	% C	% B	I.T. Sc.	Av. Grade
Freshmen	19.6	67.5	57.5	56.1	50.9	58.6	3.18
Sophomores	19.4	66.7	45.6	55.4	40.3	58.2	2.85
Juniors	20.4	65.7	51.7	54.6	45.4	58.6	2.70
Seniors	21.5	64.9	44.9	54.0	39.4	57.8	2.60
Specials	28.7	68.4	52.2	56.8	46.0	57.9	2.23
Whole University Group	20.4	65.4	50.4	54.5	44.6	58.1	2.78
Non-University Group	26.3	36.9	32.8	31.0	25.7		

An examination of Table III shows that the men in the Freshmen class and those classified as Specials bluffed considerably more than did the women of those groups, the ratio being 63.1 to 38.6% for freshmen and 59.2 to 33.2% for the special students. The women of the sophomore, junior and senior groups, however, bluffed more than did the men of those groups, the ratios being for the sophomores 46 to 34.7%, for the juniors 46 to 44.8%, and the seniors 39.9 to 39.2%. For the junior and senior groups the differences are small, and are probably not statistically significant. The average bluffing score of all the men (45.8%) was slightly more than that of the women (43.4%).

TABLE III

AVERAGES OF MEN AND WOMEN BY CLASSES

Classes	Sex	Age	No. C	No. B	% C	% B	I.T. Sc.	Av. Gr.
Freshmen	M	20.2	68.8	71.2	57.3	63.1	63.6	3.34
	F	19.1	66.2	43.8	55.2	38.6	53.7	3.03
Sophomores	M	19.5	68.6	39.1	57.2	34.7	58.5	2.82
	F	19.4	64.8	52.2	53.8	46.0	58.0	2.89
Juniors	M	20.4	66.5	50.9	55.3	44.8	59.4	2.72
	F	20.5	65.0	52.6	54.1	46.0	57.9	2.69
Seniors	M	21.7	67.4	44.8	56.2	39.2	58.1	2.84
	F	21.4	62.5	45.1	52.1	39.9	57.6	2.37
Specials	M	29.0	71.0	67.0	59.1	59.2	65.0	2.25
	F	28.4	65.9	37.5	54.8	33.2	50.8	2.22
Totals	M	20.3	66.3	51.8	57.0	45.8	59.0	2.82
	F	20.5	64.6	49.0	54.0	43.4	57.2	2.74

Relation between bluffing and grades and between bluffing and intelligence test scores. Two questions attained commanding interest as the investigation progressed and it became apparent that students were giving varying degrees of expression of familiarity with fictitious items. First, is there any relation between the amount of bluffing and grades earned at the University, and vice versa? Secondly, is there any relation between the amount of bluffing and intelligence, *i.e.* do the students who bluff most, score highest in intelligence tests? In order to answer the first, the data were tabulated so as to show the amount of bluffing on the basis of grades; to answer the second they are tabulated on the basis of scores received in the Otis Intelligence Test.

The first treatment (Table IV) shows that students who got highest grades (2.99 or above) did least bluffing; answered correctly a slightly higher percentage of the legitimate items; and scored highest in the Intelligence test. The correlation between amount of bluffing and grades, −0.09 ± .039, however, was so small as to be statistically insignificant.

TABLE IV

DISTRIBUTION OF PERCENT CORRECT, PERCENT BLUFF, AND INTELLIGENCE TEST SCORE ON A BASIS OF AVERAGE GRADES

Average Grades	No.	% *C*	% *B*	I.T.Sc
1 to 1.99	28	57.5	43.5	63.4
2 to 2.99	106	54.3	42.0	58.0
3 to 3.99	86	52.9	47.6	56.6
4 to 4.99	12	54.4	47.6	52.0

TABLE V

DISTRIBUTION OF PERCENT CORRECT, PERCENT BLUFF, AND AVERAGE GRADES, ON A BASIS OF INTELLIGENCE TEST SCORE

Intelligence Test Score	No.	% *C*	% *B*	Av. Gr.
70 or above	16	59.1	40.6	2.56
60–69	93	57.2	47.2	2.45
50–59	89	53.1	44.6	2.81
40–49	28	49.6	38.0	2.97
30–39	5	52.2	45.8	3.69
20–29	1	51.6	40.7	2.42

The second treatment (Table V) showed that those who scored high in the intelligence test (60 or above) answered the highest percentage of the legitimate items correctly; received higher grades; and averaged slightly more in bluffing. The 16, however, who scored highest in the intelligence test (70 or above) averaged less in bluffing than did those scoring less (with the exception of those scoring between 40 and 50); while those who scored between 60 and 70 in the intelligence test averaged most in bluffing. As in the case of the relationship between bluffing and grades, however, the correlation between bluffing and intelligence test score (0.0756 ± .043) was so small as to be of no statistical significance.

Thus it would appear, on the face of the results, that it is not the student who bluffs who gets highest grades, and that those students who bluff most are not those of highest intelligence, but those just above the average in intelligence.

SUMMARY

The findings of the investigation may be summarized as follows:

(1) Among the university groups to which the test was given, bluffing was found to be universal. That the extent of this bluffing was impressive

is indicated by the fact that 50% of the students expressed familiarity with 46% or more of the 113 fictitious items. The percentage frequency with which fictitious items were checked as being familiar ranged from 5% to more than 81%.

(2) Among the groups outside of the university, bluffing was found to be much less prevalent only about 17% expressing familiarity with 46% or more of the fictitious items. The median percentage score of bluffing for these groups was slightly above 19%, the scores ranging from 0 to 69.1%. Seven percent of those comprising these groups checked none of the fictitious items as being familiar to them.

(3) In proportion to the number of legitimate items checked correctly, the amount of bluffing by university students was but slightly larger than that by the non-university groups. In other words, the latter groups not only refrained from expressing familiarity with fictitious items, but practiced the same inhibition with reference to legitimate items with which they had no familiarity.

(4) The data show a negative correlation between amount of bluffing and age. Thus maturity, and all it implies, appears to function to decrease the type of activity under investigation.

(5) Although the relative amount of bluffing was not constant for the various classes of the university, on the average the men were found to bluff more extensively than the women. This condition was most marked in the case of the freshmen, the percentage frequencies of bluffing for that group being, men 63.1, women 38.6.

(6) The students who ranked highest academically averaged lowest scores in bluffing in the test. In other words, the investigation appears to indicate that the highest grades, at least, are not secured by bluffers.

(7) Students who bluffed most were those scoring just above the average in the intelligence test, and not those scoring highest.

SEASHORE'S MEASURES OF MUSICAL ABILITY APPLIED TO CHILDREN OF THE PRE-SCHOOL AGE

By ESTHER MCGINNIS, University of Minnesota.

The purpose of this investigation[1] was to determine whether it is possible to use Seashore's measures of musical ability[2] with children of the pre-school level; to determine what methods could be devised for so doing, and what reliability and validity the results of such an adaptation would have.

It would be of interest and value to parents and educators if it were possible to detect differences in musical ability in young children. Such information would help in planning for the musical education and training of the child and would also throw light on the question of the innateness of musical ability. Seashore in his *Psychology of Musical Talent* says the ability to distinguish pitch differences, and the ability to distinguish intensity differences are innate and not modifiable.[3] No experimental verification of this statement, however, could be found in the literature on children below school age; neither is there any available data on the effect of training given to very young children who appear to lack musical ability.

Individual differences in children's reactions to music are observable very early. Babies move their bodies rhythmically in time to strongly accented music; older children 'do what the music says' by stamping to loud music, tiptoeing to soft; some children 'carry a tune' and show a keen delight and pleasure in hearing music. Others do none of these things. Why? Is it because of physical differences in ability to discriminate rhythm, intensity, pitch, etc., and are these abilities innately determined? In a nursery school situation, should the child who fails to respond to the music hour be excused because of inability, or may the lack of response be due to social conditioning while the innate ability is present but needs help in order to be expressed? To determine the presence or absence of musical ability becomes an important question in the treatment of the young child at home and in the nursery school.

As a first approach to the study of this problem, three of the six Seashore tests were selected for trial: intensity, pitch and consonance. The others are probably equally important, but those named are less complicated and involve distinction between two sounds only. Each record was given individually twice to each of 16 children, 8 boys and 8 girls, enrolled in the nursery school at the Institute of Child Welfare, University of Minnesota. The developmental status of the group is indicated in Table I.

The first difficulty to be met is the length of the task. Each side of a record includes 50 pairs of notes requiring 50 judgments. Few young children are able to give sustained attention for so long a period. By marking the record immediately after the last note of the first 30 judgments with a slight scratch of a pin point, each side of the record may be given in two parts. There is enough space between the end of the thirtieth and the

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[1] From the Institute of Child Welfare. Made under the direction and with the assistance of Dr. Florence L. Goodenough.

[2] C. E. Seashore, *The Psychology of Musical Talent*, 1919; *Manual of Instructions and Interpretations for Measures of Musical Talent*, 1919; *A Survey of Musical Talent in the Public Schools*, 1920.

[3] *Op. cit.*, 59 ff., 99 ff.

beginning of the thirty-first judgment, so that the needle can be placed on the scratch, the victrola started, and the drag in speed will be taken up by the time the first of the next pair of notes is reached.

The next difficulty is the inability of the child to understand Seashore's terms "weak and strong," "high and low," "better and worse." The words "loud and soft" (used in music hour in the nursery school) "baby and daddy" referring to the story of the Baby bear (examiner's voice very high) and the Daddy bear (examiner's voice low and deep), and "pretty and ugly" were substituted. Practice was given with a mouth organ and with the victrola until the child seemed to comprehend clearly what was wanted.

TABLE I

RANGE AND AVERAGE IN MONTHS OF THE GROUP'S CHRONOLOGICAL AND MENTAL AGES, TOGETHER WITH THE RANGE AND AVERAGE OF THEIR INTELLIGENCE QUOTIENTS

Measure	Chronological age	Mental age	Intelligence quotient
Range	41–59	42–79	97–148
Average	49	59	122

The third difficulty is the uninteresting nature of the material of the tests to the young child. The sounds are monotonous and unmusical and the children were bored and disappointed, as music to them means melody and action. To overcome this very important difficulty several means of motivation were used.

The tests were called games. Intensity was the loud and soft game, and it was compared to a similar game used in the nursery school. Pitch was the baby bear and daddy bear's voices, and the game was to tell who spoke the last note, the baby bear or the daddy? Consonance was the pretty and ugly game. The mouth organ, used at first only by the experimenter for illustration, later (properly sterilized each time) was given to the child, and at the beginning and end of the experiment he was allowed to play it. A game was made of this, the examiner guessing which notes the child was playing. The record blank was filled out directly in front of the child so he could see the spaces fill up. The children were interested in the amount done, wanted to see how much other children had finished, and pointed with pride to what they had accomplished. At times, when the children wanted to stop or had a tendency to answer before they heard the second of a pair of notes, the examiner beat the notes in the air with her hand, making no noise but holding the child's attention.

TABLE II

AVERAGES OF OMITTED RESPONSES

Trial	Test		
	Intensity	Pitch	Consonance
1	6.8	6.9	0.25
2	4.9	5.7	0.37

The fourth difficulty is omitted responses. The responses were scored on a percentage basis by taking the percent right of the answers, not of the total. If too many omitted responses occurred, that section of the record was repeated. The number of omitted responses decreased on the average from the first to second trial in intensity and pitch. See Table II.

The small number of omitted responses in the consonance records is due to the longer time between judgments. It would greatly improve the other records for use with young children if the time between judgments were increased. The correlation of omitted responses on both trials with C.A. is −0.494, showing a tendency for the older children to make fewer omissions.[4] The correlation of omitted responses with mental age is −0.54. Correlation of omitted responses in pitch with those in intensity is only +0.148 which shows that individual children were erratic in their omitted responses and varied from test to test.

TABLE III

RANGE OF THE PERCENT CORRECT JUDGMENTS IN THE BEST TRIAL, CORRELATION OF PERFORMANCE WITH C.A. AND WITH M.A., AND NUMBER OF CHILDREN GIVING RECORDS COMPARABLE TO THE NORMS OBTAINED BY SEASHORE FOR 5TH GRADE CHILDREN

Test	Range percent correct in best trial	Correlation of performance with		No. children to be compared with Seashore's 5th grade norms
		C.A.	M.A.	
Intensity	51.2–65.3	+0.14	+0.20	6 above 55% right
Pitch	50.5–62.2	−0.08	−0.30	9 above 55% right
Consonance	39.6–68.0	−0.10	−0.26	9 above 58% right
Combined score			−0.18	

In each test a number of children made a ranking sufficiently high to be compared to Seashore's norms for 5th grade children. The low correlations with C.A. and M.A., given in Table III, show that the ranks made on these tests do not depend on chronological or mental age.

The correlation between consonance and pitch is the highest, that for intensity and pitch second, and that for consonance and intensity lowest in both sets of data.

TABLE IV

CORRELATIONS BETWEEN EVERY TWO OF THE THREE TESTS (Using best trial)

Test	Our results	McCarthy's results*
Intensity and Pitch	+0.492	+0.09 to +0.38
Consonance and Pitch	+0.623	+0.21 to +0.48
Consonance and Intensity	+0.26	+0.10 to +0.16

*From data about to be published by Dorothea McCarthy, University of Minnesota, on the reliability and validity of the Seashore Music Tests. The tests were given to groups of school children, junior high and college students. Validity was studied by comparison with ratings and school marks in music.

The results for each test were analyzed by columns to see whether the percent right decreased in order of the difficulty of the judgments. While no such tendency could be observed, the fact may have been obscured by the erratic attention of the children. Frequently a child did as well or better on the most difficult column as on the easiest.

The reliability for the intensity record compares favorably with the results of McCarthy and others. That for pitch is very low, which is probably due to the erratic responses of the children rather than to an inability to grasp the meaning of the test, as it compared favorably with the other tests in validity. Consonance is also fairly reliable.

Three of the nursery school teachers independently rated each of the children on general musical ability.[5]

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[4] All correlations are Spearman's rank order unless otherwise stated.

[5] The coöperation of Miss Marion Mattson, Miss Catherine Thompson and Miss Adelia Boynton, of the Child Welfare Institute, University of Minnesota, is gratefully acknowledged.

A composite of the three ratings was made by averaging the three ranks for each child. The correlation of the composite rank with C.A. is +0.176 and with M.A. +0.147, showing practically no relationship. Agreement on individual children was high. On the composite rank the first five were in the first half of the group, and the lowest four were among the last six on every rating.

The results obtained by McCarthy are given for comparison in Table VIII.

TABLE V

RELIABILITY COEFFICIENTS BASED UPON THE CORRELATION* BETWEEN FIRST AND SECOND TRIALS OF EACH TEST

Coefficient	Test		
	Intensity	Pitch	Consonance
Our results	+0.43 P.E. = .14	−0.02 P.E. = .17	+0.37 P.E. = .15
McCarthy	+0.68	+0.70	

*Pearsonian correlation.

TABLE VI

CORRELATION BETWEEN RATINGS

Rating	Correlation	Multiple Correlation	
1 and 2	+0.66	1.23	+0.72
1 and 3	+0.55	2.13	+0.66
2 and 3	+0.43	3.21	+0.55

TABLE VII

CORRELATIONS BETWEEN SCORES IN THE THREE TESTS AND COMPOSITE AND INDIVIDUAL RATINGS

Rating	Test			Composite score in the three tests
	Intensity	Pitch	Consonance	
1	+0.04	+0.20	+0.39	+0.40
2	−0.13	+0.21	+0.21	+0.13
3	+0.13	+0.25	+0.41	+0.40
Composite	+0.04	+0.17	+0.42	+0.37

TABLE VIII

MCCARTHY'S CORRELATIONS BETWEEN SELF ESTIMATES, MUSICAL TRAINING, OR MUSIC GRADES WITH SCORES ON TESTS IN TERMS OF PERCENTILE RANK

Ss	Test		
	Intensity	Pitch	Consonance
58 adults, 2 groups	+0.40 +0.54	+0.54 +0.68	not given
35 adults	+0.16	+0.47	+0.34
71 5th and 6th grade children	+0.46	+0.50	+0.52
81 Junior high school students	+0.85	+0.32	+0.29

In the case of the consonance test the coefficients obtained compare favorably with the findings for older children and adults. The correlation between the composite scores on the three tests and the composite of the three ratings (+0.37) is quite as high as was ordinarily obtained between teacher's judgments of intelligence and scores earned on the earlier revisions of the Binet Scale. With suitable modifications, particularly in the direction of shortening the records and increasing the intervals between judgments, it appears probable that these tests might become valuable research instruments for use with young children.

THE FUSION OF NON-MUSICAL INTERVALS

By E. R. GUTHRIE and H. MORRILL, University of Washington

In a recent number of this JOURNAL Brues published the results of an investigation of the fusion of non-musical intervals.[1] A recent experiment in the University of Washington Laboratory resulted in a striking confirmation of his results by the use of a radically different method.

Brues used Stern variators to sound two notes. One of the notes was of constant pitch and the second note was varied so as to include twenty-four quarter-tone intervals from the first tone to its octave. Four *O*s were asked to record the degree of fusion of the combination on a scale from 1 to 5 (5 representing perfect fusion). For most of the tone combinations 10 judgments were obtained from each *O*. For a portion of the octave 6 additional judgments were made. The average degrees of fusion reported are represented on the accompanying Fig. 1 by the small circles. Brues' averages have been reduced to S.D. units and on the graph are plotted from the mean of the Washington data with standard deviations equated.

In the Washington experiment Stern variators were also used. Forty-four intervals were explored, extending from unison (395 with 395 v.s.) to an interval beyond the fifth, 395 v.s. with 617 v.s. at intervals of approximately 5 vibrations per sec. Calibration of the Stern whistles by comparison with accurate tuning forks and reeds showed a virtually linear error in the readings of the variators. The pitches as indicated on the variators extended from 400–615 v.s. at intervals of 5 v.s. Under the conditions of the experiment, which included a necessity for rapid setting of the instruments, there was an average deviation of 0.84 v.s. from the true pitch as determined by tuning forks.

*O*s in groups of about 50 were asked to record each combination as 'pleasant' or as 'unpleasant.' The tone combinations were given in random order. When this had been done, the same *O*s were asked to judge, on a repetition of the series, whether or not each combination had 'consonance' or 'dissonance.' With 5 groups the 'pleasant-unpleasant' discrimination was obtained first. With 3 groups the 'consonance-dissonance' discrimination was made first.

Consonance had not been discussed in the elementary psychology class of which the *O*s were members. It was described to them as a combination of purity, smoothness, and good-blending—a description which may be revolting to many psychologists, but which proved satisfactory to naïve *O*s.

Reports on the discrimination of 'consonance' and 'dissonance' were obtained from 381 *O*s, and on 'pleasantness' and 'unpleasantness' from 372 *O*s. The percentage of the 381 *O*s that reported any tone combination as having 'consonance' is indicated by the solid line in Fig. 6. The percentage of the 372 *O*s that reported each tone combination as 'pleasant' is indicated by the dotted line.

It will be noticed that there is no tendency for the high points of 'consonance' or of 'pleasantness' to coincide with the intervals of simple ratio, or with the 'musical' intervals of the equally tempered scale whose positions are indicated by the second, fourth, sixth, eighth, etc., of the circles indicating Brues' data. The degrees of fusion indicated in the Washington experiment and in the experiment of Brues are in substantial agreement when differences in method are taken into account. In addition to using

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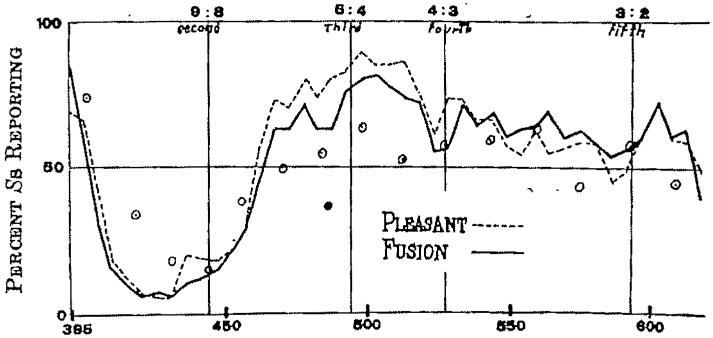
*Accepted for publication February 14, 1928.

[1] A. M. Brues, The fusion of non-musical intervals, this JOURNAL, 38, 1927, 624-638.

a different method of recording judgments, Brues worked with tones of another pitch range, from 256 v.s to 512 v.s.

We do not suggest that 'unpleasantness' and 'consonance' coincide to the extent indicated in the curves. The results indicate only that the term 'consonance' as described, and the term 'pleasantness' led to very similar responses from the *Os*. The two curves have, indeed, a difference which is far beyond the range of chance, the probability that the 'pleasantness' curve is a chance variant of the 'consonance' curve being 4.2×10^{-8}.

The curves obtained have a slight resemblance to the theoretical curves of consonance worked out by Helmholtz for tones including several partials. They have no resemblance to the curve indicated by Helmholtz for combinations of pure tones, although Dr. S. H. Anderson of the local physics department reported that the variators showed only very faint traces of the third partial, others being inaudible.



VIBRATIONS PER SECOND—PITCH OF UPPER TONE

FIG. 1

Pitch combination	% reporting consonance	% reporting pleasantness	Pitch combination	% reporting consonance	% reporting pleasantness
395–395	86	70	395–508	77	85
395–400	58	66	395–513	74	86
395–405	32	42	395–518	72	75
395–410	16	18	395–523	55	61
395–415	10	12	395–528	58	73
395–421	6	7	395–533	71	73
395–426	7	6	395–538	64	66
395–431	6	7	395–544	68	65
395–436	10	20	395–549	60	57
395–441	12	19	395–554	63	54
395–447	15	19	395–559	64	62
395–452	21	22	395–564	59	54
395–457	29	36	395–569	60	56
395–462	45	54	395–575	62	58
395–467	63	78	395–580	58	58
395–472	63	76	395–586	54	44
395–478	71	80	395–591	65	48
395–482	63	74	395–596	69	58
395–487	63	80	395–602	72	70
395–492	76	83	395–607	59	59
395–498	80	89	395–612	62	58
395–503	81	85	395–617	38	48

TWO COMPARISON STIMULI WITH ONE INITIAL STANDARD IN LIFTED-WEIGHTS

By WILBUR S. HULIN, Princeton University, and L. R. FRAZIER, Harvard University

Lifted-weight experiments have been "the classical field for absolute impression."[1] Absolute impression is based on the stimulus error.[2] Recently Fernberger and Rudisell have shown that the stimulus attitude can yield psychometric functions of high precision,[3] a conclusion which suggests the possibility that the persistent factor of absolute impression might also conform to the psychometric function.

TABLE I

SHOWING THE MEASURES OF PRECISION (*hs*), THE LIMENS, THE INTERVALS OF UNCERTAINTY, AND THE VALUES OF SUBJECTIVE EQUALITY FOR THE NORMAL AND TEST SERIES

O	Series		*h* 'greater'	*h* 'less'	Limen 'greater'	Limen 'less'	Interval of uncertainty	S.J.E.*
	Normal series		.0728	.0770	100.3436	91.6700	8.6736	96.0068
H	Test series	1st comp	.0913	.1007	97.0880	91.8316	5.2564	94.4595
		2nd comp.	.0913	.0879	99.3834	90.0124	9.3760	94.7004
	Normal series		.0894	.0829	100.9780	89.4996	11.2984	95.3609
N	Test series	1st comp.	.1612	.0757	97.7884	89.0312	8.7572	95.7070
		2nd comp.	.0574	.0757	99.2768	91.3976	7.8792	94.7903
	Normal series		.0949	.0769	98.4892	87.7884	10.7048	93.6973
S	Test series	1st. comp.	.0561	.0316	108.9132	69.4708	39.4424	94.8412
		2nd comp.	.0414	.0824	112.0024	81.8552	30.1472	91.1300

*The 'subjective judgment of equality' is represented by *Xi*, the intersection of the limens.

This possibility was tested with lifted weights by obtaining judgments on two successive comparison stimuli following a single initial standard.[4] The method of constant stimuli was used with three *Os* (H, N, and S).[5] The standard weight was 100 grm. H judged 5 comparison weights of 88, 92, 96, 100, and 104 grm. N and S judged 7 comparison weights, adding 84 and 108 grm. Each *O* first judged a normal series of standard and single comparison. In the test series the standard was presented and followed by a comparison weight upon which the judgment 'greater,' 'equal,' or 'less' was immediately reported, and then directly after this judgment the second comparison weight was presented and immediately judged, also

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*Accepted for publication January 17, 1928.

[1] E. B. Titchener, *Experimental Psychology, Instructor's Manual, Quantitative*, 1905, 231. See references to Martin and Müller, Kinnaman, Fröbes, *ibid.*, 205 ff., 304 ff.

[2] *Ibid.*, lxiii-lxvii, 203 f.

[3] S. W. Fernberger, An experimental study of the 'stimulus-error,' *J. Exper. Psychol.* 4, 1921, 76; E. S. Rudisell, Constancy of attitude in weight perception, this JOURNAL, 36, 1925, 587.

[4] Method suggested by Professor C. C. Pratt, Harvard University, in 1923.

[5] Apparatus and procedure similar to Fernberger's *op. cit.*, 66.

in terms of the initial standard weight. In the normal series each *O* gave 200 judgments apiece on each comparison weight; in the test series H gave 200 judgments on each weight, and N and S each gave 100 judgments apiece on each weight. The results were computed by Urban's tables. 'Equal' judgments were discarded. The total of 6200 judgments is summarized in Table I.

Table I shows a significant similarity between the psychometric functions of the test series and normal series for H and N. S gave many 'equal' judgments and his results are fairly negative. In the test series, all *h*s for H and one *h* for N are higher than in the normal series; one 'interval of uncertainty' for H and both for N are smaller than in the test series; and one of the values of 'subjective equality' for N and both for H are farther from the value of the objective standard than in the normal series.

APPARATUS

INSTRUMENT FOR OBSERVING OCULAR MOVEMENTS

By S. M. NEWHALL, Yale University

The instrument was devised to facilitate direct observation of eye-movements for certain problems where photographic or other elaborate technique would be either unwarranted or inexpedient.[1] It improves conditions of observation principally by presenting *E* with an enlarged illuminated image of *S*'s eye and by controlling in some measure *S*'s head movements and visual stimuli.[2] In its original use in counting the saccadic movements of children, there were also the requirements of simple adjustment, freedom from restraint or intimidation, and ready portability. The manner of meeting the requirements seems evident in the description below.

In Plate I the device is shown clamped to a laboratory standard which in turn is clamped to a table. *S* looks at the stimulus material exposed in the rack, R. At the same time he holds the portion of his upper lip just below his nose against the middle of the rod, S. This steadies his head but does not interfere with verbal reports. The open space adjacent to the rod is to allow free breathing.

E, at E, looks through an observation tube, T, which he trains upon *S*'s right eye-ball from the side without intercepting *S*'s line of vision. Training is achieved with a slightly braked universal joint upon which the tube carrier, C, is mounted. The 20-diopter convex lens set in the end of the tube can be seen in the figure. Focussing is accomplished by sliding the tube back and forth in the carrier. When the distance between eye and lens is somewhat less than its focal length, a virtual erect image of the eye which fills the field can be obtained. With practice *E* can quickly train and focus with one hand. In doing this, the writer has found it convenient to place forefinger and thumb on the end of the tube near his eye at E with the other fingers resting on the tube carrier. Forefinger and thumb then serve in focussing and all the fingers in training, without shift of position. There is a hand-wheel, visible below C, which can be used for horizontal training; but it is really unnecessary and, indeed, cannot be used for the combined one-hand adjustment. The knurled head on the right side of the tube carrier simply allows tightening to prevent the tube sliding out during transport.

Local illumination for both *S* and *E* is secured from a 60-watt full-frosted Mazda lamp, L. It is mounted as shown so that emitted light clears the top of the observation tube to the stimulus material and also passes the end of the tube to *S*'s eye. The source is too far to the side to be seriously distracting. Furthermore, if the light is switched on and off only while *S* is in position, the danger of dazzling him is eliminated. *E* is himself protected by the square screen represented behind the lamp.

Since some of the space relations required study, actual dimensions may be useful and these are here given in terms of cm. Baseboard A (40 x 13 x 1.2) is rigidly screwed to baseboard B (65 x 8 x 2) at an angle of 55°. To

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[1] The instrument is due to Professor R. Dodge in principle.

[2] W. Miles has recently reviewed the earlier simple lens and mirror methods, including ontributions by Erdmann and Dodge, Öhrwall, Javal, Landolt, and Freeman. He also has described an ingenious peep-hole technique. Cf. *J. General Psychol.*, 1, 1928, 373 f.

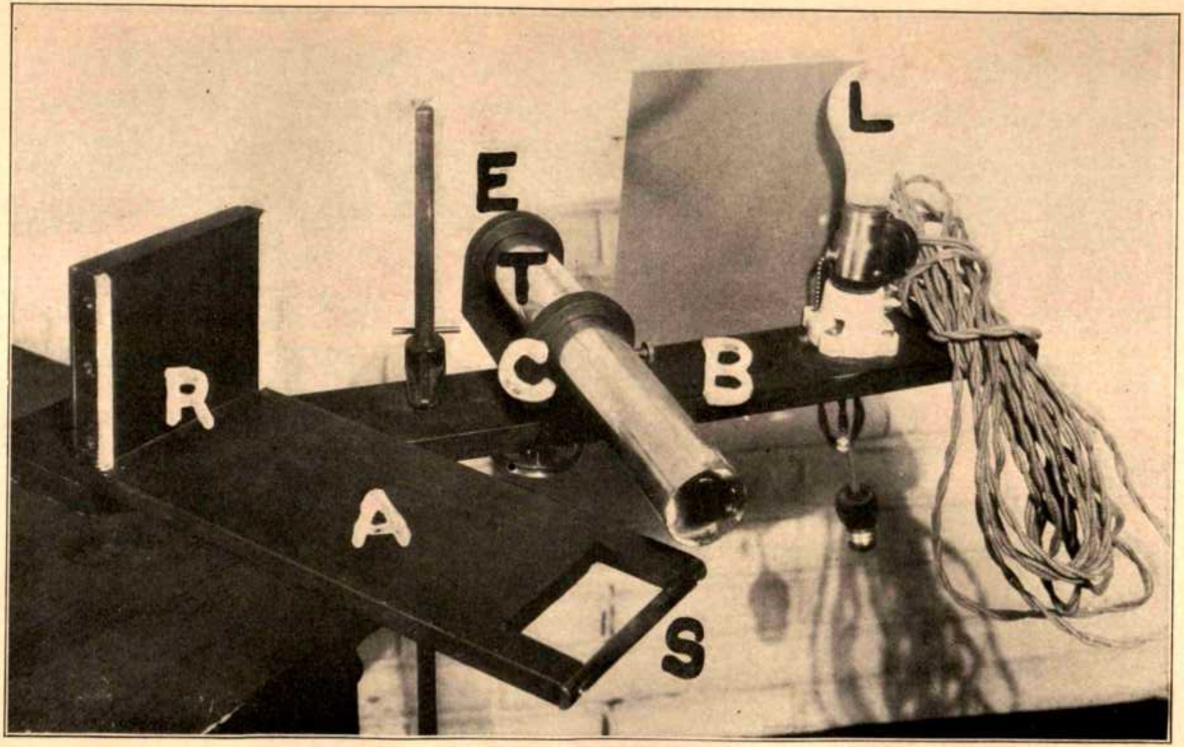

Plate I. Instrument for Observing Ocular Movements

A are screwed two parts, *i.e.* the half-round brass rod jaw or head-rest, S(1.2 diam.) and the stimulus rack, R (13 x 13 x 2), the edges of which are painted dead white to avoid contrast with the exposure card. The distance SR is 30 cm. Mounted on B are the lamp, L, and the tube carrier, C (base, 10 x 7 x 1.2; ends, 7 x 7 x 1.2). The angle formed by the lines LS and SR is 80°. The universal mountings of the tube carrier is a hinge attached to a vertical shaft which passes through a hole in baseboard B. The hinge permits up-down and the shaft left-right movement. Sliding easily through holes in the ends of the carrier is the thin-walled, internally blackened, brass tube, T (30 x 4.5 diam.). The vertical distance between the top of baseboard B and the axis of the tube is 6 cms. The mountings of the tube carrier is so located on B that the tube will point at about right angles to B when trained on *S*'s eye. Under these circumstances the end of the tube somewhat overlaps the right side of the head-rest as shown in Plate I. If binocular exposure is desired, it is obviously important that in training the tube it must not be allowed to overlap so much as to interrupt the line of vision of the right eye.

Several further points relating to design or use seem worthy of mention. One problem was to make the observation tube short enough to provide *E* with as large a retinal image as feasible but long enough to avoid distraction of *S* by over-proximity of *E*. Thirty cm. seemed a reasonably satisfactory dimension. The fact that the dull black interior of the tube restricts the field to *S*'s eye favors concentration in observing that object. The same condition facilitates a simple check of head movements. When the image of *S*'s eye and lid as a whole moves, in *E*'s restricted field, the movement is readily noticed and it must be due to head movement.

Experience indicates that a special technique is desirable in counting reading movements. If *E* attempts to follow with his own eye *S*'s movements, observation becomes difficult or impossible. The writer obtained best results by fixating a relatively stationary point as near as possible to the exposed area of the eye-ball (edge of lid) while attending to a fixed point on the moving ball itself (vein or outer edge of iris). If the reading material is difficult regressions are likely to occur and sometimes they cannot be distinguished from the long swing of the eye to the left in starting the next line. *E* may have an assistant who, with *S* reading aloud, follows the text and taps lightly with a pencil at the end of each line. Thus *E* is made confident of the ending of each line and of the time to record the number of movements observed during the reading of that line. The assistant could, if desired, take the total reading time with a stop-watch. He could also alternate functions with the *E* as a check on personal equation.

BOOK REVIEWS

Edited by Joseph Peterson, Peabody College

The Theory of Imagination in Classical and Mediaeval Thought. By M. W. Bundy. University of Illinois Studies, Vol. 12, Nos. 2-3. Urbana, University of Illinois Press, 1927. Pp. 289.

Attacked from many angles and on many grounds, the terms 'image' and 'imagination' have maintained a precarious foothold in psychology for the last quarter of a century. At present writing, with the epithets 'eidetiker' and 'introvert' in the air, only the hardier dare avow the persistence in our daily thinking of the imaginal taken in its more flagrant forms. Yet the most scrupulous of objectivists, after carefully expunging the offending terms from his professional diction, must admit a grain of curiosity as to the sources of the strange delusion which the words connote. This curiosity Professor Bundy's scholarly and readable narrative is eminently calculated both to whet and to allay. Planned originally as an excursion into critical sources, in the interests of the time honored literary controversy over the respective provinces of fancy and imagination, the study expanded in the author's hands, taking on new objectives. The volume issued is announced as the first installment only of a comprehensive survey of theories of imagination and fancy, from the dawn of Greek thought to the present. Thought is a unity, asserts the author in the Preface; the history of critical and esthetic terms must inevitably be bound up with the contemporaneous conclusions of epistemologist, metaphysician, and psychologist. Hence the rational procedure of the critic is simply to allow representatives of successive ages to speak for themselves; explaining their utterances, not in the light of his own theory but as having come from certain schools of thought, from certain personalities, at certain times; thereby revealing the drift of certain great traditions.

In Bundy's *Theory of Imagination in Classical and Mediaeval Thought* the psychologist may therefore trace not merely the varying fortunes of the two terms in question (εἰκασία from the verb εἰκάζω, to copy—later *imaginato*—and φαντασια, from the verb φαίνω to appear, or φαντάζω, to render apparent). He will find also a lucid and fairly leisurely account of descriptive psychology itself in the making; from the first tentative differentiation of the 'existential' and the 'experimental' in Pre-Socratic fragments, through the painstaking piling up of observation, the gradual clarification of the boundaries between stimulus and sensation and image, at the hands of classical philosophers and medieval schoolmen; a story told in large part through the medium of copious quotations from the works of Plato, Aristotle, Epictetus, Philostratus, Augustine, Aquinas, Dante (to mention only the more prominent sources); the whole linked by a lucid running commentary by the author, who furnishes also a final and definitive summary. Especially significant from the point of view of the esthetician is the evidence (gathered from many quarters) of the gradual re-infiltration of the terms εἰκασία and φαντασία into esthetic criticism, from which they had been rigorously excluded by Aristotle, on metaphysical rather than critical premises. Excerpts from the works of Dante, illustrative of the final synthesis of medieval theories of the function of the imagination, in Chapter XI, are perhaps of interest to the literary critic rather than to the psychologist.

Much of the material presented, it is true, is available in other sources, in the works of Croce, Saintsbury, Bosanquet, Ambrosi, Schweitzer, and in translations and histories of psychology and philosophy. Nowhere, however, is it accessible in such compact yet comprehensive form, subject to so little distortion at the hands of critic or translator; with the original text of significant passages bracketed in the translation.

In this wealth of material, representing over fifteen centuries of meticulous observation, the psychologist of an inquisitive turn may find more or less clearly adumbrated the majority of the skirmishes and squabbles of the rival psychological sects of the last two decades over the status of the image. The functioning of imaginal factors in judgment, in perception, and in thought, (fought over in the experimental studies of the nineties and the first decade of the current century) was, it seems, a pivotal point in the psychology of Aristotle, though admitted only with reservations by Plato and his followers, the Neo-Platonists and Christian mystics. In the sixth century, Olympiodorus anticipated certain recent criticisms of this position, declaring that "imagination, sailing along with reason, makes the waters turbid rather than filling the sails;" thereby antedating in a fashion the findings of the proponents of *Bewusstseinslagen*, 'awareness,' and 'pure thought' processes, who will have it that imaginal thinking is circuitous and childish.

The distinction between sensory and imaginal, between imaginative and memorial (productive and reproductive imagination) was elaborated perhaps as carefully as was possible prior to the development of laboratory technique by Augustine, who noted also the supplementary function of imagery in reading and understanding. Long before his day, the shadowy, filmy character of the visual image, certain peculiarities of after-image, of hypnagogic imagery, as well as the fine gradations between perception and illusion, and the limitations of the functions of phantasy to reinstating or recombining earlier experiences, had been remarked upon. Neither was the investigation of the permutations and commutations through which a given imaginal content may be run the inspiration purely of a twentieth century laboratory genius. This, it appears, was a favorite medieval pastime, if numerous commentaries on the origin of chimaeras, black swans, and New Jerusalems are to be trusted.

There are, of course, many gaps in the available data. The psychologist will miss, in these early records (if we except the brief sketch in *De Anima*) any comprehensive theory of the imaginative function. Of imagination as an integrative factor, operating now as the mere enriching, cementing and coloring of perception and action, now rising to a more dominating rôle in illusion, hallucinations, dreams, poetry and art, culminating in a shadowy but real supremacy in symbolic functioning (supported by neural and organic trends) in thought and understanding, there was naturally no recognition. The generic or abstractive and schematic image of Plato and of Aristotle, listed also by Augustine as in the nature of a 'counter,' was unfortunately lost sight of, to be recovered only after many centuries. Imagery from senses other than the visual while admitted was not stressed, neither was synaesthesis or the matter of imaginal types apparently broached before the modern era. Ideo-motor action and the relation of anticipatory imagery or imaginal rehearsal to imagery were, however, matters of keen concern to the Stoic, as earlier to Aristotle. Epictetus remarks that 'the ass could not walk without images,' and Augustine that 'not even the shortest syllable could be articulated in its order unless imaged.' The possible pernicious effect of 'phantasies' or 'phantasms' upon the morals also received attention, and sample apostrophes were appended for the use of those afflicted, such as 'Phantasie, you are in no manner what you appear to be, etc.' Similar admonitions may soon be expected to appear in our up-to-the-minute text in psychology.

From Plato on, the intimate association of phantasy with the appetitive or emotional nature (of deep significance for later theories of poetry and imagination) was a commonplace in critical thought, remarked upon by Proclus, Synesius and others. Nor did the functional relation of dreams and emotion escape these acute observers, who antedated Freud by nearly fifteen centuries. Many other peculiarities of dreams in dispute today (especially between dreamers and non-dreamers!), such as the occurrences of images within the image, of concomitant thought processes, of the occasional dropping away of the illusion of reality, were observed by Aristotle. As for the vivid phantasying of murders, fights and distant scenes, the cultivation of which is urged by Quintilian upon the youthful orator, it requires no vast straining of the text, no considerable reading between the lines, to discover here the prototype of Jaensch's eidetic imagery.

The possibility of a telepathic function based on imagery was (we learn) broached in the correspondence of Augustine; and the notion of the purely mental character of the image (as over against sensation), a construction fearfully disturbing to the morale of the behaviorist today, was favored by certain early thinkers, though flatly contradicted by the majority. Evidence for and against the localization of imagination in the front ventricle of the cerebrum, thought and memory in the middle and rear, was carefully thrashed out by the early schoolmen; the sub-conscious functioning of the image in recognition was broadly hinted at by Albert, and the exact point in the animal scale at which the imaginal function might reasonably be postulated hotly discussed, the more cautious denying imagery to 'ants, worms and insects.'

Such casual gleanings, however diverting and instructive to the psychologist, are of course incidental merely to Professor Bundy's main purpose, and the above inventory does scant justice to the zeal and acumen with which he pursues his main objective. For it is always with an eye to descrying the first intimations of imagination as Wordsworth conceived it, 'reason in her most exalted mood,' that he scans the early records. The first foreshadowing of the imaginative function as neither 'icastic' (imitative and naturalistic), nor 'phantastic' (illusory and subjective) merely, but constructive or creative as thought itself is creative, and adept withal in conveying thought or emotion, symbol-wise, is the true object of his search.

The vigor and freshness of the author's handling are especially arresting in his diagnosis of the causes of the curious divorce of esthetic theory from imaginative function in the post-classic period. A too literal acceptance by succeeding generations of the diatribe against art placed by Plato in the mouth of Socrates in the *Republic* (and in all probability designed as *reductio ad absurdum* of the premises of Eleatic dogmatism) militated against the truer evaluation of art and of its relation to imaginative functioning, implicit both in the *Phaedrus* and in the philosopher's persistent use of myth and dialogue. To minds in the grip of an uncompromising idealism, the emphasis upon the sensuous origin of the 'icastic' image, and later upon its physiological subtrate in the cerebrum, would inevitably fix upon it the stigma of materialism; while its status as a copy of a copy, in the austere judgment of those for whom the 'immutable idea' alone spells reality, would derogate even further from its value. It is obvious that art products, inspired by imaginal elements defined thus grossly and connoting therefore merely the reëmbodiment of the image in a material medium, would sink to the level of imitations thrice removed from reality. *Phantasia*, moreover, the imaginal viewed as appearance merely, subjective, variable, and fleeting (though later identified with imagination in its freer, more creative vein), could boast even fewer qualifications as representative of the 'immutable idea.' In the distinctions and disqualifications thus conveyed, remarks our author, we encounter the first hint of the divergent ways of fancy and imagination.

Adherents of the more prosaic tenets of Aristotle were equally hampered in the grafting of esthetic theory upon imagination. For in his zeal to cut adrift from the transcendentalism of his master, Aristotle deliberately suppressed, in the *Poetics*, the εικασια and φαντασια of Plato, foisting instead the word *mimesis* upon esthetic theory; a stroke whereby he bred confusion in generations of puzzled commentators, condemning art for centuries to quasi-realistic, conventional standards, and a puritanical abhorrence of the 'phantastic,' (a warning to contemporary zealots, who would lay a ban on the use of the term *image* and *imaginal!*). For a brief moment, in the time of Philostratus, under the impact of Egyptian symbolism upon the naturalistic art of Greece, it appeared as if 'phantasia, a better artist than imitation' might come to its own; a hope destined, however, to frustration until the outbreak of Romanticism many centuries later.

Other features of interest to the esthetician in Bundy's narrative are the gradual emergence of imagination as an integrative factor, rather than a mere mechanical power of adding and subtracting items; the recognition of the function of the imaginal as the vehicle of emotion or insight—a dim adumbration of the 'penetrative imagination' of Ruskin, and the emphatic imagination of Groos and Lipps, and also a tentative foreshadowing of some of the artistic tenets of Nietzsche, Croce and Jung. To the psychologist of the visual or aural type, bored with the efforts of crusaders to replace all reference to the imaginal with a semi-scientific lingo of 'muscle-twitches,' we especially recommend these pages; confident like ourselves he will impatiently await the author's resumption of his narrative, and his quest down through the literature, psychological and critical, of the later centuries.

An excellent general index is appended to the volume issued.

University of Illinois ELSIE MURRAY

The American Negro: A Study in Racial Crossing. By MELVILLE J. HERSKOVITS. New York, Alfred A. Knopf, 1928. Pp. xi, 92.

In 1619 the Africans were first brought to America and were sold as slaves. In the three centuries since then the negro in America has come through a long period of slavery and dependence to freedom, of a sort. (There were, of course, some 'free' negroes before the Civil War.) Our knowledge of the ancestry of the negroes in this country is the vaguest, a fact that has, as yet vainly, prompted more than one investigator of late to seek funds for a thorough genealogical research in Africa. Even the processes of aculturation of the negro in American have today received no accurate description. In the present volume Herskovits, a young anthropologist, presents to the general reader the results of his investigations of the physical form of the negro as now found in America. The American negro presents a peculiar genetic problem: the progeny of any intermixture with the white race is called a negro if his African ancestry is known, even though genetically he may be largely of white blood. Moreover, the white blood in the negro seems to be mainly from white males of slavery times, and the principles of selection are not well known. Rankings of negroes in race testing according to degree of African blood are subject to error because of the fact that there are in America different strains of African stock with various pigmentations, and also that in earlier days negroes interbred to a not inconsiderable extent with the American Indians in the Southeastern States. Herskovits does not agree, of course, with the 1920 census figures, that of the 10,463,131 negroes in the United States 84.1% are full-bloods. His method of attacking the problem as to degree of mixed blood was merely that of asking the individuals measured by him to give their descent, a method whose limitations he recognized. He worked among Howard University students, the Harlem population of New York, and a rural community in West Virginia.

Herskovits classified the individuals measured solely on the degree of negro blood as reported by the several persons regarding themselves, with the following results: unmixed, 342; mixed with Indian, 97; more negro than white, 384; more negro than white with Indian, 106; about the same amount of negro and white, 260; the same class but with some Indian mixture, 133; more white than negro, 154; same, with Indian blood, 75; total 1551. Thus only a little more than 20% are unmixed, and between a third and a fourth of the total of 1551 have Indian ancestry. The only checks on the accuracy of these figures are the physical measurements of the individuals of the several groups indicated. It is known that as compared with the European as well as with the American Indian the negro has dark skin, thick lips, broad nostrils, long legs in proportion to total stature (*i.e.* relatively short sitting as compared with standing height), and other such identifying traits. The measurements of these traits were obtained, darkness of skin being determined by matching with mixtures of black, white, red and yellow disks on a color-top and noting the degree of the black segment. A small electric motor that can be kept near the skin would have been better. Results of these several measurements show a striking agreement between reported amount of negro blood and characteristic physical traits, thus establishing, in the investigator's view, the validity of the genealogical report method tried out. The reviewer, however, finds in the book no adequate consideration of the question as to how much these reports of ancestry may themselves have been influenced by the recognizable physical traits measured. The correlation may be wholly or largely influenced by such factors? In such a case, much of the value of the contributions of the research falls to the ground; and the probability of such a weakness at the very base of the work should be removed.

Herskovits then goes on to show from his data that, contrary to the accepted view that great intermixture of races increases the variability, the new amalgam "American Negro" actually shows, except for skin color, less variability in absolute deviation from the mean than the purer strains of most of the various peoples whose intermixture he represents. The important exceptions to this rule are, in nearly all cases, certain West African peoples. But does this then not support the generally accepted view which Herskovits thinks he has put out of court? If the variability of various African peoples is less than that of the American negro, and his is less than that of the Eurpoean, then *the intermixture has increased the variability* of the negro who came to America from those African stocks. Then how can our author hold that the American negro is forming a new racial type of his own? He seems, moreover, to accept the anthropological definition of racial type—small variability—based on the conception, generally verified in biology, that intermixture of types increases variability, and at the same time, as we have seen, to reject it in another relationship. This seems to be a contradiction that needs further consideration and straightening out. If multiple crossings give, in this case of racial mixtures, decreased variability, then smallness of variability surely cannot also in this case be taken uncritically as evidence of pureness of type. One must somehow be consistent. But Herskovits is by no means a careless or superficial student, and he recognizes that his results cannot be final even as applied to the negro. However, he does seem to overlook some of the inconsistencies. The genealogical data gathered by Herskovits indicate a progressive lessening of intermixture on the part of the negro with whites and Indians, which undoubtedly tends toward the stabilizing of the negro "type" in this country.

The chapter on "White Values for Colored Americans" is very instructive to students of race differences, whether psychologists or anthropologists. Herskovits points out that because of the social value of light skin in an environment whose values are so largely set by the white man, there is a premium among the negroes on lightness of color. He finds evidence of a selective mating in the sense that negro men usually marry women who are lighter skinned than themselves. His data from Howard University, obtained in reply to the question "Who is the lighter, your father or your mother?" showed that in 56.5% of the cases the mother was the lighter, whereas in only 30.3% of them was the father the lighter. He argues that the negro men select light wives for social prestige, while the women select men of ability who "can take care of" them. In this way he would account for the somewhat greater mentality found by certain psychologists in the lighter negroes, a difference which he failed to get from the students in Howard University. According to this view, it is not the white blood soley, if indeed at all, that counts toward making this difference, but the superior ability among the negro men which is selected by the preferred light colored women. The two factors may, of course, work together in the same direction. A predominance of light colored women was also found in the Harlem population studied. To refute the objection that lightness of color may be a sex-linked trait, Herskovits shows that the average degree of darkness of skin as measured in all of the men of the entire Harlem series is 69.5 as compared with 69.8 for the women—a practical identity. What, then, becomes of the lighter men and the darker women? The author does not know, but he offers data which indicate that the lighter men "pass" over into the white group and that the dark women probably become wives in second marriages. In such marriages he notes that the wife is usually darker than the husband, and older.

These views and results, of course, must have more support; and in view of the many uncertainties still existing as to fairness of samplings and as to the basis of the correlation between reports on ancestry and characteristic negro traits, mentioned above, it may well be questioned whether a "new type" of negro is actually being established as contended by Herskovits. Moreover, his data in no way contradict the hypothesis that, in general, white blood has a tendency to increase the intelligence of negroes; nor does his work discount the findings by psychologists as to race differences. The psychologist who has had experience in race testing in America is somewhat surprised at the readiness with which meagerly supported hypotheses are set up in this study. His experience has taught him to expect that such hypotheses will be contradicted in further studies in different localities and by different investigators. This is not mentioned here, however, to discount the work of Herskovits in general, but only to warn the reader against a too ready acceptance of hypotheses as settled facts. The hypothesis is the tool with which scientists work: he may himself demolish it in the next step when he gets more representative data or sharpens his technique.

J. P.

Readings in Educational Psychology. By C. E. Skinner, I. M. Gast, and H. C. Skinner. New York, D. Appleton & Co., 1926. Pp. xxvii, 833.

Volumes of 'Readings' in the various sciences are increasing in popularity. It is difficult to imagine that the thoroughly trained reader will be content to accept such volumes as a substitute for direct reference to the original sources in the literature, although he may find them of value as suggestive of the lines to be followed in reading. If the book to be reviewed here were intended for the latter use, the reviewer's task would be easy. It would be enough simply to state that the authors have collected a large amount of material from a variety of sources, that this material was fairly well arranged and that each chapter was followed by a list of references for further reading. But the authors of this book have designed it for use by the elementary student, basing their work upon the assumptions that "the students in educational psychology can profitably read much more extensively than is possible because of lack of time or available references," and that "it is impracticable for beginners to rely upon the reference shelf or the library because of limited facilities or the size of beginning classes" (vii).

Sooner or later educators will have to face the question whether a book of this sort, when put into the hands of the untrained reader, may not be far more of a menace than a help. There is ample reason to believe that the average elementary student will receive from this volume the entirely erroneous impression that it provides him with a systematic view of the entire field of educational psychology; or, worse yet, that he will be given the impression that the field of educational psychology is in a state of utter chaos.

The volume is compiled and edited from the standpoint of stimulus-response psychology. In an early chapter, the authors present a *fifteen-line* excerpt from Thurstone's criticism of this type of psychology (in an 'abridged' form!) but, as the rest of this chapter is devoted to most convincing diagrams of stimulus-bond-response relationships, the treatment can be called systematic only with difficulty. Later on, the authors present their editorial comments in stimulus-response terms, announcing, for example, that "learning is primarily the acquisition of new patterns of stimulus-response bonds" (479). But the student is not to be abandoned to any starkly mechanistic behaviorism, for 'purpose' is treated in a most generous manner throughout the book. The reason for this becomes evident when the authors finally capitulate and inform the student, in an editorial comment, that "in a modified behavioristic program . . . it is necessary to include purpose, as well as stimulus and response" (587). It is not difficult to foresee the danger to scientific method that is to be found in a *sixteen-page* contribution quoted from one J. E. Bentley. Here the student is warned that "a continuation of this practise (*i.e.* "materialistic and naturalistic systems of philosophy") would constitute a serious menace to the Christian religion" (744). Two pages further on the student is regaled with the account of a student who testifies that the materialistic methods of psychology drove him right into skepticism! He is told that "fortunately only a few extreme behaviorists have meddled with religion" (752) and is guided along the road to scientific understanding by the author's assurance that "man is not merely a machine, but a person who operates the machinery of the bodily self" (757). In contrast to the sixteen pages allotted to this author, the same chapter provides six pages for Koffka, five for McDougall and one for Morton Prince, while Ogden is allowed five pages for his explanation of the contributions of *Gestalt* to modern education. This arrangement should prove popular with certain state legislatures, but its value for the elementary student is open to question. Without editorial comment, the authors present three articles from the pen of C. L. Redfield, in which the student may learn that "by an increasing amount of mental work performed generation after generation, the mental power of human beings may be raised by successive stages to an indefinite extent" (122). This author states that he has "been able to find but one man of considerable eminence who was born less than 58 years after his paternal grandfather" (123), and further demonstrates the influence upon the child of the age of the father by quoting lists of famous names which demonstrate beyond the shadow of a doubt that the great warriors were sons of men who had not reached their thirty-first year, while the great philosophers came of fathers past their fifty-first birthday. This material is not only included here, but is published without comment!

Of the 833 pages in this volume, 113 are devoted to the subject of intelligence and intelligence testing. Although this is by far the bulkiest chapter of the book, it contains no general criticism of the testing program, and there is nothing in the chapter to suggest that the value of mental tests is open to any doubt. To reassure anyone who may have been depressed by results of such tests, the authors offer a selection which presents an array of facts about "Great men considered dullards according to school standards" (261) together with a cheery selection entitled "Almost every child good for something" (256). This latter excerpt holds out high promise for every child except "the idiots, imbeciles, morons and morally incompetent children" (256). Aside from the obvious dangers of quotations of this sort, there are other things to confuse the untrained student. Change from editorial comment to direct quotation is indicated only by a small change is type-size, which makes it necessary to refer backward a page or more to discover which type is being used in many of the selections. Few excerpts from the current journals are provided "because of the technical character of much of the periodical literature" (viii), and there is little in the volume to suggest that any experimental work of value has ever been accomplished.

European educators and psychologists are almost entirely ignored. The eight-hundred pages of the volume do not contain a single quotation from Köhler, Bühler, or Claparède. Spearman is allowed one contribution, while Koffka is represented by four distinct quotations. On the other hand, the index contains a full column of references to Thorndike, with James a close second. Hall and Colvin receive generous amounts of space and Dewey provides most of the philosophical background. On the other hand, the emphasis upon social psychology as the basis for education is good and worthy of careful consideration. The chapter on "Mental Hygiene" is a wise inclusion which leads the reviewer to regret that it might not have received some of the space devoted to 'intelligence.' The questions at the ends of the various chapters are rather too difficult for the elementary student; but they should prove to be of considerable value in helping the instructor to formulate clearly his own views on controversial issues.

In the hands of the psychologically trained instructor, the volume may be a genuine aid in the arrangement of facts and as a source of stimulation to more extensive reading. But for the elementary student the book appears to be a source of real danger, a fact to be considered by the teacher who proposes to use it as a substitute for the library shelf and the midnight oil.

University of Illinois JOHN G. JENKINS

Die Kindersprache: Eine psychologische und sprachtheoretische Untersuchung. Fourth edition. By CLARA and WILLIAM STERN. Leipzig, Johann Ambrosius Barth, 1928. Pp. xii, 436.

The importance now attached to verbalized reactions will make inevitable a closer study of the development of language in the child. As a summary of what has been done during the forty-six years since Preyer's pioneer study and as an indentification of certain problems in the field, this monograph in its rewritten form possesses permanent value.

As in the first edition of 1907, about one hundred pages are devoted to the detailed summary of the language used by the authors' two children up to the age of six. The entire range of vocal expression is reported, with the age at which the several elements first appeared and the circumstances that led to their appearance. The second division of the monograph is the most valuable for the general student of childhood, and has been entirely rewritten. In it the writers trace the development of language from the beginning cries of displeasure to the stage at which language is complete in its structural outline. Not only the data from the first part of the study, but all carefully recorded speech histories in the psychological literature are claimed by authors as a basis for their conclusions. Nearly one hundred titles are added to the bibliography of one hundred nine titles in the edition of 1907, and much of the newer work is taken into account in the revision.

Stern's underlying theory is in general accord with critical thought on the nature of development in the child. "Originally the manner in which a person adapts himself to a situation is expressed in a total reaction of the organism in which attitude, facial expression, and action, with the accompanying sound, constitutes an inseparable whole" (p. 123). 'Sound language' (or even 'script language'), which differentiates itself from this organic whole, does not seem to constitute a late stage and it is in reality only an artificial abstraction. Nor is there a separate drive for speech. The tendencies that lead to language are not purely linguistic in nature, but have a much more general character. Vocal expression begins with the cries of pain and distress; sounds denoting pleasure appear before the age of three months, and from these develop the babbling activities. During this period the child reacts to the voices of others and comes to understand the meaning of sounds. True speech begins when the child attaches his own sounds to definite objects and activities. The first words are in reality sentences. There is development in the sentence as such and also a differentiation of the separate words. The first adjectives describe the subjective nature of the child. In personal pronouns the possessive appears before the nominative. The growing language reflects the integrating processes in the child.

The language of children is not a lower grade of a perfect language, but has a form of its own. This form is examined in the concluding part of the monograph. The tonal elements of the early babbling period, the careful analysis of sounds in words, their elision, their transference to different parts of the word, and their doubling, the etomology of the language, and other like topics are discussed in an illuminating manner.

The study will have its largest and most appreciative use among those who trace the development of the child in all of its aspects. Any questions and criticisms from this group will need to take into account the particular point of view which from Stern examines the human personality. After this position is granted, the criticism will direct itself, first of all, to the use of certain concepts that cannot be uncritically accepted to-day as they were two decades ago. Imitation is unduly magnified as an explanation of the growth of language. We need to look for something more basic: the imitating and the ability to do it correctly must be explained. The known laws of learning undoubtedly operate in the acquisition of a language as they do in learning a maze or the multiplication table. Secondly, many of the problems discussed are experimental in nature, and any discussion should be on the basis of adequate objective data. Curiously, Stern is hardly appreciative of elaborate experimental investigations like those of M. E. Smith at the University of Iowa (p. 198) and J. Schlag in the schools of Leipzig (p. 233), although he quotes from a large number of such studies. In spite of the difficulty of the experimental approach, the final answer to a very large proportion of the questions discussed in the monograph—sequence in the development of word knowledge and sentence structure, conditions that lead to the use of certain parts of speech and particular forms of sentences, sex differences in every aspect of language, the effect of learning two languages at the same time, and many others—will not be found until they have been examined under experimental conditions. Various older theories of language development are considered helpfully in Stern's discussion, but such serious work in the field as that of Karl Bühler and that of the behaviorists is not incorporated with the theory presented in the monograph.

It is unfortunate that many studies of the language of children do not refer to Stern and show no internal evidence of having been influenced by his monumental contribution to our knowledge of the subject.

Florida State College for Women P. F. FINNER

Anwendung der graphischen Methode auf Sprache und Gesang. By E. W. Scripture. Seventy-two Figures. Leipzig, J. A. Barth, 1927. Pp. viii, 114.

It is well-known that Scripture has for many years worked on the problem of devising instruments for making accurate visual records of sound waves, particularly those of the human voice. The first part of this monograph describes his various pieces of apparatus for the analysis of speech and song curves, and discusses the technique of the measurements considered and methods of evaluating observational errors. Careful attention is given to the representations of duration, intensity, quality, and precision of different vowels and consonants produced in various personal and cultural settings. Sentences—declarative, exclamatory, and interrogative—and verses spoken by different persons both in English and in German are analyzed and shown to vary in innumerable aspects, displaying characteristic differences in the degree and the distributions of energy, duration of constant and of changing elements, etc. The energy changes are somewhat periodic, the periods varying with such factors as the content of the speech, the affective state of the speaker and his personality. The means and the percentage variations of these fluctuations are held by the author to be significant of the subject's mental condition. *E.g.* the speech curve of "hippopotamus" as pronounced by an individual with progressive bulbar paralysis shows increasing indefiniteness of demarcation between the elementary characters of the curve—the so-called speech atoms—until the limits of the characters vanish entirely and the elements become indistinguishable. The word begins with marked intensity and high pitch, and rapidly decreases in intensity to a point of considerable weakness, while in pitch it approaches the curve of normal subjects. It is not abnormal in its general aspects. The wave forms are, however, irregular, the duration of elements is increased, and vowels are somewhat chopped, thus decreasing in precision. Normal persons of different nationalities show characteristic differences in pronouncing *pa-pa-pa.* At one extreme the Scandinavians make the *ps* strong, while at the other, the French render them smooth without explosion. Different individuals in any cultural group also have their own characteristic positions between these two extremes, and for each person the position is relatively constant, giving a variability of 5—15%. In progressive general paralysis this may reach 200% or more. Epileptics, on the other hand, show a decreased variability which is claimed to be characteristic and cannot be imitated. The elements of the curve are flattened, making practically a smooth line. Scripture holds, as is well known from his past writings, that he can thus reliably diagnose epilepsy, and he gives several examples to illustrate his success; but he offers no proof of the validity of his diagnosis. As to the underlying neurology of speech, he contends that every nerve lesion accompanied by speech defects conditions a specific speech pathology; that abnormal speech manifestations are constant in cases which are caused by specific nerve lesions; that the subject cannot vary the speech curves of such manifestations to show marked irregularities or temporarily to assume normal aspects. Functional disturbances, on the contrary, show marked irregularities and discontinuities in their pathological aspects. A stutterer, may vary his curves from time to time, even to making them, for short periods, fully normal. Scripture holds that the epileptic, although manifesting decreased variability as mentioned above, may, through personal encouragement, show normal speech curves for short periods of time; and he therefore concludes, in agreement with the view of L. Pierce Clark, that epilepsy has its root in a constitutional disposition and that its seizure is fundamentally but the manifestation of maladjustment. How typical the various "characteristic" curves shown by the author for different disorders are, can be determined only by extensive experimentation and clinical study. The method is, however, both ingenious and suggestive.

Interesting applications of this graphical method are made both to the instruction of deaf-mutes in oral language and to the teaching of proper muscular control to students in singing. In the former case the individual who already has learned to speak is aided visually in the acquirement of proper emphasis, enunciation, and temporal relations among the different elements, and those who are just learning can study the effects of different vocal efforts. Both may compare their results visually with normal models. In the case of singing the learner may be instructed visually as to the degree of response of different muscles involved in the control of the breath, as well as in various other respects. No mention is made in the monograph of the excellent work in vocal analysis by Miller and by Seashore, though in certain respects the work of these investigators is vastly superior to that of the author.

J. P.

Temperament and Race. By S. D. PORTEUS and M. E. BABCOCK. Boston, Richard G. Badger, 1926. Pp. xvi, 364.

The reviewer feels that this book is more valuable for its suggestiveness than for its data. "Race" is used in the popular sense and "temperament" is nowhere defined, even as to the degree of innateness implied by the term; yet the word is used very generously even for traits now regarded as primarily intellectual. Indeed, intelligence tests are included as measurements of temperament. The book is apparently intended for popular appeal; a number of photographs of "typical" boys and girls of the several "races" are included. How typical they really are, seems wholly a matter of personal judgment.

The study reported was made in Hawaii. Following a brief, and somewhat inaccurate, historical sketch of race studies, often very inappropriately selected as to representativeness, the different "races" in Hawaii are severally characterized. Useful data on the history of different peoples in Hawaii are included, but the reader is left to determine the degree of reliability of these data. In this sketch one senses distinct biases on the part of the authors. Twenty-five judges (not described) are asked to estimate the Japanese, the Chinese, the Hawaiians, the Portuguese, the Filipinos, and the Porto Ricans as races according to planning capacity, self-determination, prudence, resolution, self-control, stability of interests, dependability, and conciliatory attitude, or tact. The results agree with the author's characterizations: the Japanese lead in planning capacity, the Chinese in dependability, while the Filipinos and Porto Ricans are low throughout, but relatively nearer the other peoples in tact than in any other trait. One is forced to ask how independently the judges acted of popular prejudices and of the bias of the authors. In "efficiency index" the respective peoples named above were rated as follows: 85.5, 82.6, 51.4, 60.0, 33.0 and 33.3. Between these indices and social adjustment there is found a positive, though not a high, relationship. "Social adjustment is measured in terms of absence of crime, school advancement, occupational classifications, etc. The Chinese have a smaller percentage of murder and manslaughter than the Japanese, and also greater success in school advancement, particularly in the grades. The Japanese are described as alert, aggressive, sagacious in sizing up a situation and foreseeing its trends, selfish and unscrupulous; while the Chinese are patient, conservative, uncomplaining, unselfish, trustworthy in business, and possess little cohesion. Absolute "brain capacity" is measured by an X-ray photography method worked out by Stevenson and Porteus, the validity of which seems not to have been established; and between such capacity, on the one hand, and intelligence and certain desirable temperamental traits, on the other, a high correlation is found. The number of cases is, however, small and there is little satisfactory evidence that the samplings are good. There is a suspicious enthusiasm for the method. We read that brain growth continues through the whole period of growth in stature even after all the "powers," both mental and physical, have reached maturity; moreover, that whenever arrest or cessation of brain growth has occurred at an early age, idiocy or imbecility exists. This is all to be taken merely on the assertion; no evidence is presented or weighed. Brain size is also in close relation to cultural level in the different "races." The evidence for this statement is culled rather uncritically from many sources. Boys are found to be superior to girls both in brain capacity and in intellectual ability and initiative. There is a blissful disregard of most careful work done by psychologists on sex differences, and even the popular will find numerous dogmatic statements that are hard to accept without question. For tests of intelligence, as well as of temperament, the authors—after a consideration of various tests and data from different "races"—prefer the Porteus maze, which they find eminently suited for race comparisons because of its independence of school training and of language effects, its stress on foresight, etc. This test finds the Japanese superior to Chinese children for all ages above nine years, which, we are told, agrees with expectations based on brain capacity. The book closes with a discussion of racial theories, racial futures, and education in relation to temperament. An annotated bibliography of twenty-six titles, remarkable for importnat omissions, is given.

The entire volume bristles with ideas and bold assertions, and is written in a vigorous (and therefore interesting) style. This is largely due to a marked freedom on the part of the authors from the usual limitations upon scholars, of careful conformity to established facts, and of considerations of data reported by other investigators, which contradict the position taken on the several points discussed. It is precisely for this reason that the book may do much harm to the development of psychology.

J. P.

Criminal Intelligence. By CARL MURCHISON. Worcester, Clark University, 1926. Pp. 291.

This book is the result of perhaps the most extensive mental test survey ever made of the inmates of prisons. Using the group tests, Army Alpha, Murchison examined the prisoners of New Jersey, Ohio, Indiana, and Illinois. The work is almost completely statistical. There is practically no sociological analysis whatsoever, and no qualitative psychological considerations. The book is divided into six parts and thirty-four chapters. Part I contains a preliminary review of earlier work and statement of purpose; Part II deals with white native-born male criminals; Part III with white foreign-born male criminals; Part IV with negro males; Part V with women criminals; and Part VI is a discussion of legal punishment.

Space does not permit a summary of all the findings. Some of the outstanding results on the native white criminal are as follows. There is a correlation between mobility and intelligence of criminals, *i.e.* those who are incarcerated outside their home state are more intelligent than the 'home-grown' variety. There is a segregation of types of crime, those of fraud and deception being committed by more intelligent persons, as measured by the tests, than are crimes of violence, sex and social dereliction. On the whole, recidivists are more intelligent than first offenders, except in the case of fraud and sex crimes. In crimes of force and thievery differences are not marked. Moreover, the criminal is less literate than the American draft army, but migratory criminals are more literate than the others. Furthermore, literacy does not vary with intelligence in all the types of crimes. Highly educated persons are abundant in some groups and quite lacking in others. The lowest grades of literacy are not found in the recidivists. The author confirms the general opinion that youthful criminals are particularly given to crimes of violence, and that the fraud group is made up of older men.

Interesting comparisons as to industrial status and religious affiliations of the prisoners were made. One interesting fact was uncovered, however, that the criminals from parentage of divergent religious affiliations have somewhat higher rating than do those where both parents are in the same denomination. Some tentative conclusions are given on seasonal variations, length of incarceration, marital concomitants, and height and weight differences.

Some of the facts on the white foreign-born male criminal are these. It appears that the more superior nationality groups, as found in the army testing, produce also the more superior mentality groups among criminals. As to types of crime among the foreign-born group, individuals committing crimes against property tend to rank higher than do those committing crimes against persons. But there is considerable variability here among the various nationality groupings. The foreign-born recidivist is also more intelligent than the first offender. The so-called superior nationality groups of foreign-born produce greater tendency toward recidivism than the so-called inferior 'races.' The analysis of literacy corresponds fairly with that for the native-born group. If literacy is a 'causative' factor at all, "it seems valid to state that literacy correlates positively with the number of criminal convictions, and with the degree of deception and fraud involved" (p. 195).

The investigation of the male negro criminals show that the negro criminal, on the average, is superior to the average negro draft group, but that the entire negro criminal population of the four states examined is inferior to the negro draft from those states. This result is attributal to the inferior intellectual status of the negroes who are confined in states other than their native states, and it contrasts sharply with the situation in the case of the white criminal groups studied. In regard to the facts of intelligence and recidivism, the negroes are much like the white male criminals, but the negro criminals are slightly older, on the average, than the whites. Moreover, on the few cases given, negro Catholic prisoners were superior to the Protestant negro criminals which is the reverse of the situation with the white criminals. So, too, in contrast to the whites, married negro criminals are somewhat less intelligent than the unmarried.

The conclusions about women criminals are very tentative due to limited samples. White and negro women criminals are less intelligent than white or negro male criminals. With white women sex crimes are most common, but this is not true of the forty-one cases of negro women criminals. In the latter group the type of crime is like that of the negro men.

The final section on legal punishment has no relation to anything which precedes it in this monograph and lays the author open to attack as uninformed of work in modern penology. He recommends the abolition of the jury system, of the indeterminate sentence, the release on bond, and the parole system. He also recommends "uniform punishment for the insane, the feeble-minded, and the young" without defining what he means by "uniform punishment." So, too, he recommends that "the third penitentiary conviction to carry an automatic death penalty", and in general "the application of the deterministic philosophy to the behavior of the State as well as to the behavior of the criminal."

University of Wisconsin KIMBALL YOUNG

The Springs of Human Action. By MEHRAN K. THOMSON. New York, D. Appleton & Co., 1927. Pp. xvi, 501.

Thomson says in the Introduction that he wrote this book to satisfy the imperative need for a comprehensive scientific study which would account for the springs of human action. By springs of action he apparently means whatever motivates human conduct. He uses the terms motive and motivation to designate "all the incentives, drives, urges, motives—conscious, semi-conscious, and the so-called unconscious" (p. 20).

The long bibliography and the many footnote references indicate that much literature was examined, but the critical reader will frequently wonder at the selections and at the omissions. Although apparently fond of such phrases as "scientific evidence" the author's sources are usually not those of experiment nor of systematic induction. There is much use of secondary sources when primary sources should have been available. Scant attention is given to the work of the psycho-analysts who have had much to say about motivation. Holt's *Freudian Wish* and Freud's *Beyond the Pleasure Principle* are apparently not known to the author. Hall's *Adolescence* is not mentioned. Coué is given a respectful consideration. Telepathy, which the author seems inclined to accept, is discussed with refernces only to Jastrow and Carrington. For his general psychology the author leans heavily upon Woodworth, McDougall, and Pillsbury. But the authors quoted should not be held responsible for the curious and contradictory statements which appear in this volume. Of such there are many. There are said to be "nine senses" (457). The esthetic sense is aroused by or is derived from a "sense of harmony among the sensations" (458). Ideas have a "tremendous influence upon the psychophysical mechanism" (137). Paramnesia is an hallucinatory phenomenon (297). Rewards and punishments are said to produce after-images. "A positive after-image leads to a repetition of the act and a negative after-image arouses the inhibitory reactions because of unpleasant associations" (130). One of the elements which may factor in a compound is "primary emotions plus sensibilities" (97 f.) Another such element is "emotions plus feelings" (98). Feeling is presented as a complex of sensation and affection, but on the very next page (118) feeling is described as a "general state or attitude" which so far no one has been able to "break up . . . into further elements." The Freudian system is presented as including "two censors, one at the threshold of the fore-conscious and the other at the threshold of the conscious" (292). The author includes a remarkable elaboration of McDougall's well-known attempt to analyze complex emotions (98-104). Fifty-four different compound emotions are itemized and with each there is a list of the chief components. Sentiments and primary emotions and complexes all appear as components of these compound emotions. If the author carries any distinctions in his thinking, between instinct, feeling, emotion, sentiment, and complex, he has failed to make them clear. Mood is presented as a "more permanent affective state of mind" than is emotion "but less so than temperament," and further that "mood is less turbulent than emotion or sentiment" (106). Apparently then mood, emotion, temperament, and sentiment, are comparable states of mind; but upon turning the page one discovers that temperament is something which "determines the type and intensity of the various emotional states" (108). Attitude is defined as a "composite of all the subjective factors of attention, heredity, experience, and present purpose" and this is both "conscious and unconscious" (166). Faculties also appear in the author's scheme. Personality is presented as the agent of an operative will (172), which is described as relatively free—although the limitations placed upon it would seem to leave little room for free action (172 ff.).

There is a "critical faculty" (305) which is asleep during hypnosis. Again, there is a "faculty of the mind not to tolerate a question for long" (325). And so on throughout the 500 pages of the book.

Reflecting upon the experience of reading this book the reviewer recalled that alluring statement in the preface, that the book was to be more than an eclectic presentation, that it was to be a "new presentation." So he went through the book again looking for the new, fearing that perhaps in marking the confused and confusing statements just quoted he had overlooked its contributions; but again he failed to find it. There is, to be sure, an unusual if not unique succession of topics; but the reviewer cannot recall that these topics ever have been considered except as factors in human conduct. It seems after all then to be little if anything more than an eclectic presentation, which might have been of much value but for the author's indifference to logical and psychological distinctions which most psychologists consider important.

University of Oregon EDMUND S. CONKLIN

The Ego and the Id. By SIGMUND FREUD. Translated by Joan Rivière with a translator's note. No. 12 of the International Psychoanalytical Library. Edited by Ernest Jones. London, the Hogarth Press, 1927. Pp. 88.

Although quite a small volume *The Ego and the Id* is full of a great deal of new material and cannot be adequately condensed in any brief abstract. Freud starts out by minimizing the importance of dividing the mental life into the conscious and the unconscious, which division has been one of the primary assumptions of psychoanalysis. He feels that some other formulation must be substituted in order to continue with the researches of psychoanalysis. He, therefore, substitutes the ego, the id, and the super-ego, and proceeds to build up some new conceptions from this.

The id represents the earliest organization of the mental life of the individual and contains within itself the emotional and instinctive forces. It represents the source of energy for the individual. "The ego is that part of the id which has been modified by the direct influence of the external world." All of the ego, however, is not conscious, a part of it being unconscious. The id is unconscious. The ego has the task of bringing the influence of the external world to bear upon the id and its tendencies and endeavors to substitute the reality principle for the pleasure principle which reigns supreme in the id.

The super-ego is formed from the primary identification with the parent. The super-ego is regarded as a "precipitate in the ego" and is a modification of the ego. The super-ego, or ego ideal, as Freud also calls it, derives its characteristics from the way in which the individual deals with the Oedipus complex. The super-ego corresponds in many respects to conscience and from it are derived religion, morality and a social sense.

Freud then goes on to postulate two classes of instincts, the first of which he calls Eros, or the sexual instincts, the second which he calls the death instinct. In a physiological sense, Eros represents the tendency to increase the amount of living organic matter, whereas the death instinct represents the tendency to break down living matter and to increase the amount of inorganic matter.

Freud next brings out several points with regard to mental disease. Certain patients do not improve as they should under psychoanalysis. This is because of an unconscious feeling of guilt which appears to the patient simply as a feeling of being sick. This represents a most difficult type of case to treat since the physician is tempted to use his own personality to aid the patient's recovery. Freud rejects this method of treatment and concludes with what appears to the reviewer to be a most important statement. "After all, analysis does not set out to abolish the possibility of morbid reactions but to give the patient's ego *freedom* to choose one way or the other."

Freud next discusses the obsessional neuroses and melancholia in connection with the sense of guilt. He feels that in melancholia the ego recognizes its guilt and allows itself to be punished by the super-ego, whereas in the obsessional neuroses the ego has not accepted completely the idea of guilt and attempts to repudiate it by the help of the physician. Freud also states that this unconscious sense of guilt may turn a person into a criminal. He finds that a strong sense of guilt has been present in many criminals prior to the committing of the crime, and concludes that this sense of guilt was not the result of the crime but the cause of it, maintaining that the individual experienced relief at being able to attach the unconscious sense of guilt onto some actual fact.

The id is described as being "totally non-moral." The ego "strives to be moral" and the super-ego "can be hyper-moral." The fear of death is thought to originate in the fear of castration.

The book represents quite a new development in Freud's ideas, a frank admission that some of the older conceptions of psychoanalysis have been incorrect and inadequate and an attempt to introduce new formulations which will be more satisfactory. The book is highly stimulating to thought and can be recommended for very wide reading, whether one accepts or rejects the psychoanalytic doctrines.

The translator is entitled to the greatest praise for giving a translation which is simple, intelligible and of a high literary quality.

Boston Psychopathic Hospital KARL M. BOWMAN

Psychology by Experiment. By L. W. KLINE and FRANCES L. KLINE. New York, Ginn & Co., 1927. Pp. 338.

This is a laboratory manual designed for introductory courses in psychology. After two very brief chapters which serve to orientate the student with respect to experimental method and general procedure in the psychological laboratory, the authors devote the remaining chapters to 109 experiments. The list of topics covered and the relative weight given to each is indicated by the following chapter headings and the number of experiments under each one: movement (including various types of reaction times), 13 experiments; sensations (including the more common psychophysical methods), 29; attitude, attention, and selective action, 11; retention and association, 13; perception, 14; learning, 7; memory and imagination, 11; reasoning, 6; affective reactions, feelings, and emotions, 5.

The number of experiments is obviously and intentionally too great for a single course, but this allows for a selection. Often there is an alternative between an experiment which requires more elaborate or expensive apparatus and one which does not.

Every chapter is introduced by a number of quotations. These are evidently intended to define for the student the concepts which appear in the title of the chapter. It seems to the reviewer that this would be more confusing than helpful to the neophyte, if he can comprehend the rather condensed and isolated statements at all. The author's own explanations of the concepts in a few paragraphs of introduction which follow will probably serve the purpose much better. A topical outline in each chapter covers the subject matter under the title of that chapter. This, in the reviewer's opinion, is entirely out of place, especially at the beginning of the chapter. A logical outline is more or less a personal product and has little significance to another individual unless that other individual has already covered the ground and has followed much the same line of thought. In the student's own mind, an outline is the last thing arrived at, therefore it might appear in the last part of the chapter if at all. If the outline is included for the sake of instruction in the topics with which the experiments are going to deal, then again, I would point out that each individual instructor would want to present the topics in his own personal way. The book can be used, however, without the inclusion of these outlines. The experiments are of such a nature that they can be introduced in any desired manner.

The experiments themselves have been very carefully worked out, and seem to have stood the test of former use. Some of them are the traditional experiments of Sanford, Titchener, Witmer, and Judd, but the greater proportion of them is new. The student is stimulated with pointed questions and suggestions throughout the instructions so that he may get the maximum returns from his labors. The authors' references for reading at the end of each experiment and the extensive bibliographies at the ends of the chapters should develop scholarly habits of reading the psychological literature as well as habits of genuine research. A list of questions and exercises at the end of the chapter will serve for valuable discussions or for written reports in which the student is required to apply the principles which he has determined experimentally.

In these days, when introductory courses in psychology differ so much from one department to another, and when so many laboratories devise their own experimental procedures and select their own problems, one might think that a laboratory manual would find little place. Yet, probably the majority of departments in which experimental psychology is taught are dependent upon a manual of just this sort. To these laboratories a manual so catholic in its scope should be most welcome. There is a certain value, after all, in placing in the hands of a beginning student a carefully worked out manual such as this one is. The traditional manuals are sadly out of date, not in their methods so much, as in the subject matter which is now emphasized. The reviewer does not hesitate to recommend the manual for use as a guide to students and for its high suggestive value to instructors in beginning psychology.

University of Nebraska J. P. Guilford

Heredity and Human Affairs. By E. M. East. New York, Charles Scribner's Sons, 1927. Pp. xi, 325.

This book by the author of *Mankind at the Crossroads*, a botanist of wide biological perspective, is of interest to psychologists from two standpoints. In the first place, it includes certain data from ratings and psychological tests as applied both to individual and to racial differences, and it organizes them with other biological data into a genetics of scientific humanism. The psychologist can hardly wish that these data had been somewhat more representative and up to date. The more recent and critical studies are slighted. The psychologist is also interested in the book because it affords him an authoritative and timely statement of the various developments in genetics as they bear on human traits. Convenient summaries of the contributions by various investigators of the heredity of human traits are included. In the interpretations by the author the reader is constantly reminded that the individual's traits are to be understood, in any case, not in the gross terms of his race or even of his parents as complete organisms, but rather in terms of the 'packets of genes' from which he came. These genes are carried in the 48 chromosomes of each parent, which afford an enormous variety of probable combinations in fertilization, and therefore of human characters. Following a presentation of the mechanisms of heredity as now represented in Mendelism, applications are made to the facts of sex determination and of sex-linked traits—hemophilia, red-green color-blindness, atrophy of the optic nerve, night-blindness (hemeralopia), and coloboma, a defect which usually shows only in the iris, but may affect lens, retina, and choroid coat. The last four defects may prove to be complex types, and coloboma is not yet certainly established as a sex-linked trait. Other serious eye defects that are not sex-linked are also considered, as well as congenital deafness, abnormalities of the teeth, form and color of hair, skin color, baldness, etc.; and, finally, feeble-mindedness and forms of insanity, which may be due to the interaction of very many genes, following, however, the simple and well-known Mendelian laws, are taken up. Effects of marriage between near kin are considered in considerable detail, as well as certain race differences in intelligence and in other traits. While the author recognizes that individual differences within any race are greater than race differences, he attributes practically all great accomplishments by negroes in America to the white blood in them, *i.e.*, to the effects of their genes from the white race. Genetically, race characters are often ill-defined. A valuable discussion is given of the possible influences of nurture on native race differences.

Much emphasis is put on the evidences of degeneration in the human stock of leading nationalities, due to a constantly decreasing reproduction rate of superior as compared with inferior individuals. The author shows convincingly that most schemes for charity result only in the acceleration of this degeneration process, and he looks upon an intelligent birth-control as the greatest hope of solution. "Until birth-control is recognized as a necessary social prophylactic by public health officials and family welfare organizations, this well-intentioned philantrophy and social service is nothing but a brutal gesture to posterity" (p. 307). East sees hope in the working of birth-control methods, even among the lower-ability groups, from the fact that the lower economic strata have shown significant declines in birth rate after the passing of laws in England successively increasing the restrictions on child labor (Pearson), thus no longer permitting children to be made a financial asset.

This book, by a high authority in genetics, is written in an engaging style which should take it into many non-academic circles where its effect is sure to be beneficial toward the establishment of a saner and more scientific view of human life and its possibilities. Definite programs for race improvement will doubtless follow a more general acceptance of the scientific point of view.

J. P.

Psychology: Its Methods and Principles. By F. A. C. PERRIN and D. B. KLEIN. New York, Henry Holt and Co., 1926. Pp. x, 387.

This book is one of the type which grows increasingly popular—a psychology which, while mainly behavioristic, still maintains a respectable family connection with introspection and conscious states.

The section on the "Biological Foundations of Behavior" is largely the physiology of the receptors and effectors, with unusual emphasis upon the glands. Since this is a text in psychology it would be desirable to treat at greater length the mental aspect of the function of these organs. The omission of the facts of color vision and auditory experience is undesirable although there is some compensation in the inclusion of more than customary material on pain, the static sense, and kinaesthesis.

The chapter on the "Psychological Foundations of Behavior" is the best chapter in the book despite certain questionable or inaccurate statements. On page 96 there is a confusion in the statement of the three possibilities of the sequence and duration of stimulation in the conditioned response; (1) and (3) as there stated are the same. On page 90 it is suggested that lightning as a cause of fear is a substitute stimulus for the idea of destruction and death. Would it not be better to point out that though this is the common explanation of the layman the fear reaction probably came first, and for most people still does, come to the *thunder* and that the suggestion of destruction and death as the cause of the fear is a rationalization? Again, the term *selection* is said to be preferable to *attention* (p. 79) but the latter word is the one which the authors continue to use (pp. 292, 364, etc.). The topic of conditioned emotional reactions, which is merely suggested in most introductory texts, is very well presented.

Two groups of factors in human motivation, the physiological and the social, are recognized. The treatment of the physiological motives (which includes emotions) is satisfactory; but that of the social motives is inferior because it is largely descriptive rather than explanatory. That there is a mutual relation between biological and social motives is stressed; but the nature of this relation is not sufficiently demonstrated. Since the mechanism of the conditioned response has been used so ably in other parts of the book (in the elimination of instincts and in the explanation of learning, for example) the reader wishes that it had been pressed into service again to make the origin of the prestige motive" less obscure.

There are two especially good features in the chapter on learning: the discussion of the part which consciousness plays in the learning process, which is an original contribution, and an unusually clear summary of a number of theories of learning.

Much of the last chapter is devoted to the proof of the theme that thinking is a form of substitute behavior. The mechanism of the conditioned response is again successfully utilized to account for the process of substitution of certain symbols for overt behavior. One wishes, however, that this excellent account might have been somewhat condensed and more attention given to various types of mental activity such as reasoning, memory and imagination. The interest of elementary students in these topics is strong and should not be neglected. These subjects could and should be developed without a reversion to faculty psychology.

University of Chicago DORRITT STUMBERG

A New Method of Mental Testing. By J. J. STRASHEIM. Baltimore, Warwick and York, 1926. Pp. 156.

Strasheim believes that mental testing has reached an impasse for two reasons: (1) there is no acceptable definition of intelligence; and (2) there is a need of new research upon intelligence in order to free the present tests from their limitations and to give them new life.

He starts out with the theory of Spearman that intelligence is a purely cognitive capacity involving the ability to perceive fundaments and to educe relations and correlations between them. He devised tests especially to measure this capacity.

The first of three groups of tests consists of three 'situation' tests. They are similar to the Binet tests of comprehension in which the child is asked "What would you do if—?" In Strasheim's tests, however, the situation is introduced in the form of a story. The tester is told in the beginning how to meet a certain kind of situation. The story is told a number of times with variations. Every time the testee has to meet similar but increasingly greater difficulties. He may use his previous knowledge gained in the test, or perhaps also from his earlier experiences, but must modify his method each time to fit the situation.

The tests seem well adapted to demonstrate Spearman's theory of intelligence but seem to be highly impractical as a psychometric tool. Too much will depend upon the individual tester. The procedure is long and tedious and the results are hard to quantify.

A second group of tests was devised to measure intelligence as it plays a rôle in learning. The testee is given a series of 'pencil and paper' mazes in which he is to discover by repeated trials the general principle which determines the correct path. These tests seem to be more suitable for quantitative purposes but the author has done little to validate them or to establish a scale. The number of testees was small and they were merely classified as young or old, bright or dull.

A third group was composed of cancellation tests. The testee has to cross out from 2 to 8 different letters wherever they appear in pied type. The letters to be crossed out are kept in mind either perceptually or by memory. Another variation is to have certain letters crossed out only when they appear in combination with other letters.

The author makes detailed analyses of the results from these tests, demonstrating their relation to Spearman's theory. He also points out at every opportunity the application of this theory to the problems of education. There are occasional polemics against the theories of learning which make of it a purely mechanical 'chance' process, *e.g.* the trial-and-error notion. What he has to say on this subject will bring relief to those who have a chronic autonomic tension set up by mechanistic theories of learning.

The book will be a disappointment to those who expect to find, as the title implies, 'a new method of testing,' all ready for use in measuring minds. To others it will be highly suggestive, if carefully studied, in its approach to the problem from a particular point of view. In the reviewer's opinion, however, it is the theory which is new rather than the tests. The positive gain is the attempt to see clearly what the usual tests of intelligence measure. Perhaps this attempt to develop tests carefully worked out on the basis of an *educed* theory is a typically British method as opposed to the trial-and-error methods of the hyper-kinetic American psychologists who are striving to accomplish the same end.

Kansas University J. P. GUILFORD

Statistical Methods for Students in Education. By KARL J. HOLZINGER. Boston, Ginn & Co., 1928. Pp. viii, 372.

This contribution is too thorough and discriminating to be "just another statistical text." In general level of difficulty it is harder than a number of texts which in their endeavor to be elementary gloss over difficulties and present statistical issues in the atmosphere of great simplicity which do not belong in this atmosphere. This may be illustrated by a quotation from page 27; "It will be noted that the median is here defined as the middle score." It would be characteristic of spuriously simple texts to stop at this point, but Holzinger goes on, "For an odd number of cases this definition offers no difficulty, but with an even number the use of the value half way between the two middle scores is a convention supplementary to the definition." Any number of similar illustrations wherein difficulties are made explicit rather than suppressed could be given, and in each instance the procedure followed makes for a sound treatment. Certain difficulties, such for example as the numerous situations in which small populations alter statistical measures, are not uniformly treated, but the book goes so much further than the customary statistical text in matters of this sort as to give a most favorable impression.

It is obvious that the author knows the mathematical shortcomings of students of education taking statistics, for he treats of certain matters ordinarily assumed, such for example as an elementary knowledge of logarithms. In the treatment of foundation mathematics the book is rather lacking in uniformity for a quite adequate elementary treatment of logarithms is provided but a very inadequate treatment of determinants.

Logarithms are treated in Chapter 4 and determinants are taken up in chapter 15, so those who have meanwhile dropped by the wayside never realise the later shortcoming.

The writer finds nothing particularly new in the subject matter and this is commendable in a book of this level, for it is much easier and of less value to devise novel techniques than to use soundly well-established formulas.

The later chapters are enjoyable reading for one with some slight mathematical foundation and should easily induct the high school teacher of mathematics into some of the mysteries of statistics.

Error in a book of this extent is practically unavoidable. The writer has noted just one that to him seems to be serious. On page 235 is given a formula for the probable error of the difference between two uncorrelated means. This is followed by an illustration wherein as a preliminary step "two groups of pupils were equated with respect to intelligence and initial ability." Thus the illustration is a problem wherein there is correlation between means. The significance of a difference is the single universal problem of statistics and it is unfortunate that there should be an error upon this fundamental matter. The later treatment of differences, pages 242-243, is correct, but here the difference involved is that between correlation coefficients and not a difference of one of the more common types—between means, individual scores, or between proportions.

On the whole the book is well written, of excellent make-up, and sound and scholarly.

Stanford University TRUMAN L. KELLEY

Mental Abnormality and Deficiency. By S. L. PRESSEY and L. C. PRESSEY. New York, The Macmillan Co., 1926. Pp. xii, 356.

The authors state in the Preface that they have written this book with two purposes in mind: (1) To formulate material for a college course; and (2) To furnish a source of practical value to those interested in various special fields in which problems of personality and adjustment are frequently met. They repeatedly emphasize that their work is for students, school officials, social workers, and personnel managers.

The style and general plan of the book are very acceptable. Special note should be made of the good bibliography and the glossary of technical terms. There are certain high spots in the book, such as the discussion of feeblemindedness, the epileptic personality, and the degree of mental abnormality that the supposed normal shows. The case study outline that is presented seems rather awkward. It seems more practical to be less rigid in temporal divisions than this provides.

The reviewer takes issue with the fundamental note of the book. It is a book written by psychologists for lay people; so far as clinical psychiatry is concerned, therefore, it seems not only useless but pernicious to encourage these readers to train themselves to proficiency in the differentiated types of psychoses. In justice to the authors one must add that they frequently advise consultation with psychiatrists, but it does not seem that this counsel acts as an effective check. The book suffers as do most small texts, from a certain amount of didacticism. There are many isolated statements which a medical specialist would probably question, *e.g.* "cases of extreme excitability are comparatively common, and are, very likely related to disturbances in the functioning of some one or more of the ductless glands" (p. 4); 'a complete physical examination by a physician is usually desirable whenever this is possible. But since an examination is often impossible—frequently it is not called for" (p. 36); "type too fine to be easily read by the patient (manic) when in a normal condition is now read without the slightest difficulty" (p. 49); "she had a complete collapse and died in a few days of apparently a broken heart" (p. 104); the listing of an irregular pulse as a common symptom in neurasthenia (p. 110); the statement that one who is merely mentally peculiar and not psychotic can consider himself God Almighty, Himself (p. 136); that an investigation of the personality of the dementia praecox, "very likely shows that the patient was one of those children who develop slowly. Walking and talking were delayed and throughout childhood the patient was somewhat sickly" (p. 150); "such individuals (manics) may turn up with fair regularity every few years with a touch of excitement, amuse and delight the hospital for a while, perhaps, then go through a depressed period and then return to the world again" (p. 167); "the family of a person suffering from chronic alcoholism is likely to be alcoholic and defective" (p. 187); "the person who develops a delirium on one or two degrees of temperature is probably psychopathic" (p. 209); "congenital syphilitics are almost invariably feebleminded" (p. 221); and that "national prohibition has accomplished great things in regard to the alcoholic psychoses." (p. 296).

Although many of these statements might be tenable as unusual personal opinions of the authors, a text-book that appeals to a group without adequate scientific training for sound critical judgment in this field, is hardly the place for them. It seems to the reviewer that a book on a broader biological and economic basis would serve its purpose better than one with clinical discussions and clinical case reports as the dominant note.

Boston Psychopathic Hospital MANFRED S. GUTTMACHER

Ueber den Aufbau der Wahrnehmungswelt. By E. R. JAENSCH and associates. Leipzig, J. A. Barth, 1927. Pp. xxx, 567.

Here are bound together within the compass of a single volume fourteen papers which have been previously published in psychological journals. The content is fascinating, often tantalizing, but the volume is exasperating. It lacks the continuity which a well planned volume would have, and in bringing together a lot of papers there is produced inevitably much repetition. There is in the book much that one desires to refer to again and again, but there is no index. For a volume of more than five hundred pages this is a serious omission. There is, to be sure, a very full table of contents which is helpful although not entirely consoling. Nine of the papers are by Jaensch alone, three others by Jaensch in collaboration with Reich and Freiling, one is by Freiling alone, and the other is by Kröncke. There is no summary of the findings.

Much of the material presented is now well known to students of the eidetic image problem. The first paper presents in some detail a number of interesting and sometimes clever methods for the study of and detection of the eidetic image. It includes also a description of the T and B types. In other papers the play of the eidetic image in the visual perception of location, the effects of attention, and the effects of eye-movement are studied. There are some short papers which compare the experimental findings with a small number of anthropological works, with the conclusion that the eidetic image is common to primitive peoples. Some very alluring experiments are presented on what is termed the *Kovariantenphänomenen.* When some threads are so placed that there is no clue to their relative distances away except the appearance of the threads themselves, if one is moved another appears to have been moved also. This is likewise examined for its relation to the eidetic image. There is a long paper on the place of the eidetic image in the development of perception and a very long one (130 pages) on the biological relations. This last is primarily a detailed presentation of the development of the eye, emphasizing especially its close relationship to the brain.

The argument for an eidetic stage in development out of which have been differentiated our perceptual and conceptual processes, and for the continued influence of eidetic images in many individuals, is attractive and as the literature accumulates is almost convincing; but it can never be wholly so until there is experimentally obtained evidence more reliably based than most of that to be found in this volume. Frequently the experimental researches reported here were made on not more than half a dozen subjects, rarely more than fifteen. Differences are presented without any consideration for their reliability. In fact there is an almost complete indifference to the technique of statistical method. Perhaps the indications of these studies can be supported by research with larger numbers but until that is done one must hold tenaciously to the position of suspended judgment.

University of Oregon EDMUND S. CONKLIN

Brains of Rats and Men. By C. JUDSON HERRICK. Chicago, University of Chicago Press, 1926. Pp. 365.

Professor Herrick's book is a reëxamination of cortical function and its development in mammals, in view of the work of Lashley on the learning processes in rats.

Eight chapters are devoted to the evolution of the various parts of the brain, their functions, and interrelations. Three chapters are given exclusively to the experiments of Lashley with the author's own interpretation of the results. He agrees with Lashley that the cerebral cortex of the rat participates in habit-formation, but does not regard all parts of the cortex physiologically equipotential. There is some localization of function, at least in the projection centers, according to the author, but there is much more definite localization of function in the brain of man. Whereas the cerebral cortex of the rat exerts a relatively greater tonic effect over the lower centers, the human cerebrum has a relatively greater phasic function.

In the remaining five chapters the author deals with more speculative topics, such as the 'subconscious,' 'consciousness vital function,' 'vital reserves,' and the 'ends of human conduct.' He belabors himself over the old problem of mind-body relationship, and insists again and again upon the existence of consciousness, or 'introspective' data. While committing himself to a deterministic monism (17 ff.) as the essential position of a scientist, he later comes very close to an interactionism when he insists that awareness is a link in causal chain of events (Chap. XVI). While one may be convinced from his discussion that consciousness is an observable scientific datum, one does not feel satisfied that the author has found a logical place for it in a monistic system.

The interpretation of the important work of Lashley by one so competent as Professor Herrick is enough to attract the attention of anyone interested in the problem of learning or in cortical function in general. Although the book gets its motive and its title from this source, relatively little space is given to it. A great service is rendered, however, in the collection and interpretation of other recent anatomical and physiological data coming from various sources, and its application to the problem of the evolution of brains.

The material, on the whole, is presented with unusual simplicity and clarity, in view of its technical nature, so that the intelligent layman may find the book not beyond his depth, and interesting as well as instructive.

University of Nebraska J. P. GUILFORD

The Natural History of our Conduct. By W. E. RITTER and E. W. BAILEY. New York, Harcourt, Brace & Co., 1927. Pp. vi, 339.

This is a delightful study in comparative psychobiology written with a real charm. It deals primarily with the development of psychical activities as adaptive agencies. The author draws upon a wealth of material acquired through a mellow life of active work in zoölogy performed on broad biological and philosophical lines. After having passed three score years and ten Professor Ritter confesses that he has been "unwilling to suppress the emotional aspect of his responses to nature to any such extent as many scientists seem to do." This forms a dominant motif in his work and contributes largely to its charm. Neither as psychologist nor philosopher in the specialized sense of these terms does he make any claims for himself.

He assumes that brute activity lends as much assistance to the understanding of human activity as does brute structure to the understanding of human structure. Professor Ritter draws upon data from the lives of the protozoa as well as from the lives of the anthropoids. He recurrs to the similarities and differences in human activity. One cannot claim that in this work there is anything in the approach or in its material that is startlingly new. It contains, however, a wealth of interesting data well correlated for its purpose. There are many interesting discussions, among these are: the worth and limitations of paleontological evidence; maladjustive activity in its four phases resulting in waste of time and energy in useful materials, in injury to kind and in self-injury; and finally the tenet that man's superiority is not due to his cerebral cortical development alone but to the anatomy and physiological activity of his hands as well.

One misses certain pertinent discussions to which the reviewer feels Professor Ritter could add evidence of value. For example: Watson's work with infants and the questions of the inheritance of acquired characteristics, particularly in view of the recent important contributions to this subject.

There is a good deal of unnecessary repetition that could be omitted in subsequent editions, *e.g.* on page 162 there is a paragraph repeated verbatim from page 126. The author states and restates too frequently the aims and purposes of the volume.

On page 59 the results ascribed to the cutting, in the cat, of the vagus nerve are actually those that result from its stimulation.

Professor Ritter states in his preface that he is responsible for the material in their work and that Dr. Bailey, his collaborator, is largely responsible for its organization, readableness and cogent presentation. It must be said that she also did her work extremely well.

Boston Psychopathic Hospital MANFRED S. GUTTMACHER

General Introduction to Psychology: A Survey of the Programs of Psychology. By C. R. GRIFFITH. Revised Edition. New York, The Macmillan Co., 1928. Pp. xix, 607.

The second edition of Dr. Griffith's *General Introduction to Psychology* has been largely rewritten and amplified. The volume aims to present between the covers of a single book a comprehensive view of modern psychology. The treatise as it now appears is divided into the following parts: I. The foundations of psychology; II. Genetic psychology; III. Social psychology; IV. The psychology of the unusual and of the abnormal; and V. Psychotechnology.

In the first section, the usual topics of general psychology are treated in outline fashion. The biological point of view is maintained. A short history of psychology and a consideration of the great schools and of explanatory principles in psychology is also given in this part of the book.

The evolution of mental life is the main topic considered in the second part of the book, which is entitled "Genetic Psychology." The development of the animal and the human intelligence is considered. The inheritance of intellectual traits is clearly treated and the point is well made that the organism must always be thought of as the *product* of heredity and environment, and not as the *sum* of these factors.

In the first section on social psychology, general problems are first examined and then the "monuments to mental life," such as language, the system of law, and religion are considered.

A treatment of individual differences is aptly used as an introduction to the pages devoted to a consideration of abnormal psychology. Intelligence, mental diseases, hallucinations, dreams, occultism, hypnotism, and many other topics are here considered. It is rather surprising to the reviewer to find the ordinary sensory illusion treated in this section of the book.

After a consideration of the relationship between pure and applied science, the author turns in the section on "Psychotechnology" to consider character analysis, educational psychology, and the various aspects of psychology related to legal procedure, medicine, and commerce.

The book is certainly a valuable one. The student who masters it can no longer be ignorant of the secret that psychology as it stands today is a true miscellany. As one reads the informative pages of this book, he discovers that the psychologist is indeed versatile. He is experimentalist, or as the author prefers to say, "experimenter" in his own right. He is system-maker and systematizer of systems. He is neurologist and endocrinologist. He is a philosopher concerned with the freedom of the will and the existence of Deity. Last, but by no means least, he is advisor to that important American academic figure, the football coach.

Brown University LEONARD CARMICHAEL

The Expansion of Rural Life: The Social Psychology of Rural Development. By J. M. WILLIAMS. New York, Alfred A. Knopf, 1926. Pp. xvi, 346.

This is the second volume of a series on the development of what the author regards as a typical rural community in America. The first volume is *Our Rural Heritage*, published in 1924, which covers the early developments. Several other books have come from the author's pen, dealing with the sociological aspects of the same community. Central New York was chosen as the location for study because (1) it is the birthplace of the author, who has studied the community directly for a period of some twenty-five years, (2) it was settled by a variety of nationalities—English, Dutch, Huguenots, Germans, Scotch, Irish, Welsh—and therefore affords opportunities for observations on the mixing of different cultures, and (3) pioneers went from this section to build up various communities further west. The identity of the community studied is concealed; consequently the reader has no adequate check on the reliability of the sources used by the author. Fortunately, from this standpoint, the work does not pretend to be scientific, and no one would make such a charge. There are no references whatever to standard psychological investigations. The present volume traces the development of the community down to the present time. The two volumes in this series, on what the author calls "Social Psychology of Rural Development," record the attitudes of the people toward various of the factors affecting their lives, and the changes that are wrought in these attitudes through the successive developments that have taken place due to inventions, improvement in transportation and communication, scientific progress, etc. A long review is not justified here, but it is relevent to say that the two volumes are very informative as to the general life of the community. The evolution beliefs, attitudes and methods of coöperation, and the differentiation of interests are clearly portrayed. How much of this is really typical and objective fact, independent of the selection and bias of the writer, is the problem that is unfortunately left to the reader to solve. The treatment is, however, both interesting and suggestive to the social psychologist; because the data reflect popular views in their natural development, and give the roots, however reliably or unreliably, of many current beliefs and practices in rural communities of this country. It would be valuable to have thoroughly scientific studies of typical communities, but such work can be done only by the coöperation of scientists in many fields.

J. P.

Applied Psychology. By A. T. POFFENBERGER. New York, D. Appleton & Co., 1927. Pp. xx, 586.

Evidence of the practical tendency of the day and the consequent rapid expansion of applied psychology is furnished by the mere bulk of this edition, which succeeds "Applied Psychology" by H. L. Hollingworth and A. T. Poffenberger. The earlier volume, published in 1917, was little more than half as large. Numerous experiments performed since that date are described by Poffenberger, and other features are added to the text, which in the main follows the outline of the former book. The new footnote references are of particular value. The volume, comprehensive as it is, could not of itself survey the expensive field. It does, however, indicate fairly exhaustively the many lines of application which psychology has found.

The book is divided into two parts: (1) psychology applied to individual competence; and (2) psychology applied to occupational activities. Applied Psychology is defined as that "applied to the affairs of daily life." Its fundamental fact is asserted to be "that the individual is the unit of action." There follows a study of the individual and factors influencing him—heredity, age, sex, environment, and drugs. Special chapters are devoted to learning, thinking, suggestion and rest. Part II discusses the principles of vocational psychology, the rôle of judgment and methods of measurement, then proceeds to specific results from experiments in industrial psychology, business, law, medicine and education.

The point of view is dynamic. Among the general topics taken up are reflexes, instincts, racial differences, genius, mental defects, skill, practice, mental growth, the overlapping of traits attributed to sex differences, and phenomena of work and fatigue. The environmental conditions of ventilation, illumination, and distraction are stressed. The difficulty of obtaining control subjects in experiments on the effect of drugs, it is claimed, makes the validity of results doubtful.

An account of the search for objective measurements, with its rewards up to date, occupies much of Part II. Vocational selection and guidance, general and special capacity, character and interest, require specific tests. The maximum return for labor is of prime importance to industry, and the consumer's satisfaction is of great significance to business, while economy of effort and the increased efficiency of the human machine are common problems which must be solved by adjustment of conditions. Here we encounter difficulty in removing traditions and superstitions.

The final stage sought by the law is prevention of crime. To this end, the causes of crime, determination of guilt, limitations of testimony, and treatment of the offender are studied. Again, in medicine prevention is coming into prominence. Before diagnosis of abnormal behavior can be made, there should, the author contends, be a study of the normal. In treatment of disease as well, psychological factors are present. In this connection he suggests a course in experimental psychology, to be introduced into the curriculum of the medical schools. Only the main directions of application in education are indicated, as other publications cover this field.

To those who wish a text which shall form a satisfactory foundation for a survey of applied psychology, and to laymen interested in the outlook of application, not by 'quacks' but by sound investigators, the reviewer recommends this book. You will find the material well compiled and conveniently arranged, and the style lucid.

Cornell University FLORENCE WOOLSEY HAZZARD

The Effects of Music: A Series of Essays. Edited by MAX SCHOEN. New York, Harcourt, Brace & Co., 1927. Pp. ix, 273.

This volume consists of a series of twelve experimental studies of the effects of music. Eight of these studies are chosen from the papers submitted in a competition conducted by the American Psychological Association in 1921 for the most meritorious research on the subject. The remaining four papers are reports of investigations carried on in the psychological laboratory of the Carnegie Institute of Technology by Dr. Max Schoen and Dr. Esther L. Gatewood.

The prize-winning study, contributed by Professor M. F. Washburn and her assistants [see "A Correction" this JOURNAL, p. 675], deals with the effect of immediate repetition on the pleasantness or unpleasantness of various types of phonograph music. They find that repetition tends to decrease the pleasantness of popular music but to increase the pleasantness of seriously classical music. Among the problems which are dealt with in the other studies are types of musical listeners, the nature of musical enjoyment and its relation to specific emotional effects, the organic effects of music (as recorded objectively), and the mood effects of music.

The volume as a whole represents a significant contribution to the psychology of musical appreciation, but perhaps its greatest significance is methodological. The authors have shown by their results that such 'complex' experiences as the enjoyment of music may be successfully studied by the introspective method. The reports, for the most part, are reports of the effects of musical selections in their entirety, as played on the phonograph, rather than of the melodic fragments such as psychologists have usually worked with in the past. To be sure, none of the results are final. Each study raises more problems than it solves. But that in itself is no condemnation of the work done. Students who are interested in this field of psychology should find the book a valuable source of orientation for future investigations.

Cornell University E. FRANCES WELLS

A Study in Moral Theory. By JOHN LAIRD. New York, Macmillan Co., 1926. Pp. xviii, 322.

This volume is a subtly reasoned and well polished essay. Its general point of view may be expressed by the following quotation: "anyone who takes account of his action should also take account of his principles" (p. 9).

The preface notes that the argument may be divided into four parts: the first, analytical; the second, psychological; the third, sociological; and the last, philosophical.

The distinction which characterises moral enquiry is between reasons that are simply explanatory and reasons that justify or condemn. Moral theory deals not with the antecedent facts that have brought things about in a certain way, but with the problem ought they to be of this kind. Plainly the only consideration relevant to the justification or condemnation of an action is its value or lack of value—its goodness or badness. Moral theories usually deal either with the conception of value, or the conception of obligation. "Of these the former, by itself, seems insufficient; the latter, when justified by the former, to be the proper object of moral study" (p. 196), for although right conduct must be justified by its value, it is conduct with which we have to deal. The mere existence of good or beautiful things is no moral matter. Imperatives thus define a most essential field of moral study, and their justification and illustration is extended to some length.

Since "moral theory has to do with justifying, not with explanatory reasons" (p. 119), and since psychology is an explanatory science, it follows that psychology can never be the basis of ethics. But such notions as "moral obligation" are mere words unless they actually apply within a certain psychological field. The "admission of obligation on our own part seems to presuppose, first, that the authority of conscience depends upon the possession of a certain species of knowledge" (good and evil); and "secondly, that these beliefs concerning good and evil (while not infallible) are not inadequate to the requirements of our action in a given situation, in so far as moral consideration really are binding; and thirdly, that these beliefs must be capable of effective influence upon our conduct" (p. 201). The truth of these assumptions are vigorously defended.

The next chapter deals with collective action. The final chapter places ethics in a natualistic metaphysics; but insists that biological principles do not determine ethical ones, and that, "if our convictions are not shams, all our directions in terms of values must have an absolute, not a relative, basis" (p. 303).

University of Washington RALPH GUNDLACH

Vocabulaire technique et critique de la philosophie. By ANDRÉ LALANDE. Paris, Félix Alcan, 1926. Vol. I, 1926, pp. vii, 499; Vol. II, 1926, pp. 563.

This work has been in the making for many years, the general idea of it and the method to be followed in its construction having first been sketched by Lalande in an article published in 1898, and proposed to the International Congress of Philosophy in 1900. It is one of the publications of the Société Française de Philosophie, and has been published in parts and reviewed and criticized by the members and corresponding members of this society. Views and discussions by different members are given in footnotes with names attached. The terms defined and discussed in the text are followed by their foreign equivalents or approximate meanings (Greek, Latin, English, German, Italian). The terms considered are not only terms peculiar to logic, moral philosophy, esthetics, and metaphysics, but also terms used in psychology, sociology, and various other subjects related to the four members of the philosophical group named. In all cases, however, the treatment is from the philosophical point of view. As a consequence of this fact terms used in modern objective psychology are, of course, largely neglected. A supplement of 88 pages, added mainly by Delande, and therefore not carrying an indication of authorship of different items, gives additional terms and also supplements to the treatments of terms occurring in the main vocabulary. For the convenience of the reader the terms discussed elsewhere in these volumes are marked with an asterisk. In this supplement one finds, as new additions, such terms of interest to psychologists as aptitude, attitude, behaviorism—which, however, gets less than four lines—correlation, language, structure, variability, etc. Under "form" and "structure" the psychology of *Gestalt* gets short notice.

These two volumes are especially useful to psychologists working on the historical aspects of psychology.

J. P.

Measurement of Intelligence by Drawings. By Florence L. Goodenough. Yonkers, N. Y., World Book Co., 1926. Pp. 177.

This is a description of an experiment to ascertain the extent to which the nature of the drawings made by young children is determined by their intellectual endowment. Other investigations have suggested that concept development is a greater desideratum in effective drawing than specific esthetic or manual aptitude. It cannot be asserted as proved in the face of such contradictory evidence as that of Leta Hollingworth showing the paradoxical contrast in merit between drawings of certain children having high and low intelligence quotients.

Goodenough has, however, brought to light data from several thousand children for objectively differentiating between those cases of drawing ability due to level of intelligence and those due to other factors.

The child is given the task of drawing a picture of a man without model, copy, or suggestions. Test results are influenced by practice in drawing the human figure, but are relatively unaffected by the ordinary formal drawing instruction of the primary school grades.

Because of the scoring method, artistic qualities per se of the drawing become negligible factors and subjective judgment is eliminated. A few points showing increase in grades of difficulty on which the drawings are scored may serve to suggest the character of the complete scale of fifty-one units: head present, legs present, length of trunk greater than breadth, nose, mouth, arms and legs, shown in two dimensions, correct number of fingers, head in proportion, eye details, etc.

The validity and reliability of the test is approximately that of the other more commonly used group verbal primary level tests and higher than most non-verbal scales. Its particular value lies in its serviceability for testing mentality of deaf children and foreign children having language difficulties. The other major advantage is its brevity, requiring no more than ten minutes for group administration and two minutes for individual scoring. The probable error of estimate of an I.Q. is approximately 5.4 points at all ages from five to ten years.

The author's chapter on the psychological interpretation of drawings is consistent with and might even be regarded as excellent support of Piaget's thesis of concept quality inherent in the nature of the young child's mind.

Vassar College — Miriam C. Gould

La representation du monde chez l'enfant. By Jean Piaget. Paris, Félix Alcan, 1926. Pp. 424.

This is the third of a group of studies by Piaget dealing with the qualitative aspects of child thinking. Piaget is not concerned with measurement of the child's mentality, nor with charting growth, nor with eliminating errors. He is a zoölogist as well as a psychologist and approaches his material with the bent for minute classification and for discovery of structural rather than functional status of the child's mind. His evidence includes soliloquies, conversations between children, their mutual questions and explanations of cause, of motivation, and of justification.

The problems of children's ideas of reality and causality are treated as confusions: initial confusions of object with symbol or name, of percept with object, of percept with image, of attributes with things, of dreams with events, of being with participation. Added to these are the accidentally acquired confusions in the child's concepts of relationships between them.

A section on child animism discusses the immature concept of life as disclosed in the tendency to attribute consciousness to things. Factors predisposing to this mechanism and conditioning it are the process of systematization of ideas, the process of extension of vocabulary and of awareness of the universe.

The last section deals with children's early concepts of the origin of stars, water, trees, earth and the significance of this character of primitive thinking. Piaget does not offer any markedly original interpretation, nor does he ally himself with mystical or ulterior modes of analysis of these notions common to all children. From the author's point of view, the child world is not the fiction of unpredictable imagination, but a series of stages of explanation logically reached by selecting and assimilating new materials with old in accordance with laws inherent in the developing thought process, proceeding from dynamic substantialism to magic and from magic to static explanations. Quite independently of adult help or supervision the child assembles step by step the basis for the construction of his concepts appropriate to the stage of his mental structural development. The adult thought elicited by children's questions and demands, plays far less of a rôle in actually molding a child's ideas or determining their quality than has been the prevalent supposition. The adult's words cannot have for the child the adult meaning and it has long since become impossible for the adult to think in the manner of the child.

Vassar College MIRIAM C. GOULD

Economics and Human Behavior. By P. S. FLORENCE. New York, W. W. Norton & Co. Inc., 1927. Pp. 95.

The author attempts to determine to what extent the modern psychologists' criticism of the economists' over-rationalization of man is valid. After a statement and examination of the economists' standpoint and of the attack by the psychologists, he concludes that the economists' defense is inadequate.

He then proposes a restatement of economic theory in terms of "demand price" and "supply-price" instead of "utility" and "cost," to avoid questionable psychological implications. This theory is to be supplemented by an inductive, experimental attack on economic problems in place of deductions from any *a priori* view of human nature as the main source of economic knowledge. Offering different wages for various hours of given work and making observations of the laborer's response, is an example of the methods proposed for carrying out this program of reform.

The author's position that the psychologists make man a non-calculating creature driven by instinct, is somewhat extreme and erroneous. He does not appreciate fully the modern view of rational behavior. The disagreement among psychologists as to whether certain universal action-tendencies are instincts or whether they are instincts *plus* habit, is of less importance to economic theory than is maintained in this book. The author is also in error when he lists the emotions of fear, rage, and love as the only "instincts" observed by Watson.

George Peabody College C. W. TELFORD

Human Nature and Education. By A. S. WOODBURNE, New York, Oxford University Press, American Branch, 1926. Pp. ix, 292.

This volume is the outgrowth of a series of lectures apparently to actual or prospective teachers on "Modern Development in Educational Psychology." The traditional categories of general psychology are interestingly presented and numerous applications are made to the problems of teaching and learning. It is rather unusual for a book on educational psychology to give so much attention to philosophical interpretations. A discussion of the freedom of the will alone occupies several pages.

The jewel of consistency seems somewhat dimmed in places. We read, for example, that "there are no such things as a reflex, an instinct, or consciousness. These terms would be much better used only in the descriptive sense as adjectives qualifying the words 'act' or 'behavior' and should not be used as substantives." Yet only four pages later we find that under certain conditions "the hunger stimulus could be responded to by the food instinct without the intervention of consciousness." The latter statement is the more representative of the volume as a whole. An inaccuracy of terminology occurs where, in speaking of experiments on transfer of training, the author says: "one group, called the 'control group', is subjected to special training, whereas the other is subjected as much as possible to identical conditions minus the special training."

The chapters on imagination and examinations are especially good. One valuable feature of the book is found in the numerous glimpses it affords into the educational and social conditions in India.

Kansas State College J. C. PETERSON

The Escape from the Primitive. By HORACE CARNCROSS, New York, Charles Scribner's Sons, 1926. Pp. xiv, 348.

This book is of interest to the psychologist who wishes to recommend to his classes an authoritative, non-technical, psychoanalytic study of human behavior. *The Escape from the Primitive* refers to the escape from the primitive ways of thinking about the order and relation of events in the world, from the system of thought called magic.

The 'primitive' also means the Unconscious where, in the experiences of life, motives are deposited, incubate and mature, determining man's behavior. The psychoanalytic technique cuts into the mental anatomy of the unconscious, exposing false motivation and childish primitive thinking and demonstrating how these prevent the world from growing up and solving the problems of war and peace, of society and the individual, and of progress.

Boston Psychopathic Hospital S. C. ROTH

Common Sense and Its Cultivation. By HANBURY HANKIN. Introduction by C. S. Myers. New York, E. P. Dutton & Co., 1926. Pp. ix, 284.

This book is interesting chiefly to the general reader. It deals with psychological material in an anecdotal, unscientific manner. The author draws a broad distinction between two types of thought; one, intuition or common sense, the other, deliberative reasoning. Intuition has its basis in forgotten memories, which by virtue of being forgotten, are lodged in the subconscious. The fact that intuition is quicker than deliberative thought may, in the opinion of the author, be due partly to the more comprehensive way in which data stored in the 'subconscious mind' are classified and associated. Our modern education is of value to intuition only when it is able to be forgotten and function from the subconscious. A classical education is better in this sense for the business man because its formal nature makes it easy to forget; hence it trains in forgetting, while a scientific education encourages deliberation. The book is replete with incidents from the lives and especially the educational experiences of eminent persons, which incidents the author cites in support of this theory.

Peabody College KATHERINE VICKERY

Guidance for College Women. By MABELLE B. BLAKE. New York, D. Appleton & Co., 1926. Pp. 285.

The subtitle of this book—"A Survey and a Program for Personnel Work in Higher Education"—indicates the intended scope of the treatment, but it was obviously impossible to assemble in this volume more than a small sampling of methods and results of significant personnel projects. Miss Blake has, however, defined the general problem well, formulated principles and answered many objections.

One of her chapters deals cursorily with questions of college admission criteria, another with the relation of secondary school guidance to students' purposes and knowledge of college education.

Various administrative methods adopted for assisting students in early orientation in college, in selection of courses and in occupational choice are briefly indicated. There are also suggested the elements of guidance to be found in student activities which formulate principles of group responsibility and self-discipline.

Results of questionaries of students and alumnae regarding their experiences of need of college guidance, and the findings concerning use of rating scales and records, the functions of personnel directors, faculty advisors, and deans are summarized succinctly.

The opposing policies of personnel administration through centralization or distribution of labor are stated, though not subjected to thorough appraisal. In fact, the book is designed for those who are unfamiliar with the personnel movement in colleges. Its scanty critical consideration of procedures makes it of minor value as a manual or guide to those officers who are actually in the ranks.

Vassar College MIRIAM C. GOULD

The Realm of Mind: An Essay in Metaphysics. By FREDERICK J.E. WOODBRIDGE. New York, Columbia University Press, 1926. Pp. viii, 141.

There are three chapters to this little volume: (1) the realm of mind; (2) objective mind; and (3) many minds. By realm of mind is meant that domain of existence which we inhabit and in which we discover a logical structure. The objective mind is a system of the physical world in terms of which things may be treated as propositions or formulas. The third chapter takes up the individual. A man's mind is not any addition to his body but in fact what he is: he may be analyzed into mind and body along the lines of his activities. This, of course, does not involve mind and body as two distinct and substantial elements of his composition. The third chapter has but little relation to the first two. The book is unified only by its aim to keep the distinction between perceptions and thinking, and physical objects and events, and to put them all in the same 'realm.'

University of Washington RALPH GUNDLACH

Principles in Bio-Physics: The underlying Processes controlling Life Phenomena and Inner Evolution. By C. RICHTER. Harrisburg, Pa., Good Books Corporation, 1927. Pp. 86.

This book is made up of weird speculations expressed in an odd form of symbolization. The author postulates the electrical nature of life-energy and contends that cells which bear the same rate vibrate in resonance. To the psychologist the book has no value except perhaps as one more illustration of the futility of speculation which is inadequately grounded upon the natural sciences.

University of Illinois PAUL THOMAS YOUNG

Source Book for Social Psychology. By KIMBALL YOUNG. New York, Alfred A Knopf, 1927. Pp. xxvi, 844.

The aim of the present work is to present materials which are not easily available to the student and the general reader. A table of contents divides the book into six parts: (1) The general features of social behavior; (2) The psychological foundations of social behavior; (3) Personality and social behavior; (4) Social attitudes and the subjective environment; (5) Leadership and prestige in social behavior; and (6) Collective behavior. There are 27 chapters with a total of 229 sections of reading material. Every chapter begins with a statement from the author descriptive of the materials which follow, and at the close of each chapter there is a list of class assignments with a selected bibliography.

The materials more than cover the field of social psychology. To the reviewer the excerpts seem of rather unequal merit, some quotations having been made apparently to fill out the book. Several case studies from the collection of the author are printed for the first time. Most of the readings are sound, instructive, and interesting. The book is a useful source to place in the hands of students of psychology and sociology.

University of Illinois PAUL THOMAS YOUNG

Educational Psychology. By E. H. CAMERON. New York, The Century Co., 1927. Pp. xiv, 467.

This volume is intended for the more mature student of educational psychology. To the reviewer it seems, however, that the principal function the text can have is to serve the instructor as a suggestive outline of topics to be dealt with in a course. Numerous important questions are mentioned and disposed of in a short paragraph or two, so that it is difficult to see how even the mature student can get an adequate view of the problems involved.

The book does not systematically develop a point of view. It treats the usual topics of original nature, mental measurements, individual differences, nature and nurture, intelligence, kinds of learning, the psychology of common school subjects, and others. Experiments are frequently cited, and there is throughout a marked tendency to think of behavior in terms of 'connections,' although the author does on several occasions speak of learning in terms of patterns (though this word is not employed); but the significance of patterns and meaning in learning is not dealt with. Instead the reader is left with the view that learning is principally a matter of repetition, trial and error, and association.

Cornell University FRANK S. FREEMAN

The Psychology of Mental Disorders. By ABRAHAM MYERSON, New York, The Macmillan Co., 1927. Pp. 135.

An excellent book, written primarily for the layman. The author divides the mental diseases into two groups of major and minor psychoses, and discusses them from the standpoint of etiology, symptomatology, and clinical grouping. Heredity and crime in relation to mental disease and feeblemindedness are dealt with. The subject of eugenics is not neglected, and the author points out how this field needs scientific research more than legislation or publicity.

A program for mental hygiene is presented, emphasizing how prevention, to be of value, must begin with the child. The book is made especially interesting and valuable by the wide experience of the author, whose conclusions are based to a large extent on personal observation and research.

Boston Psychopathic Hospital O. S. ENGLISH

Social Psychology: A Text Book for Students of Economics. By R. H. THOULESS. London, University Tutorial Press, 1925. Pp. vii, 376.

"The present work is an attempt to cover the syllabus of the Psychology sub-section of Sociology in the B.Sc. (Economics) Examination of London University. . . . The book therefore ranges widely over the whole field of psychology, developing in greater detail those parts which seem to have the most important bearing on practical problems."

The first half of the book treats chiefly of instincts, following McDougall and the English tradition. The work is differentiated from the ordinary text by the inclusion of two chapters on the psychology of economic values the materials of which would be interesting to students of economics. The latter half of the book also contains chapters on the acquired elements in behavior, sensation and perception, imagery and thought, conflict and volition, the psychology of morality, individual psychology, the psychology of esthetics, and the psychology of scientific and of religious development.

The book is written from a reflective and critical point of view, and is based largely on English psychology with inadequate use of the American and German materials. As a whole the book is clearly written and substantial.

University of Illinois PAUL THOMAS YOUNG

Über Korrelation. By W. BETZ. Leipzig, Johann Ambrosius Barth, 1927. Pp. iv + 65. *Zeitschrift für angewandte Psychologie,* Beiheft 3 (second edition).

This is a revised edition of an earlier work under the same title, which was published in 1911. The only text of its kind in German, it is similar in content to the chapters on correlation in the numerous statistics textbooks in English. It contains, however, some things not ordinarily included in these textbooks, as, for example, a critical discussion of Deuchler's method of rank order correlation and a brief and easily understood statement of Spearman's early work on a "general factor." The statistical subjects presented are product moment correlation methods, the eta coefficient, probable error, rank-order methods of correlation, four-fold tables, contingency, multiple correlation, spurious correlation, and the Spearman work on a "general factor" and the "hierarchy."

University of Chicago THELMA GWINN THURSTONE

Investigations in the Hygiene of Reading. By J. H. BLACKHURST Baltimore, Warwick & York, 1927. Pp. 63.

Chapter I discusses the history of experiments upon reading and also presents data which show changes during recent decades in the height of letters, length of line, and leading of school books. Chapter II describes a method for investigating the most readable size of type, leading, length of line, and marginal identation for children of the first four grades. Chapter III presents in tabular form experimental data upon the problems indicated above. A record was made of the time of reading and of the errors in reading for school children. In conclusion specific recommendations are made regarding the typography of books for use in the first four grades.

University of Illinois PAUL THOMAS YOUNG

History of Ancient and Medieval Philosophy. By H. W. DRESSER. New York, Thomas W. Crowell Co., 1926. Pp. xii, 338.

A practical textbook in philosophy, reviewing the developments from early Greek times to the seventh century.

J. P.

History of Modern Philosophy. By H. W. Dresser. New York, Thomas W. Crowell Co., 1928. Pp. xvi, 471.

This volume completes the sketch of philosophy begun in the first volume. It devotes a number of pages to philosophy in America, to which certain psychologists (*e. g.* James and Baldwin) have contributed. Of modern writers in Europe and England, Bergson, Croce, Bradley, Bosanquet, Ward, and others receive attention, Croce getting 15 pages.

J. P.

Psychology and Its Use. By E. D. Martin. Chicago, American Library Association, 1926. Pp. 47.

This is a popular discussion of some uses and abuses of psychology. The title is rather optimistic for a pamphlet of its length.

Kansas State College J. C. Peterson

Fear: The Autobiography of James Edwards. By J. R. Oliver. New York, The Macmillan Co., 1927. Pp. viii, 366.

This book contributes nothing to our knowledge of fear. It is not a scientific treatise, but merely a novel—which the reviewer found to be rather stupid—written in the form of an autobiography, giving the story of the struggles and fears of a typical and common case of psychasthenia. The author is very religious, is embued with an idea concerning the efficacy of prayer, and writes his book with the avowed purpose of showing "exactly where the work of the psychiatrist must stop, and where the work of the priest or the evangelical minister must begin."

K. M. D.

German-English Dictionary of Psychological Terms. By C. A. Ruckmick. Iowa City, Athens Press, 1928. Pp. 45.

This dictionery gives the English equivalents of over 1200 German words. It will be helpful to the student acquiring a reading knowledge of German psychology. The author has drawn heavily upon the glossaries of Titchener and Judd, and his translations with few exceptions are those generally accepted by psychologists. The addition of a glossary of abbreviations commonly found in the German psychological literature is welcome. The explanation of these abbreviations is nowhere so readily available; the standard German-English dictionaries do not give them, they are not explained in the literature, consequently the student is often at a loss to know where to turn—as indeed must also be true of foreign students over against the abbreviations used in English and American psychology. The author would have done well in this part of his dictionary to have given the German as well as the English equivalents of the abbreviations.

K. M. D.

The Mind and its Mechanism. By P. Bousfield and W. R. Bousfield. New York, E. P. Dutton and Co., 1927. Pp. 224.

Several interesting chapters on the functioning of the "psychic brain." It is not easy to see wherein the author has made Freud's conception of the censor any clearer by postulating in its place an anti-psychogram to replace an unpleasant psychogram. However, in a field so transcendental new conceptions are always welcome; so this is not a severe criticism. Favorable comment is due the authors on their summary of each chapter.

Boston Psychopathic Hospital O. S. English

NOTES AND DISCUSSIONS

'HABIT' IN 'APPRECIATION'

Education is concerned with the direction of three types of activities, or better, perhaps, three aspects of activity, in human beings: (1) activities of overt performance—such as speaking and moving; (2) activities that condition or eventuate in overt performance—such as judging and reasoning; and (3) activities that end, or may seem to end, in themselves—such as feeling, insight, or, in general, awareness or consciousness.

So far as education is designed to adjustment of the individual with the physical and social environment in which he lives and of which he is a part, the first two types are significant. Man is, from the point of view of adjustment, an organism in nature and a member of society. As such he *is* what he *does*. His every activity, then, is subject, for valuation, to criteria of efficiency in adjustment. Values subsist in the pragmatic sanction. Education is concerned with conduct, moral, civic, vocational, and the other categories so familiar to us; that is, the ends of education lie in the achievement of organic and social well-doing. Education becomes education for 'efficiency.'

So far, however, as education is designed to the "enrichment of the inner life," whether that be an aspect of adjustment or not, the third type comes to the fore. Man is, from this 'humanistic' standpoint, a self. He *is* in his *awareness*, as well as in his physiological dynamics. Criteria for valuation become subjective and individual, rather than objective and social. Values are inherent, not derivative. The ends of education are in well-being or 'appreciation.'

Experimentation in the functioning of physiological mechanism seems to have furnished a key to method in control of organic reaction. The 'law of use' in formation of habit, or the 'conditioned reflex,' seems to work. Education for adjustment comes into its own, and is on the way to become a 'science' of education. Reactions of overt performance are demonstrably governable by application of the law to educative procedure. We seem to have a method of education for 'efficiency.' Have we in that also a method for education in 'appreciation'?

I. According to the view of very 'advanced scientists' in the field of human behavior, the question is wearily useless. Under given conditions, if a man is conscious, he is conscious as he is conscious; that is all there is to it. Since neither he nor anyone else can observe and describe how he is conscious, scientists need not concern themselves with that. No real loss is involved, since it matters not a whit to the organism or to the world what awareness exists or does not exist in a man under any conditions. There may be 'appreciative' process in one who behaves, but since we can neither know what that is nor what brings it about, we had best cease worrying and stick to our knitting. As we direct successfully the sequences of subvocal response that constitute the intermediary processes of judging and reasoning we are affecting the only knowable insight; as we direct successfully the secretion of the adrenals, the thyroid, *et al.*, which condition capillary contraction and expansion, rate of heart beat, miscellaneous heavings of the entrails, etc., we have, in so far forth, taken charge of the knowable emotions and feelings. For both scientific and practical reasons it is wise to confine ourselves to education for 'efficiency,' and to dismiss the mystical function of education for 'appreciation.'

Under such a view criticism of the 'law of use' as a means to development of activities of the 'inner man' is, of course, without point and futile.

II. A second view admits more of speculation into the behavioristic hypothesis. It suggests the admission of consciousness into the category of behavior. It extends the principle of the conservation of energy to include the supposition that consciousness is a form or manifestation of energy. A feeling, idea, or process of awareness, then, is interpreted as itself a response to some stimulus of glandular or muscular activity; that is, a response in what we call consciousness to a response in the objective physiological organism. Except for being one step removed it stands in the same relation to initial stimulus of a 'chain' as does a movement of the jaw or secretion of saliva. If that is so, however, we should expect the 'law of use' to operate to the establishment of habits of 'appreciation,' just as it does to the establishment of habits of 'efficiency.' Does it so operate?

Among students of human behavior we find agreement upon one law of habit formation—the 'law of use.' The use of a 'conduction unit'—neurone, reflex arc, line of force—for the conduct of 'impulse,' electrical or other, between a receptor mechanism and a response mechanism, affects the readiness of that conduction unit to receive, to conduct, or to discharge in response, when the 'same stimulus' occurs again. Thus a 'habit' of responding in a particular fashion or not responding in that particular fashion may be established. The more frequently the connection is effected the more it is affected to facilitate speed in connection and accuracy in response. Predictability of response from stimulus grows in certainty. That is the 'law of use' in habit formation.

But does this law hold for 'connections' leading to awareness? Experience, not experiment, seems to point to a negative answer. Nothing seems more certain, from subjective evidence—the only kind of evidence that is or can be admissible—than that with 'perfecting' of a habit of bodily response my consciousness thereof or therefrom diminishes or changes in kind. As I perfect my skill in operation of the typewriter, for example, I become less and less aware of keys, of shift, of bell, of touch and movement. In the degree that organism, through habit, takes over the operation, I, as inner man, less and less 'identify myself' with the operation or share consciously in it. Again, I solve a problem in algebra through a series of subvocal mechanistic responses that seem to involve rather acute awareness of certain ideas. But if I repeat my reaction to the same symbolic stimuli those ideas do not recur with increasing certainty and clarity in consciousness. Quite the opposite. Sooner or later the stimuli cease to 'present' a problem in any sense. I react to them with overt correctness and more certainty and speed, but I am less conscious of the ideas that seemed at first to belong to the solution. Habit, again, takes over the problem for me. I, the man who is aware, have less and less to do with it. The 'law of use,' then, accounts for my 'efficiency' in operating the typewriter and in 'solving' the algebraic problem, but it does not account for the difference in my 'appreciation' of the operation and the solving.

Now, whether I speak of the change in 'appreciation' as quantitative—which is, perhaps, not improper in discussing a mechanistic hypothesis—or qualitative, the conclusion I must come to is the same. The 'law of use,' as now formulated and understood, appears not to govern uniformly the conditioning of activities of consciousness as it does appear to govern habits of muscular and glandular response in the organism. There is the suggestion, of course, that use which strengthens the bonds between stimulus and organic response may weaken, in some cases at least, the assumed bonds between organic response acting as stimulus, and awareness as assumed response.

The criticism, then, points to either of two possibilities: (a) the hypothesis that consciousness can be placed in the category of behavior is unsound; or (b) the 'law of use' represents but a special instance of some more inclusive principle of behavior yet to be discovered. In either case the attempt in educational practice to develop particular appreciations by appreciation of particulars does not find its sanction here.

III. A third view, or rather composite of hypotheses, alike as they bear upon the 'law of use' in education for 'appreciation,' may be stated thus: Consciousness is not response or reaction end. Yet it is related to function in organic mechanism. Consciousness 'arises' as the channels of 'connection' are obstructed. The relation may be causal—which necessitates assumption of a factor of identity in cause and effect; so there may be transformation of the energy of 'impulse' into the energy of consciousness, similar to the conversion of electric current into heat, or light, or motion. Or the relation may be that of incidental but assured concomitance, in which consciousness does not share in the properties of mechanic 'impulse,' but may 'arise' therewith, nevertheless. For discussion here it makes no difference whether awareness is effect or accompaniment or something more mysterious, so long as we grant that it 'arises' with obstruction of the channels of reaction.

To illustrate before proceeding further: Assuming that the neurone is the unit of conduction, consciousness 'arises' with resistance to 'impulse' at the synapse or in the axone. This resistance creates a condition corresponding to that of a dam or partial dam in a stream; against it 'impulse' urges as does water against the dam. In whatever happens, say, an overflow or spreading out, 'impulse' takes on a new aspect which *is* consciousness, just as the overflow of water takes on the aspect of a lake. Or the spreading 'impulse' may *result* in consciousness, just as the overflow of water results in change of color in the soil upon which it flows. Or, again, outspreading 'impulse' is *accompanied by* consciousness, as the overflow of water may be accompanied by frogs, sandpipers, herons, and mosquitoes. The analogies are, of course, crude, but may be, nevertheless, suggestive of concepts of relation between 'physiological process' and 'psychological process' which have wide influence.

Once more, to be sure, the 'perfection' of habit in response at the arc end is, under the 'law of use,' fatal to the conscious elements. Use removes the obstruction, or cuts away the dam. Consciousness sinks as it arose.

There is, however, this comfort for him who would, through habit, enrich the 'inner life.' Before a channel can be obstructed there must be something in the nature of a channel. That is, education for 'appreciation' is still concerned to make 'connections.' To do so involves the 'law of use.' But it is also concerned with the blocking of channels. The implication seems to be that education for 'appreciation' should seek to initiate rather than to 'perfect' habits; it must develop many and branching and relatively shallow channels of organic reaction; not one assured response to one given stimulus, but varied potential responses to that stimulus; not one response to one stimulus, but one response potential to varied stimuli. 'Impulse' must flow as through a "shorn and parcelled Oxus" rather than as through a millrace or a barge canal.

To put the matter in quite different terms, this third view implies—and experience seems to bear out the implication—that consciousness 'arises' in situations involving both the familiar and the novel. To the completely familiar we respond with promptness and accuracy, but automatically and without awareness; to an utterly novel 'situation' we do not respond at all; we have no equipment therefor. In situations between these extremes awareness comes. We respond neither by blind habitual reaction nor by blank inaction, but with 'appreciation' of one sort or another. Habit, or instinct, furnishes the 'familiar' elements in such situations; environment the novel elements. Education for 'appreciation,' then, must work in the middle ground, seeking to open avenues of initial habit that may function partially in introduction to many predictable *kinds* of situations, rather than completely in certain particular and specifically predictable situations.

It appears, then that direct education in and for 'appreciation' is out of the question under a behavioristic hypothesis. But under the more speculative hypothesis of behavior, indirect education for 'appreciation' is possible. Educational practice guided by the 'law of use' to ends far short of fixed arcs of reaction, may contribute to 'enrichment of the inner life.'

Cornell University THEODORE H. EATON

THE GEORGIA EXPERIMENT

Psychology at the University of Georgia is ranked among the natural sciences. It is included in the science group together with physics, chemistry, and biology. This has been brought about by the fact that all the undergraduate courses in psychology are taught in conjunction with the laboratory. All the courses, of which there are three, run three quarters. Four hours of university credit, equivalent to eight hours semester credit, is given for every course each year. In the elementary course three 1-hr. periods a week are given to the class-room—to lectures, discussions, and examinations—and one 2 to 2 1/2-hr. period a week is given to the laboratory. The advanced undergraduate courses have three 2-hr. laboratory periods and one conference a week.

The class in the elementary course is divided for the laboratory work into sections of approximately 20 students each. The limit for a section is placed at this number so as to lighten the burden upon the instructor, and also to reduce the amount of duplicate apparatus needed.

Two special aims underly the course: (1) the student is required to do his experimental work as nearly as possible under conditions of research; and (2) he is thrown so far as possible upon his own resources and is required to do all that is possible by himself in place of merely following instructions and verifying statements.

The accomplishment of these aims is attempted with the help of a manual written especially for the purpose, and the procedure in the course involves certain essential features which include the following: (a) discussion at the beginning of the year of scientific method with special emphasis upon sources of error and reference to books which are to be studied in this connection. (b) Study and discussion with the instructor of two or three sample reports well done by former students which are constantly used for reference in writing up reports. (c) Instructions and an outline for writing up experiments as follows: statement of the problem; method, materials, and apparatus; data or results of experiment; sources of error; conclusions; theoretical explanation; answers to questions. (d) The use of 'originals' when students have progressed sufficiently.

The discussion of scientific method is for the purpose of helping the student to a scientific attitude and to insight into the care with which he should work. The selected reports of former students set a standard for the writing up of reports and help the students to understand what is required. The instructions to students including the outline for writing up the experiment help them to a systematic organization of their work and to a clearness of thinking. The most difficult part of this work is to get the student to keep his conclusions clear of his statement of results of experimentation. Conclusions are accepted only if they agree with the experimental data whether or not they agree with the textbook statements. Any variance from textbook statements is to be indicated. Theoretical explanations are to be quoted from books which are in the laboratory, such as those of Ladd and Woodworth, Pillsbury, Dunlap, Titchener. All graphs, tables, legends, etc., are to be done carefully and as if the report were to go to the printer.

Thirty experiments are the minimum for the course although more than thirty may be done. As given at the present time the thirty experiments are selected from the fifty-five indicated below. The first thirty-eight are regular experiments, the remainder are originals.

Measurement of error in Müller-Lyer and size-weight illusions;
Drawing of brain and nervous system;
Learning in finger maze;
Mirror drawing with star;
Auditory acuity;
Description of visual experiences with brightnesses and colors, including description of course of after-image;
Retinal sensitivity: color zones and blind spot;
Study of visual contrasts and the vision octahedron;
Drawing of the eye and its parts;
Mapping of cold and warm spots;
Mapping of touch spots;
Perception of illusion of movement and drawings of the ear;
Span of attention;
Stereoscopic vision: perception of depth, retinal rivalry;
Auditory perception of direction of sound;
Two point limen;
Imagery: types of imagery, vividness of imagery;
Visual illusions;
Associative connection;
'Free' or 'uncontrolled' association;
Tapping associations;
Psychology of report;
Memory span;
The conditions of interest;
Judgment of the duration of time;
Judgment of distance;
Judgment of emotion from photographs;
Reaction times;
The psychogalvanic reflex;
Judgment of intelligence from photographs;
Ability to follow directions;
Study of an intelligence test;
Statistical study of results of intelligence test;
Study of another test, as an achievement test;
Study of correlation;
Study of another test, as ethical discrimination or will-temperament;
Information test;
Vocabulary test.

The originals include the following:

Indirect vision;
Apparent size of objects that stimulate the skin at different places;
Binocular and monocular vision;
Modification of a reflex;
Unconscious action;
Set or disposition;
Methods of memorizing;
Meaning and retention;
Individual differences;
Correlation *vs.* compensation;

Affective value of colors;
Taste perceptions;
Judgment of human traits;
Improvement with and without knowledge of successes and failures;
Division of time in learning;
Learning and definiteness of practice.

Provision is also made for one long experiment as in learning. Few students, however, are permitted to take a long experiment. The writer has tried giving beginning students long experiments and has come to the conclusion that it is better to give one experiment a week for the following reasons: interest is better maintained; the one long experiment (although the student learns more in a limited field) may incline him to think that he does not like psychology, whereas, with a number of experiments, interest is always developed in connection with some of them; a short series of experiments can be correlated much better with the lectures and discussions and there is better understanding of all subjects discussed after the laboratory work has been done in connection with them.

The selection of experiments has been made with the following points in mind: practicability for a 2 to 2½-hr. period, expense of duplicate apparatus, correlation with lectures and discussions, amount of psychology that students might learn, introduction of student to various types of experiments and tests.

One of the features of the course is the giving of a minimum of instructions for experimentation. This is for the purpose of requiring the maximum of understanding and work on the part of the student. The preliminary talk and verbal instructions with demonstration by the instructor at the beginning of a laboratory period supply for any given experiment what the instructor judges that the students need in addition to what is in the manual. At the first of the year more instructions are given and ter they are minimised. The student cannot blindly follow instructions; he is forced to know what he is doing and to formulate for himself his problem and method. The originals are used only after the student has progressed sufficiently in the judgment of the instructor and he is then helped as little as possible. All work must be fininshed and reports handed in before the students leave the laboratory.

The advantage of this course may be summarized as follows. (1) Demanding work to be done as nearly as possible as it is to be done in advanced courses and in research work. (2) Requiring the statement of the problem in the student's own words and his own statement of methods. (3) Demanding effort at solution of some problem instead of following detailed instructions point by point. (4) Demanding that the student draw conclusions from the facts obtained by experiment. He must be true to his facts. He must learn to go where the facts take him. Here is training in one of the most fundamental traits of scientific work. There is no science without facts; neither is there any science without logic; from one point of view, science is facts plus logic. In this course the facts must be obtained under experimental conditions; the conclusions must follow with logical necessity from the facts. (5) Demanding attention to the sources of error in the experiment. (6) The use of originals which demand and develop ability to apply what has been learned of scientific method. (7) Complete and thoroughly written reports of each experiment as nearly as possible as it would be done for a journal article—graphs, legends, figure numbers, etc., must be included and done as they are done in an acceptable reference book or a psychological journal. (8) Students are forced to study the best texts for theoretical explanations and thus gain familiarity with them.

The course may, on the other hand, be discouraging to the beginning student unless the instructor encourages him and helps him to understand the value of such work. Another possible disadvantage may appear in the length of the written reports. This is not wholly bad though it is sometimes wearisome to the student.

Comparing the results of about 100 students a year for three years with students of earlier years, there appears to be four points of superiority in the results of the present plan: (1) greater ability to recognize and to state a problem definitely; (2) to draw conclusions from data; (3) to keep conclusions properly separated from the results of experimentation; and (4) greater familiarity with texts used in the laboratory. Graduate students show the effects of this training as compared with students who have not had it. A number of former students have volunteered the statement that they can apply the methods learned in this course to the solving of problems in other situations.

The classicists have challenged the ability of science to give adequate training to our youth. Experimenting is the highest type of learning as it includes all other kinds. Through experiment and the improvement of our experimental courses science may demonstrate the possibility of making the best contribution to the student's education.

University of Georgia A. S. EDWARDS

FERTILITY AND PARENTAL INTELLIGENCE

It has been pointed out that the principal determinations, such as those of Lentz and of Sutherland and Thompson, of the correlation between fertility and intelligence have considered only *offspring* intelligence; this has been due, of course, to the fact that it is ordinarily only children who are available for testing. Though it seems *a priori* unlikely that the values of these correlations would be greatly altered by using parental intelligence, the complete analysis of the problem certainly requires their experimental determination.

Opportunity may be made for a crude approximation to the desired figures by reëxamining the writer's data on about 100 California families, collected in 1924 for the purpose of determining the values of the familial coefficients of resemblance.[1] These data share with those of most similar studies the defect that it is impossible to take into account from them the childless portion of the population; on the problem of incomplete families they are perhaps somewhat better than most, since the criterion of the inclusion of a thirteen-year-old in each family brought the average age of mothers to the vicinity of 42 or 43. The nature of the test used (paper-and-pencil variety) tended to lower the frequency of record both of illiterate (*i.e.* fecund) families and younger children; the latter error has been corrected in the present calculations wherever possible. More serious is the impossibility, for personal reasons, of deriving the usual child intelligence-fertility correlations from the same data for purposes of comparison.

It was found possible to combine the scores for the 11 tests of the battery (6 verbal, 5 non-verbal tests) in to a single score, which was not attempted in the original monograph. This yields what must be regarded as a fairly adequate measure of general ability of the intelligence-test variety. The procedure in this was not strictly orthodox; it was made possible by the fact that the age curves for both means and standard deviations of all tests descend at a negligible angle through the range populated by practically all the parents. It consisted in multiplying or dividing the raw score on each test by such an integer as was necessary to bring all the age curves above-mentioned to substantial congruence, and then adding the 11 results (or, in the case of N tests—$4 < N < 11$—adding the N results and multiplying by 11/N). Since by hypothesis all the standard deviation curves of the converted scores approximately coincide, this amounts to weighting the 11 tests equally, which seems to involve no unreasonable assumptions. These combined scores constitute one variable in the correlations obtained; the other is the number of children for whom record was secured, corrected in some cases in which it was known that there were other children. The few cases in which the families were probably incomplete were included, after it was ascertained that that their inclusion resulted in no appreciable effect on the correlations.

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[1] R. R. Willoughby, Family similarities in mental-test abilities, *Genet. Psychol. Monog.*, 2, 1927, 235-277.

The Pearson correlation coefficient between mother's intelligence and number of children was −0.11 ±0.06; that between father's intelligence and number of children was +0.06 ±0.07. The former is based on 108 cases and the latter on 87. The sources of error mentioned above make it unfruitful to generalize very extensively from this result; but it is noteworthy that even with fairly liberal allowances for error the figures would still be surprisingly near zero. Keeping in mind the augmenting effect of assortative mating recently shown to follow from multiple-factor assumptions,[1] it appears increasingly probable that the assortative mating coefficient (0.40 to 0.60) and the fertility-intelligence coefficient (−0.10 to −0.20) form a balanced system, the effect of which is to keep the population intelligence mean approximately constant, or at least subject to determination by local accidents or by other factors. There seems to be, as a matter of fact, no adequate evidence for any historical change either in the degree of the value of the fertility-intelligence correlation, assortative mating, or the level of the population mean.

Of passing interest are the constants of these distributions: The mean number of children for both fathers and mothers is 2.42, and the standard deviation is 1.17; the coincidence in the case of both figures is of interest as showing that it is not a fertility factor which eliminates the fathers (many of whom are deceased). The average intelligence score of the mothers was 100.2, and that of the fathers 108.0; the difference of 7.8 has a standard error of 4.8, corresponding to odds of about 15 to 1 in favor of a real difference. The standard deviations of the parental intelligence scores were 32.3 for the mothers and 35.1 for the fathers; the difference of 2.8 has a standard error of 3.5, and so is only apparent. The difference between the parental fertility-intelligence coefficients (0.17) has a standard error of 0.14, corresponding to odds of about 8 to 1 in favor of a real difference. The suggestion implicit in this last finding, viz., that the effective determinating factor of the negative fertility-intelligence correlation itself may be the superior ability of intelligent women to anticipate in imagination the trials of motherhood, has not, to the writer's knowledge, been advanced before, and seems a promising field for further investigation.

Clark University RAYMOND R. WILLOUGHBY

THE THIRD GERMAN MEDICAL CONGRESS FOR PSYCHOTHERAPY

The third medical congress for psychotherapy, which was held this spring in Baden-Baden, had like its predecessors two principal aims: first, to give the general medical practitioner some insight into the problems of psychiatry; and secondly, to clear the way for mutual understanding between conflicting schools of psychotherapists.

This time the two schools struggling for supremacy were the individual psychology of Alfred Adler and Freud's theory of psychoanalysis. Individual psychology was represented by the reports of Seif (Munich), Wexberg (Vienna), and Künkel (Berlin). Their psychoanalytical opponents were Schultz-Hencke (Berlin) and Schilder (Vienna).

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[1] R. R. Willoughby and Goodrie, Neglected factors in the differential birth rate problem, *Ped. Sem.*, 34, 1927, 373-393.

Seif gave an outline of the point of view of individual psychology with regard to the problem of neurosis. According to him the neurosis is a disturbance of interhuman relations, an excuse and an effort to justify the discouraged, egocentric individual in escaping from his lifework. Accordingly, the cure consists in educating the discouraged and dependent individual to become a courageous and independent one, by converting his egocentric attitude into an appropriate one which makes him fit for life.

Wexberg reported on organic inferiority in connection with certain results of internal medicine. According to his view Adler's doctrine of organic inferiority represents an extension of the principle of the point of minor resistance to the sphere of clinical pathology. Whilst the doctrine of constitution assumes partly the conception of a specific disposition for certain diseases, partly that of a constitutional debility of the whole organism, Adler asserts a predisposition of certain organs and organic systems to diseases of different kinds. There are families for instance, with a disposition to debility of the alimentary tract, which manifests itself in one case by an *ulcus ventriculi*, in another by a stomach carcinoma, in a third case eventually by a dyspepsia. Wexberg thinks that this connection is nowadays often accepted in constitutional research as well as in internal medicine. Less attention is paid to another section of Alder's doctrine, the compensation of organic inferiority. This supercompensation changes an inferior organ not into a normal one, but into an 'emphasized' one. The psychologically important consequence of the compensation of an organic inferiority is that eventually the entire organism seems to be grouped around the emphasized organ as its center.

Schultz-Hencke, one of the psychoanalytic speakers, asserted that all theses of individual psychology are implicitly contained in psychoanalysis, while the reverse is not true. Individual psychology merely inquires whither the symptoms are tending, whereas psychoanalysis also searches for causes. He does not deny that individual psychology is giving satisfactory service in the range of what psychoanalysis calls "the secondary profit of disease," that is, demonstrating the actual psychical situation which favors the fixation of symptoms. But its range is far narrower, considering the enormous extension of the field, than that of psychoanalysis. Schilder analyzed Wexberg's report and gave a fundamental criticism of Adler's doctrine of organic inferiority. He pointed out that Adler's doctrine is, using a term of modern Gestalt-psychology, typical 'piece-work' (*stückhaft*). The inferior organ is not isolated in the organism. Temperament, strength of the affects, and of the intellect of the entire personality, determine how it shall manifest itself. The problem of constitution goes far too deep to be capable of solution by the formula of organic inferiority.

The second subject of discussion was the problem of character. What is character; how is it to be conceived? Häberlin (Bale), Allers (Vienna), and Klages (Zürich) tried to investigate these intricate problems.

Häberlin pointed out the tasks confronting scientific characterology, and the means by which it can solve them. The principal task of characterology is to formulate a psychologically and logically ordered system of the points in which two individual characters may differ. Characterological differences must follow the connection of actions. This connection is described first by its form, secondly by its content. Formal characterological features are, for instance, the constancy of character, its differentiation, the course of action, intelligence, memory, etc. The content is the interest that manifests itself in the individual's actions.

Allers too asserted that understanding of character must be based on analysis of its actions. Every action tends towards a definite result which always consists in establishing an entity. Further, the action is always the releasing of a tension out of which it has arisen, and which it neutralises. According to Allers character cannot be understood from the structure of impulses, neither does he admit that it is determined by innate constitutional factors. He defines character as the formal principle of the total of all reactions of a human being. Since action tends towards values, character may be defined as the substance of individual laws of value-preference.

The third subject of the medical congress of psychotherapy was the relations between experimental psychology and psychotherapy. Lewin (Berlin) pointed out in an extremely interesting paper that experimental psychology has now become capable of treating successfully problems of emotional and impulsive life, of essentials importance in therapy. The experiment need not aim at imitating definite situations in real life; it is possible experimentally to construct the dynamical equivalent of such situations. It is even possible within the scope of laboratory experiments to arrange situations where the person tested is not aware of being part of the experiment; experiments exempt from task. Lewin described experiments aiming to explain tension and detension of need-systems, the degree of communication between different needs and psychical spheres, the question of repression, the different kinds of surrogate gratifications; experiments which have already produced enlightening results.

A series of lectures completed the program. Keen discussion after the lectures proved the active interest taken in these questions by the German medical world.

University of Vienna GEORGE GERÖ

DO AMERICAN PSYCHOLOGISTS READ EUROPEAN PSYCHOLOGY?

In 1925 the Research Information Service of the National Research Council issued, for 114 American libraries, a union check list for 194 foreign serials. These serials are the most important for psychologists in the sense that they are those listed in the *Psychological Index* for 1922. The list of 114 libraries consists of 31 public libraries (including the libraries of academies, societies, institutes and museums, not connected with universities) and 83 university or college libraries (including university museums and medical schools).

For the purposes of this note I have selected the nine psychological journals that I should suppose to be most generally read by general and experimental psychologists in America, and I have added one Italian journal for interest, although most Americans do not read Italian. Here follows the frequency of the occurrence of these journals in the 114 libraries:

Mind	82
British Journal of Psychology	69
Brain	56
Archiv für die gesamte Psychologie	53
Zeitschrift für Psychologie	50
L'année psychologique	38
Archives de psychologie	37
Zeitschrift für Sinnesphysiologie	35
Psychologische Forschung	15
Rivista di psicologia	11

The three British journals head the list. English is favored although German psychology has long led British psychology. It is quite proper, however, that German serials should be more readily available than French;

nor should it be surprising to find the Italian journal last, though the psychologist who is forced to use it (having come to it perhaps by way of the *Psychological A'stracts*) does not find himself very well served.

It is also apparent that the older journals have the advantage. In 1925 everybody in America had for a few years been discussing *Gestaltpsychologie*, and yet the *Psychologische Forschung*, then in vol. 6, was available in only 3 universities in New England, 4 in the Middle States, 5 in the Middle West, one in the far West, and in two libraries of scientific societies. Could a recent graduate student, who had been placed anywhere but in one of these 13 universities, be expected to know anythng much at first hand about this all-important new movement in German psychology and its experimental work?

It is safe to say that very, very few American psychologists, young or old, subscribe personally to these foreign journals. Not only are they too expensive for the younger men; but statistics show that very few members of the American Psychological Association, which includes nearly all of the psychologists we are discussing, subscribe for any of the Psychological Review Publications or for this JOURNAL. Psychologists who do not subscribe at home are unlikely to subscribe abroad.

What then do my data mean? They mean that a psychologist placed at one of these 82 academic institutions (and the list includes nearly all the large universities and is representative of the colleges) has an excellent chance of finding in the library *Mind*, which he will seldom want, a good chance for the other British journals, less than an even chance for any German journal, still less for the French, and only about a 15% chance for a new, important German journal in its fifth year.

I am not saying that American psychologists would (or could) ordinarily read German if they had it to read. They might read it more if they had it. They might have it more if they demanded it. I am saying that they probably do not read it very much. No wonder *Psychological Abstracts* has to translate into English. A psychologist asked me the other day why this JOURNAL does not print more foreign articles and print them without translation. Here is the answer. Here too, I should say, is the reason for scientific provincialism, not only, I think, in America, but also in Britain and Germany. Perhaps there is nothing to do about it, unless the fault and the remedy lie in our graduate schools.

Harvard University E. G. B.

A CORRECTION

In his *Introduction to the Effects of Music*, a volume of studies by various authors, edited by the undersigned, Dr. W. V. Bingham, in referring to the Edison Competition, states on page 5: "Judged on such a basis, the choice rested on the admirably crisp and clear-cut report of experiments by Professor Margaret Floy Washburn and her student assistants at Vassar College. This prize-winning study on 'The Effects of Repetition' is found on pages 199 to 210." This statement is incorrect. The prize-winning study is the joint work of Dr. Washburn and Professor Dickinson, on "The Sources and Nature of the Affective Reaction to Instrumental Music," constituting Chapter VI of the book.

Carnegie Institute of Technology MAX SCHOEN

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By J. P. GUILFORD, University of Nebraska

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